PRINCIPLES OF THE THEORY OF HEAT

VIENNA CIRCLE COLLECTION

VOLUME 17

VOLUME EDITOR: BRIAN McGUINNESS

ERNST MACH

PRINCIPLES OF THE THEORY OF HEAT

Historically and Critically Elucidated

With an Introduction by

MARTIN J. KLEIN

Edited by

BRIAN McGUINNESS

D. REIDEL PUBLISHING COMPANY

A MEMBER OF THE KLUWER ACADEMIC PUBLISHERS GROUP

DORDRECHT / BOSTON / LANCASTER / TOKYO

Library of Congress Cataloging-in-Publication Data

CIP

Mach, Ernst, 1838–1916.
Principles of the theory of heat.

(Vienna circle collection ; v. 17)
Translation of: Die Principien der Wärmelehre.
Bibliography: p.
Includes indexes.
1. Heat. 2. Physics–Philosophy I. McGuinness Brian. II. Title. III. Series.
QC252.M213 1986 536 86–21921
ISBN-13: 978-90-277-2206-5 e-ISBN-13: 978-94-009-4622-4
DOI: 10.1007/978-94-009-4622-4

DIE PRINCIPIEN DER WÄRMELEHRE

First published by Verlag von Johan Ambrosius Barth, Leipzig, 1896
This translation from the 2nd edition, 1900
Mach's Introduction, Chapters I—V, XXIV, and XXVIII—XXIX translated from the German by Thomas J. McCormack and published in The Open Court (1900—1904).

Translation revised and completed by P. E. B. Jourdain and A. E. Heath

Published by D. Reidel Publishing Company,
P.O. Box 17, 3300 AA Dordrecht, Holland.

Sold and distributed in the U.S.A. and Canada
by Kluwer Academic Publishers,
101 Philip Drive, Assinippi Park, Norwell, MA 02061, U.S.A.

In all other countries, sold and distributed
by Kluwer Academic Publishers Group,
P.O. Box 322, 3300 AH Dordrecht, Holland.

Reprint of the original edition 1986

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ERNST MACH'S *PRINCIPLES OF THE THEORY OF HEAT*

Introduction by Martin J. Klein

Ernst Mach, whose "incorruptible skepticism and independence" Einstein never stopped admiring,[1] chose to describe himself as simply a scientist and not as philosopher or historian of science. In contrast to most of our contemporary philosophers and historians of science, Mach had every right to choose that name. He was, after all, Professor of Experimental Physics at the University of Prague for almost thirty years,[2] and his historically oriented, philosophically critical books on the principles of mechanics, optics, and theory of heat had their origins in his regular duties as a teacher of physics. Mach's renunciation of the title of philosopher was explicit and repeated, despite his evident concern with philosophical issues.[3] Even in the Preface to his book, *Knowledge and Error*, written in 1905, Mach referred to himself as only a "weekend sportsman" when it came to hunting in philosophical preserves, and denied the existence of a "Machian philosophy." He said again that he was "not a philosopher, but only a scientist," and declared: "If nevertheless I am at times somewhat obtrusively counted amongst philosophers, the fault is not mine."[4] Mach's refusal to claim to be a historian is less explicit but no less definite. Despite his hope that in his book on optics, for example, he had "laid bare . . . the origin of the general concepts of optics and the historical threads in their development," Mach hastened to add that "results of historical research have not been accumulated here."[5] And in the book before us Mach warns his readers not to expect the results of "archival research," and goes on to say in the same sentence that he has been more concerned with "the connection and growth of ideas than with interesting curiosities."[6] This remarkable conjunction, implying that archival research could unearth only matters of antiquarian interest, is calculated to shock historians of all persuasions.

If Mach did not think of his books as professional contributions to either the history or the philosophy of science, how did he view them? The goals of his work are announced most explicitly in the Preface to his *Mechanics*, and they do not sound very different from those of many authors writing treatises on the fundamentals of one or another of the branches of physics. Mach wants to "clear up ideas," to bring out "the positive and physical essence of mechanics," so often "completely buried and concealed beneath a mass of technical considerations."[7] He wants to "lay bare the gist and kernel" of his subject. Only when he adds that he wants to "get rid of metaphysical obscurities" does Mach use language noticeably different from that commonly employed by scientific authors. And even here he is dealing with something that every conscientious teacher of physics will recognize: the experience of lecturing "with a certain amount of enthusiasm" on some familiar, generally accepted set of ideas and then suddenly realizing that something is not clear, not only for the lecturer (and his audience!) but also for those writers who have long been repeating these ideas.[8] Some fundamental obscurity has been allowed to persist, perhaps for decades or even centuries. Such obscurities and the problems they concealed were often the stimulus for Mach's historical studies and philosophical critiques.

Mach's younger contemporary, Heinrich Hertz, commented on something very similar when he described the difficulty of introducing mechanics to a thoughtful audience, "without being occasionally embarrassed, without feeling tempted now and again to apologize, without wishing to get as quickly as possible over the rudiments, and on to examples which speak for themselves." Hertz was concerned with the problematic status of the concepts of force and mass, and imagined "that Newton himself must have felt this embarrassment," and that "Lagrange, too, must have felt this embarrassment and the wish to get on at all costs."[9] Hertz's reaction to the obscurities he found in all existing treatments of the subject was to attempt the creation of a new and more abstract system of mechanics, one whose "logical purity" could be proven "in all its details."[10] Mach did not share Hertz's desire for a logically tight system; neither abstract theory nor system building appealed to him. He sought the solution to these difficulties by other means, believing that in such cases there was "only one way to enlightenment: historical studies." Obscure concepts, concepts that had acquired the name "metaphysical," could only have gotten that way "if we have forgotten how we reached them." Historical study of the origin and development of scientific ideas would eliminate the obscurities and lead to genuine understanding. "History has made all; history can alter all. Let us expect from history all," wrote Mach. But this elevated utterance did not suit his usual plain style and so he added that what we should hope for "first and foremost" from any historical investigation, including his own, was that "it may not be too tedious."[11]

That hope is generally realized in Mach's historical writings, most of which are as lively and interesting now as they were when they appeared. Mach did not follow any existing model of historical or philosophical or scientific exposition, but went at things his own way combining the various approaches as needed to reach the goals he set for himself. When he is at his best we get a sense of the Mach whom William James met on a visit to Prague, the Mach whose four hours of "unforgettable conversation" gave the forty year old, well traveled James the strongest "impression of pure intellectual genius" he had yet received, and whose "absolute simplicity of manner and winningness of smile" captivated him completely.[12]

Consider, for example, the first few chapters of this book, *Principles of the Theory of Heat*, which Mach devotes to the notion of temperature, that most fundamental of all thermal concepts. He begins by trying to trace the path that leads from our sensations of hot and cold to a numerical temperature scale. He proceeds from the early use in the seventeenth century of the variable volume of a fluid (gas or liquid) as an indicator of the thermal state of a body, an indicator more sensitive and more reliable than our sensations, to the introduction of numerical scales based on the volume of the thermometric substance to denote these thermal states. But when more extensive and more careful measurements were made early in the nineteenth century by Dulong and Petit, it became clear that every temperature scale depended in an essential way on the particular properties of the thermometric substance that had been chosen. This historical discussion is actually the preliminary to a critical analysis of the temperature concept, but along the way Mach takes the opportunity to describe some of the historical experimental arrangements with the kind of loving attention to nice points that only a genuine experimenter could provide.[13] When he does turn to a critical analysis based on his historical sketch of thermometry, Mach argues that the numerical value of the temperature of a body in thermal equilibrium, measured on any empirical scale, is only an "inventory-number, by means of which the same thermal state can again be recognized, and if necessary sought for and reproduced."[14] There is, however, no basis for introducing the idea of the "true" or "actual" temperature of a body as a property more or less imperfectly determined by the thermometers one can actually use. Mach dismisses the "actual" temperature as an illusion comparable to Newton's absolute space and time, other illusions that he considered he had dispelled in a well-known section of his book on mechanics a decade earlier.[15] Having shown that a temperature scale is, in effect, a numerical way of denoting or naming the thermal states of bodies, Mach goes on to devote a chapter to philosophical reflections on names in general, and on numbers as particular kinds of names. This short chapter is followed by another in which Mach discusses the concept of the continuum, again with special reference to temperature but in a way that indicates how little he was at home with the concepts of pure mathematics.[16]

Mach can be seen at his best in his historical and critical treatment of calorimetry (in Chapters X and XI). Joseph Black, the principal figure in this development, was one of Mach's heroes, and he writes about Black and his work with an insight born of admiration. Mach appreciated "the certainty and clearness" with which Black introduced the concepts of quantity of heat, of heat capacity, and of the latent heats of fusion and vaporization, but he also appreciated Black's general attitudes to science. He saw Black as "a worthy successor of Newton," thinking especially of the way Black "was at pains to dismiss arbitrary fancies, whether they originated from the heads of others or from his own head; to explain facts by facts; to adjust his own conceptual constructions to the facts; and to limit himself to the narrow and indispensable expression of what is actual."[17] This was Black's ideas of the Newtonian approach to science, and it was also Mach's.[18] (Mach wrote that Newton's "reiterated and emphatic protestations" that he was concerned not with hypotheses about causes, but simply with the "*actual facts*," were clear proof that he was "a philosopher of the *highest* rank."[19]) Mach evidently enjoyed recounting Black's arguments and pointing out the far-reaching conclusions he had been able to draw — as in his analysis of the implications of the slow rate at which snow and ice melt when the air temperature rises above the freezing point — "by simple attention to unremarkable experiences which are accessible to everybody." When Mach goes on to comment on Black's "glance, so susceptible to the events in our daily surroundings" and his "clear-sighted analysis of particular experiments,"[20] we cannot help thinking how aptly these words apply to Mach himself. It was Einstein who emphasized Mach's "immediate joy in seeing and understanding," and pointed out that even in his old age Mach looked at the world "with the inquisitive eyes of a child, delighting in the understanding of connections."[21]

Mach was not a theoretical physicist. He saw theories as merely economical ways of condensing the relationships among natural phenomena that are discovered by experiment. If some theories claimed to be more, then they were surely hypothetically based like the atomic theory that Mach rejected. Such schemes might be heuristically valuable for a time but they could be expected to wither away as science progressed. "The object of natural science," Mach wrote in the concluding sentence of his first historical work, "is the connection of phenomena; but the theories are like dry leaves which fall away when they have long ceased to be the lungs of the tree of science."[22] With this rather negative and restrictive attitude to theory, it is not surprising that Mach omitted major aspects of the fields he wrote about. It is a serious limitation in a critic of science that he fails to appreciate the beauty and the power of highly developed theoretical systems.[23]

One is aware of that limitation of Mach's in his book on mechanics, where the keen critical analysis of concepts is not extended to the systematic developments of Euler, Lagrange, and their successors. The same limitation affects the *Theory of Heat*, especially in Mach's treatment of that remarkable conceptual structure, thermodynamics. He does give a sound, interesting, and even dramatic account of the way a situation arose in which "truth and error were in a confusing state of mixture,"[24] and of the resolution of the difficulties in the early 1850's through the work of Rudolf Clausius and William Thomson. But Mach, writing in 1896, gives his readers no sense of the significant new insight into thermodynamics and the vast extension of its scope to be found in the work of Josiah Willard Gibbs in the 1870's. This omission was properly called "indefensible" by Joseph E. Trevor, when he reviewed Mach's book on its appearance, but it is matched by analogous omissions in what Trevor called "his justly famous treatise on mechanics,"[25] as I have already suggested.

Mach's books attracted many readers. Their author, who had felt utterly alone when he set forth on his course of historical-critical studies in science, was gratified by the worldwide response his writings eventually received.[26] He took special pleasure in the recognition accorded his work by colleagues in physics, in philosophy, and in the history of science — recognition from those who were in a position to appreciate the value of what he had done. Mach never forgot, however, that he had written his books as introductions to the subjects they treated, and that he had intended them for an audience much broader than that made up by his professional colleagues. What sort of ideal reader might a teacher like Mach have had in mind?

Considering his general approach and the subjects he chose to emphasize, Mach was presumably addressing himself to a student keenly interested in physics and particularly attracted by its fundamental ideas and problems. This student was tacitly expected to share Mach's responsiveness to the natural world around him, and his readiness to ask a child's naive and all but unanswerable questions about that world. He should be as critical as Mach about the generally received concepts and theories that purport to account for natural phenomena. Mach would have expected that his ideal student did not especially care for the mental gymnastics of problem solving or the polished elegance of mathematical theories. If this student were to be most receptive to what Mach could provide, it would be best if he were not already sitting at the feet of some great teacher. And finally, if Mach were faithful to his own deepest principles, he would have wanted his ideal reader to be as independent and as skeptical of authority as Mach himself, and therefore ready to subject Mach's views and arguments to the same sharp scrutiny he would give to those of others.

Did Mach hope his books would find such readers? We cannot say, but we do know that they found at least one. That ideal reader was Albert Einstein, who was a young student at the Zürich Polytechnic at the time of his first encounter with Mach's writings in 1897. Einstein recalled that event fifty years later in a letter to Michele Besso, the old friend who had arranged that first encounter.[27] "I remember very well that you referred me to his *Mechanics* and his *Theory of Heat* during my first year as a student, and that both books made a great impression on me." Einstein recognized that Mach's influence on his intellectual development was "certainly great," though he found it hard to say just how great it had been or to indicate where it could be seen. "The extent to which they [Mach's books] affected my own work is, to tell the truth, not clear to me." Einstein was less conscious of Mach's influence than that of others when he wrote to Besso, but he recognized that he might not be aware of the full impact Mach had had on him. "As I said, I am not in a position to analyze whatever may be anchored in my unconscious thought." The traces of Mach's influence on Einstein are worth looking for. If we can see what his ideal reader found in Mach's books, we may learn something more about both the books and their reader.

The Einstein who began to read Mach in 1897 was that very unorthodox student who spent most of his time in his room "studying the masters of theoretical physics with sacred passion," instead of attending the regular lectures.[28] From those masters — Kirchhoff, Helmholtz, Boltzmann, and Hertz, among others — he learned to appreciate the power and the appeal of a unified physics that would explain all of nature on the basis of mechanics. "What made the greatest impression upon the student," he wrote in his "Autobiographical Notes," "was less the technical construction of mechanics and the solution of complicated problems than the achievements of mechanics in areas that apparently had nothing to do with mechanics: the mechanical theory of light . . . and above all the kinetic theory of gases."[29] The successes of this latter theory "supported mechanics as the foundation of physics." This vision of a mechanical physics, first formulated in the middle of the seventeenth century, expanded and deepened when Newton added the concept of force to the basic categories of matter and motion, had dominated the thinking of physicists for over two hundred years. Even though many reasons for doubting that this vision could ever be realized accumulated during the nineteenth century, it was still possible at the end of the century for a thoughtful physicist like Hertz to write (with some exaggeration): "All physicists agree that the problem of physics consists in tracing the phenomena of nature back to the simple laws of mechanics."[30]

What Einstein found in Mach, above all else, was a critical examination of this belief in mechanics as the fundamental science and an explicit skepticism about mechanical physics. In his *Mechanics* Mach writes: "The view that makes mechanics the basis of the remaining branches of physics, and explains all physical phenomena by mechanical ideas, is in our judgment a prejudice. Knowledge which is historically first is not necessarily the foundation of all that is subsequently gained. . . . We have no means of knowing, as yet, which of the physical phenomena go *deepest*, whether the mechanical phenomena are perhaps not the most superficial of all, or whether all do not go *equally deep*."[31] Mach's doubts about mechanical physics and his view that it "suffers from being a doubtful anticipation and from one-sidedness,"[32] go back to his earliest historical work, and are restated more than once in his *Theory of Heat*.

Einstein recognized that it was Mach who "shook this dogmatic faith" in the mechanical world view.[33] He considered it Mach's "great merit" that he had "loosened up the dogmatism that reigned over the foundations of physics during the 18th and 19th centuries."[34] But the conclusions Einstein drew from Mach's successful attack on mechanical physics were very different from Mach's own. In rejecting the attempt to explain all phenomena in mechanical terms, Mach argued for a strictly phenomenological physics. "One thing we maintain, and that is that in the investigation of nature we have to deal only with knowledge of the connection of appearances with one another. What we represent to ourselves behind the phenomena exists *only* in our understanding."[35] He saw the mechanical conception of physics as "a hindrance to us in the knowledge of phenomena." Giving up the idea that mechanics was the basic science meant to Mach freeing oneself from the arbitrary and unjustified attempt to find a common theoretical foundation for all of physics. Einstein saw things very differently. He, too, was convinced that mechanics could not provide a basis for physics, but he would not retreat to Mach's position of phenomenalism. That mechanics was not the foundation for all of physics did not necessarily mean there was no such common foundation. Einstein made it his life's task to construct a new unified foundation for his science. It was a task he could not complete, but he never gave up the vision of a unified physics that the mechanical world view had inspired.

In Mach's opinion the idea "that all physical phenomena reduce to the equilibrium and movement of molecules and atoms" was a prime example of the way in which mechanical explanation could become a "hindrance."[36] Throughout his writings he argued against taking atoms seriously. "It is a bad sign for the mechanical view of the world that it wishes to support itself on such preposterous things, which are thousands of years old," he wrote in 1872.[37] He had not changed his opinion a quarter of a century later when the *Theory of Heat* appeared: "Modern atomism is an attempt to make the idea of substance in its most naive and crudest form . . . into the basic concept of physics."[38] He thought that these "childish and superfluous" pictures made a "peculiar contrast" to the real spirit of contemporary physics. The useful analogies that atomistic thinking might supply must never be confused with physical reality.

Mach's youthful reader in Zürich could not have ignored this opposition to atomic theories. Since Einstein was also an ardent reader of Boltzmann's book on the kinetic theory of gases, he would not have overlooked Mach's criticism of a key point in Boltzmann's work: "The mechanical interpretation of the second law, by making a distinction between ordered and disordered motions, by drawing a parallel between the increase of entropy and the increase of the disordered motion at the expense of the ordered, seems to be really *artificial*."[39] Mach also quoted approvingly a remark made by his former student František Wald: "In my opinion the roots of this entropy law lie much deeper, and if the attempt to bring the molecular hypothesis and the entropy law into agreement succeeds, then this would be fortunate for the hypothesis, but not for the entropy law."

Once again Einstein's response was to take due note of Mach's critical position, and then to proceed quite differently from Mach. If Mach (and others) were still skeptical about the existence of atoms, this meant that the evidence in favor of their existence was not conclusive. And Mach was quite right in pointing out that because the second law of thermodynamics could be derived from statistical mechanics, it did not follow that the second law had to be derived on that basis; sufficient conditions need not be necessary ones. Mach's cogent criticisms did not, however, make Einstein reject atomism and statistical mechanics. Instead they may well have spurred his efforts to probe these subjects more deeply than anyone had yet done. This probing of the fluctuation phenomena that are necessary consequences of statistical mechanics led Einstein in 1905 to the Brownian motion and a crucial test of "the kinetic-molecular conception of heat."[40] In Einstein's first letter to Mach in 1909, he wrote that he was sending several reprints and referred specifically to one of them in his letter: "I should like to ask you particularly to look briefly at the one on Brownian motion, because that paper discusses a motion that, I believe, has to be interpreted as 'thermal motion'."[41] (Einstein actually forgot to include the reprints, but when he did forward them to Mach a week later he signed the accompanying note "Your admiring student."[42]) It is too bad that Mach, already past seventy, did not allow himself to be persuaded by the Brownian motion or any of the other new evidence available by then. He never accepted the reality of atoms.[43]

One of the lessons Mach taught in his books was that the structure of a developed science may owe quite a bit to the accidental circumstances of its history. Mechanics, for example, might now look quite different if it had been developed along the lines suggested by Huygens's work, which would have been a logical possibility, rather than along the

Newtonian line that was actually followed.[44] Statements now considered to be basic laws would then be derived theorems, and vice versa. Mach went even further when he emphasized that a principle like that of the impossibility of perpetual motion cannot be thought of as merely a theorem of mechanics, "since its validity was felt long before the edifice of mechanics was raised."[45] The roots of such a principle "are to be sought in more general and deeper convictions."[46] All of this suggests a flexible approach to physics in which the status of a particular result need not be the status it has acquired in the theoretical formulation which happens to have developed historically. Some general principles may well be more significant and even more reliable than the logical structures on which they now seem to be based.

This is certainly a lesson Einstein learned remarkably early in his career, although we cannot prove that he learned it from Mach. Even in his first papers on statistical mechanics, Einstein repeatedly draws his reader's attention to results that in no way "suggest the assumptions underlying the theory from which they were derived."[47] He evidently felt free to use such results even when that supposedly underlying theory no longer applied. He did so in 1904 when he applied the equation for energy fluctuations, derived from statistical mechanics, to blackbody radiation, evidently not a mechanical system. And Einstein argued in 1911 that Boltzmann's principle relating entropy and probability could be relied on completely — "We should admit its validity without any reservations"[48] — even when exploring the treacherous domain of quantum phenomena where there was "no firm foundation on which to build."[49]

The most striking instance in which Einstein seized upon a principle that seemed to be firmly embedded in a particular theory, and recognized that it must hold under circumstances to which that theory did not apply, is his special theory of relativity. It was well-known and universally accepted since the seventeenth century that the same laws of mechanics hold for all observers moving uniformly with respect to each other, that one cannot distinguish the state of rest from a state of uniform motion.[50] This was a principle in mechanics, however, and it was not consistent with the electrodynamic theory developed by James Clerk Maxwell and clarified by his successors, particularly H. A. Lorentz.[51] Einstein pointed out that a variety of considerations, both theoretical and experimental, suggested, nevertheless, "that there is no property of the phenomena that corresponds to the idea of absolute rest, not only in mechanics but also in electrodynamics, and furthermore that the same electrodynamic and optical laws are valid in all frames of reference for which the equations of mechanics are valid . . . ."[52] Einstein made this suggestion into one of the postulates of his theory, extending the "principle of relativity," as he called it, from mechanics to all of physics.

Einstein wrestled for many years with the problem of reconciling the contradictory claims of the relativity principle and the Maxwell-Lorentz electrodynamics. It was not until 1905 that he realized that the "apparent incompatibility" of these two was indeed only apparent: "at last it came to me that time was suspect."[53] Or, as he put it in 1907: "It turned out, most unexpectedly, that it was only necessary to understand the concept of time sufficiently sharply in order to get over this difficulty."[54] (This must be the most extraordinary use of the word "only" in the history of science.)

This all seems very far away from Mach, and especially far from the book before us. But perhaps there is a connection after all. In his discussion of the development of thermodynamics Mach emphasized the conflict, apparently irreconcilable, that William Thomson saw in 1849 between Carnot's principle and the then recent experiments of Joule.[55] Carnot had introduced his principle by an argument that made explicit and apparently unavoidable use of the caloric theory of heat, in which the quantity of heat was conserved. Thomson had just made two new and unexpected applications of Carnot's principle, and was convinced that was "still the most probable basis for an investigation of the motive power of heat." Joule's impressive experiments, on the other hand, seemed "to overturn the opinion commonly held that heat cannot be generated." Thomson saw no way out for the moment; there were "innumerable difficulties — insuperable without farther experimental investigation and an entire reconstruction of the theory of heat from its foundation." Yet, as Mach described, the very next year Rudolf Clausius showed that the conflict in the principles was not irreconcilable. Carnot's principle need not be abandoned in order to accept Joule's result that heat and work were equivalent and interconvertible. The conservation of heat was not, in fact, a necessary basis for Carnot's principle, as Clausius showed by providing a new basis — what we now know as the second law of thermodynamics.

Is it possible that Mach's ideal reader remembered this story as he struggled to reconcile the seemingly irreconcilable — relativity and electrodynamics? We do not know, but there is no doubt that Einstein often pointed out the analogies between his special theory of relativity and thermodynamics. Both were what he called "theories of principle" rather than "constructive theories."[56] Perhaps some features of Mach's discussion of the way the laws of thermodynamics were developed remained "anchored in [Einstein's] unconscious thought."

In any event this sketch of what Einstein found in Mach's writings and put to use in his own work may suggest the rich variety of ideas Mach offered — and still offers — to his readers.

Yale University

EDITOR'S NOTE TO THE ENGLISH EDITION

The original of this work was published in some haste, in order to controvert the views of Boltzmann. Its Englishing has been a more leisurely process. Mach's Introduction and eight other sections (those here styled Chapters I—V, XXIV, and XXVIII—XXIX) were translated by T. J. McCormack and published between 1900 and 1904 in *The Open Court*, the house journal of the distinguished publishing company of that name. In 1912 or 1913 P. E. B. Jourdain, the historian of mathematics, undertook to revise and complete the translation. He also corrected and amplified many of Mach's references and footnotes and added further footnotes and even some text material of his own. Such revision, correction, and amplification (but not, I think, addition) was continued after Jourdain's death in 1919, principally by A. E. Heath. It was judged complete in 1942 and a typescript with manuscript corrections was delivered to Miss Elizabeth Carus, then head of the Open Court Publishing Company. For all that it was cast up in hundreds of words and otherwise marked, the typescript, by some oversight was never published, although, or perhaps because, Miss Carus was at one point convinced that it had been.

A remark by Mr. Rush Rhees, who gave Heath some assistance (which he characteristically minimizes) with the translation, enabled me to infer its existence and to institute inquiries, which eventually led to Professor Elizabeth M. Eames's finding the relevant typescript, untitled, in the Open Court Archives or that part of them held in the Morris Library of Southern Illinois University, Carbondale, Illinois.

This long history (and I must confess that seven years have now passed since the translation was discovered) has been partly due to the laboriousness of all translation (no doubt T. J. McCormack's motive for returning to his own career), partly to the sad coincidence of Jourdain's and Paul Carus's deaths in 1919, but chiefly to the nature of Mach's text with its encyclopaedic profusion of references to men of science and their writings, references often given with the cursory brevity of one who expects the reader to come half way to meet him. The attempt to render all these precise and still more the attempt to take the reader by the hand and explain to him the significance of what Mach is saying and the advances made since Mach's time were bound to founder, since the volume of new work, and even of new publications of old work, would outstrip the diligence of the most assiduous editor. Nor am I sure (if the attempts could be successful) that the book would quite be Mach's. At all events, and despite the interest of the typescript to the historian of science, I have cut the Gordian knot by restoring the notes, to what I hope to be an intelligible version of their original form. Some further comments thought now necessary are enclosed in square brackets and should be attributed to Jourdain or Heath or (when signed *Ed.*) to myself.

Additions to the text I have likewise removed, except that Jourdain's (as I judge) expansion of Mach's summary of the work of the Thomsons in Chapter XIV could not be excised without major surgery. I have let it stand as a piece of harmless partiality. *En revanche* I have not included Jourdain's translation of a paper by Gay-Lussac which Mach only printed (and in French) because it was then not available elsewhere. The translations of Mach's Prefaces are my own.

It is a pleasure to thank Professor Eames and Mr. Rhees, also Professors Paul Schilpp and Eugene Freeman, for help in finding the translation. The Morris Library of Southern Illinois University, Carbondale, and its Librarian have been most forthcoming. Fullest acknowledgement is due to them and, above all, to the Open Court Publishing Company for permitting the publication in the present series of a work originally commissioned by them.

Oxford, October 1985 BMcG

AUTHOR'S PREFACE TO THE FIRST EDITION

The present work sets itself a similar task to that confronted in my *Mechanics.*[1] It aims to give a critical epistemological elucidation of the foundations of the theory of heat, to lay out for inspection the facts that influenced the formation of the relevant concepts, and to show why and to what extent the former are to be understood in the light of the latter. That such a point of view is applicable in this area as well as in others has been indicated in previous works of mine.[2]

Like my *Mechanics* again, the work is from one point of view the outcome, from another the basis, of my lecture courses. Many a teacher will have had a similar experience: to be engaged in the exposition, not without enthusiasm, of generally received views and suddenly to notice that his words no longer come from the heart. Subsequent reflection in private usually leads, after no great interval, to the discovery of logical anomalies, which, once recognized, become intolerable. Such was the origin of many of the individual discussions here collected, and by means of them I have some hopes of fulfilling my general aim: to eliminate idle and superfluous notions and unwarranted metaphysical assumptions from this branch of physics also.

Very many sources have been laid under contribution, but the reader should not expect to find here the results of a search through the archives. My concern is not with the curious and entertaining detail but with the growth and interconnexion of ideas. Biographical particulars are rarely given. Individuals are regarded as intellectual, or at most ethical, personalities, which in my view can only benefit the historical picture here developed.

To keep the size of the book within reasonable bounds, I have had to restrict myself to fundamentals. Subjects that others have treated in detail and where I have nothing material to add, I have passed over or touched on only briefly. The dynamic theory of gases and thermochemistry both fall under this head. It will be understood that I had to leave very recent publications undiscussed. In particular Maneuvrier's history of the *C/c* ratio appeared too late for me to use it. Yet I find that my account of the development of this subject agrees essentially with his.

Those chapters that are of an historical or critical nature I have arranged in the order that, in my judgement, best brings out the complex and changing interrelationship of the questions treated. There are, however, a number of chapters whose content is of a more general and abstract epistemological nature. They belong together as essays in cognitive psychology and I have placed them at the end for the peace of mind of those physicists who find such reading-matter not to their taste. In these chapters coherence demands that I touch again on some topics from my *Popular Scientific Lectures*,[3] the published form of which was not available, nor was publication even envisaged, when I wrote. It will be found that the treatment of problems in the two works is complementary.

Vienna, August 1896 ERNST MACH

AUTHOR'S PREFACE TO THE SEÇOND EDITION (EXCERPT)

A reference in *The foundations of geometry* by B. A. W. Russell alerted me to *The Concepts of Modern Physics* by J. B. Stallo, a work I have since come to know.[1] I should not like to let slip an opportunity to recommend as most relevant this rich and illuminating work. I am wholly at one with the author in his efforts "to eliminate from science its latent metaphysical elements". The first edition of Stallo's work is dated November 1881, but it is partly a reworking of articles published in 1873 and 1874, which in turn go back to public lectures delivered in 1859. It would have been very helpful and encouraging for me, had I known of Stallo's investigations in the Middle Sixties, when I began to work along the same lines.

Vienna, August 1899 ERNST MACH

INTRODUCTION

It is a commonplace of history that the modes of thought current in a given period and acquired by the labors of generations past are not always conducive to the advancement of science, but frequently act as a clog on its progress. Time and again inquirers who stood aloof from — and even in opposition to — the schools, such as Black, Faraday, and Julius Robert Mayer, have been the originators of great scientific advances — such as could only have sprung from their lack of bias and their freedom from traditional professional views. Though the intellectual vigor and unconstraint demanded by such performances are not the outcome of either art or education, but are distinctively a product of nature and the exclusive gift of individuals, nevertheless the mobility and untrammelled play of our thoughts may be greatly enhanced by *scientific education*, at least if it looks beyond the fostering of talents requisite merely for the mastery of the *problems of the day.* Historical studies are a very essential part of a scientific education. They acquaint us with other problems, other hypotheses, and other modes of viewing things, as well as with the facts and conditions of their origin, growth, and eventual decay. Under the pressure of other facts which formerly stood in the foreground other notions than those obtaining to-day were formed, other problems arose and found their solution, only to make way in their turn for the new ones that were to come after them. Once we have accustomed ourselves to regard our conceptions as merely a means for the attainment of definite ends, we shall not find it difficult to perform, in the given case, the necessary transformations in our own thought.

A view, of which the origin and development lie bare before us, ranks in familiarity with one that we have personally and consciously acquired and of whose growth we posses a very distinct memory. It is never invested with that immobility and authority which those ideas possess that are imparted to us ready formed. We change our personally acquired views far more easily.

Historical study affords still another advantage. A consideration of the development, mutations, and decay of ideas leads directly to the discovery, scrutiny, and criticism of the developmental process of our own unconsciously formed views. When the process of growth of these views is not understood, they confront us with all the insuperable might of some alien power.

The purpose of the present book, like that of my *Mechanics* is to trace the evolution of the conceptions of the theory of heat. This task has been facilitated somewhat by some preliminary researches, but the undertaking is, upon the whole, a far more complicated one than that of my earlier work. Whereas the development of the fundamental principles of mechanics was accomplished by three men within the brief space of about a century, the growth of the theory of heat took an entirely different course. Many investigators took part in the building up of this department of physics. Slowly and tentatively, by trial and error, one little advance after another was made, and only very gradually did our knowledge of these phenomena attain to its present magnitude and relative fixedness.

The reason is not far to seek. The motions of bodies are immediately accessible to the senses of sight and touch, and the whole course of events can be observed. Phenomena of heat, on the other hand, lend themselves far less readily to observation. They are directly accessible to one sense only, and are perceptible only discontinuously, in special cases, and usually only when observed intentionally; they therefore play a far more subordinate part both in our intellectual and our perceptual life. They can be brought within range of the dominant senses of sight and touch only indirectly and intricately. The devices for their investigation therefore were, from the very outset, of a predominantly intellectual character, and there were thus insinuated into the very first observations of the subject much subconscious bias and many obscure metaphysical conceptions which seem to be prior to experience and to extend beyond it.

CHAPTER I

HISTORICAL SURVEY OF THE DEVELOPMENT OF THERMOMETRY

1. Of the sensations which we assume to be provoked in us by surrounding bodies, the *sensations of heat* (cold, cool, tepid, warm, hot) form a distinct series or a particular class of elements bearing a definite relationship to one another. The bodies which produce these sensations likewise exhibit, both as to themselves and as to other objects, a distinctive physical behavior definitely associated with these sense marks. A very hot body glows, gives forth light, melts, evaporates, or burns away; a cold body congeals. A drop of water on a hot plate evaporates with a hissing noise: on a cold plate it freezes, and so on. The collection of these instances of the physical behavior of a body, which are connected with the mark of our sensations of heat — the collection of reactions — is termed its thermal state or state with respect to heat.

2. We should be unable to follow the physical processes here involved with anything like readiness and completeness if we were restricted to sensations of heat as our criteria of thermal states. Pour cold water from A (Fig. 1) and hot water from C into a third vessel B, and, after

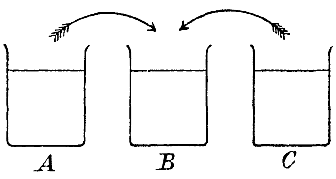


Fig. 1.

holding the left hand for a few seconds in A and the right hand for the same length of time in C, plunge both hands into B; the *same* water will feel warm to the left hand and cold to the right. The air of a deep cellar feels cold in summer and warm in winter, although it can be definitely shown that its physical thermal reaction remains approximately the same the year round[1].

As a matter of fact, the sensation is determined not alone by the body producing it, but partly also by the condition of the perceiving sensory organ, the susceptibility of which is always appreciably affected by its antecedent states. In the same way the light of a lamp seems bright on coming from a dark room, but dull on coming from the sunlight. The sensory organs have, in fact, been biologically adapted not for the advancement of science, but for the maintenance of favorable conditions of life.

Where sensation alone is concerned, sensation alone is decisive. It is, then, an indisputable fact that a body reacting physically in exactly the same way does feel at one time warm to us and at another time cold. It would be utterly unmeaning to say that a body that we feel to be hot is "really cold". But, where the physical behavior of a body with respect to other bodies is concerned, we are obliged to look about us for some distinguishing characteristic of this behavior which shall be independent of the variable and intricate constitution of our senses which is difficult to control; and such a distinguishing characteristic has been found.

3. It has long been known that the *volumes* of substances increase or diminish, other circumstances remaining the same, according as the sensations of heat produced by them are greater or less. In the case of air, this alteration of volume is striking in the extreme. It was familiar even to Hero of Alexandria[2]. It was Galileo, however, the great founder of dynamics, who appears to have first conceived the happy thought of employing the volume of air as a mark of the thermal state, and of constructing on the basis of this idea a *thermoscope* or *thermometer.* It was taken for granted that an instrument of this kind would indicate the thermal condition of the bodies with which it was in contact, on the principle that bodies which are unequally warm soon provoke exactly the same feeling of warmth when brought into contact.

4. The dilatation of air by heat was employed by Hero mainly for the performance of conjuring-tricks. Figure 2, taken from the 1680 Amsterdam edition of his work[3] illustrates one of these devices. A fire being kindled on a hollow altar, the heated air in the enclosure expands, and, pressing against the water in the globe beneath, forces the water through a tube into a pail which, by its descending weight, opens the door of the temple. When the fire is extinguished, the door closes.

Experiments of this character were very much to the liking of

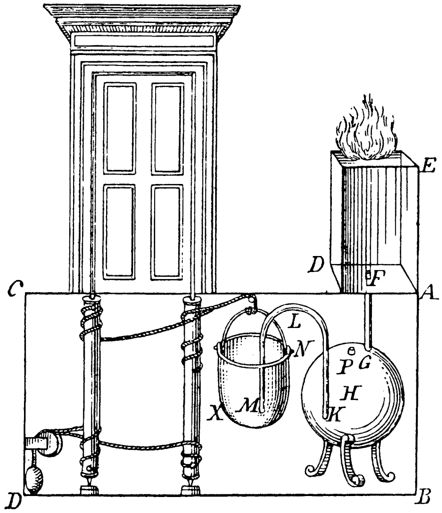


Fig. 2.

Cornelius van Drebbel, of Alkmaar in Holland, who enjoyed in his day the reputation of a magician. In his book of which a translation of the title is: *Treatise on the Nature of the Elements, Winds, Rain, and so on*[4], published in 1608, the experiment illustrated in Figure 3 is described. From a heated retort, the neck and orifice of which are plunged under water, air is expelled in bubbles, and is replaced, after the retort cools, by the inrushing water. The same experiment was described earlier by Porta[5] who went so far even as to determine the

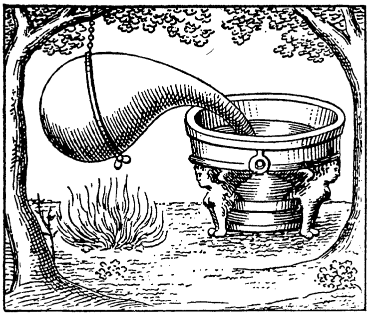


Fig. 3.

amount of expansion of the air by marking the limits of the occupied space before heating and after cooling. But Porta did not hit on the idea of making a thermoscope. In a translation by Ensl[6] of the *Recréations mathématiques*[7], the invention of the thermometer is ascribed to Drebbel, in the description appended to the cut reproduced in Figure 4. But it appears from the researches of E. Wohlwill[8] and F. Burckhardt that this supposition is entirely groundless. Neither is Santorio of Padua, to whom important applications of the thermoscope are rightly credited, the inventor of this instrument.[9] Viviani stated in his biography of Galileo that the latter invented the thermometer in 1592. Galileo himself claimed the invention, and this opinion was shared by Sagredo (who knew Santorio) in a letter to Galileo of March 15th, 1615.

PROBLEMA LXXXIII.

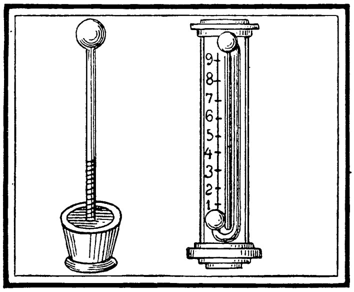


Fig. 4. De Thermometro, fine instrumento Drebiliano, quo gradus caloris frigoris 2 aera occupantis explorantur.

5. From Burckhardt's investigations, which we are here following in the main, it appears indisputable that Galileo was the first to employ the dilatation of air for registering states of heat, and that he therefore is the inventor of the thermometer.[10] The form of this thermometer, as well as of those patterned after it, is given in its essential features in Figure 5. The chief inconvenience of the instrument was that its indications depend on the pressure of the atmosphere, for which reason only observations made in immediate succession furnished comparable results. The division of the scale was mostly quite arbitrary. Here begins the real history of the development of scientific thermometry, of which it is our purpose to give a sketch in the following pages. In doing this we shall try to order the facts so that the way in which each idea provoked its successor, and each step prepared for the one that came after it, is apparent.

Fig. 5.

The form of the air thermometer has undergone many modifications. Guericke's[11] thermometer differs from the original type, above described, only in externals and in more elaborate mechanical construction.

The instrument described by Sturm[12], on the other hand, is a closed differential thermometer and is independent of the pressure of the atmosphere. The air in the bulb (Fig. 6a) is confined by a column of liquid, which, on the temperature's rising, is forced into the longer tube, the air-space of which is shut off from the outside atmosphere.

A siphon-shaped air thermometer closed at both ends and similar in form to the differential thermometer, but having only *one* bulb filled with air, the other containing a *vacuum,* was invented by the Frenchman Hubin[13] (Fig. 6b). A similar but less perfect arrangement we owe to Dalencé[14].

Entirely novel ideas were introduced into thermometry by Amontons.[15] His thermometers consisted of a glass ball *A* about eight centimeters in diameter (Fig. 7), almost filled with air. This air was shut off from the atmosphere by a column of mercury, which partly filled the ball *A* and the thin vertical tube *BC* (1 mm wide). When the ball was heated, the volume of the air contained in it was only very slightly altered, while its tension increased greatly and, by it, the height of the column of mercury, *mn*, which it bore.

Amontons, who was acquainted with the works of Mariotte and referred to them, discovered that the total pressure, including that of the atmosphere, which a quantity of air in *A* will bear when immersed in cold water is increased by one-third of its amount when *A* is plunged into boiling water. This increase of pressure always amounted to exactly one third of the total initial pressure, whatever the latter might be and

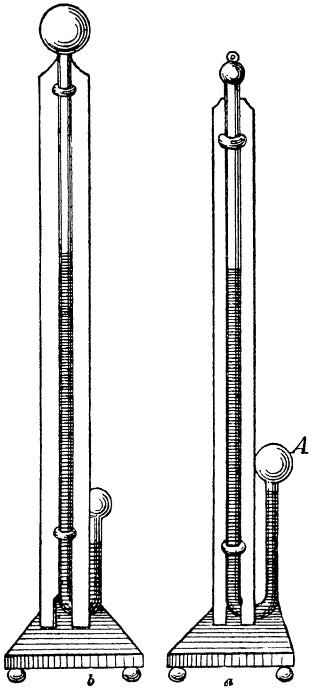


Fig. 6.

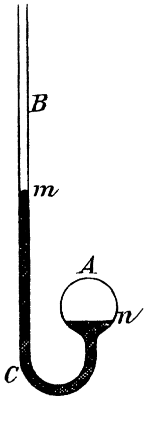


Fig. 7.

whatever the quantity of air in the ball. On the strength of this experiment Amontons concluded that the temperature of boiling was constant.[15] To obtain a greater range of pressure, he filled the ball with air by a simple contrivance until it bore at the boiling temperature the total pressure of a column of mercury 73 inches in height. With the air "tempered", as he phrased it, the column was some 19 inches shorter.

These air thermometers are not independent of the pressure of the atmosphere, but its influence can be calculatent by taking into account the barometer reading. Amontons discussed the great lack of conformity in the readings of the spirit thermometers then in use, and made the attempt to graduate them more accurately by comparison with his own. He also endeavored to make determinations of higher temperatures, by heating one extremity of an iron bar to white heat and ascertaining by the air thermometer the temperature of the point at which tallow just begins to melt, the temperatures of the remaining points being determined by methods of intrapolation and extrapolation not entirely beyond criticism.

In one of his memoirs[16] Amontons actually declared the expansive force of the air to be the measure of the thermal state (temperature), and advanced the idea that the lowest possible degree of cold corresponds to zero tension. In his view, accordingly, the greatest summer heat was to the greatest winter cold, in Paris, only as 6 to 5 approximately.

A remarkable instance of prejudice was exhibited by Amontons in his practice of using, in addition to the boiling point of water, and in the face of his brilliant idea of an absolute zero-point of temperature, the totally unreliable and unnecessary test of "cold" water for indicating a *second* fundamental point.

Amontons also gave expression to interesting subsidiary views. Having observed that the increase in the tension on a rise of temperature is proportional to the density of the air, he suggested an explanation of earthquakes by assuming very dense and heated layers of air in the interior of the earth. He computed that air at 18 leagues depth would have the density of mercury. Nevertheless, the compressibility of air has in his opinion a limit, and cannot possibly extend beyond the point where the "springs" of which the air consists come into contact. Heat consists of "particles in motion."

It will be seen that the ideas of Amontons constitute a decided step in advance in so far as they permit of the construction of genuinely comparable thermometers. Subsequently Lambert actively espoused them. And the scale of temperature at present in use coincides with that of Amontons in its essentials.

Lambert[17] made considerable use of the air thermometer. Like Amontons he regarded the tension of the air as the measure of the temperature, and he also assumed a point of absolute cold to correspond to zero tension. But, following Renaldini, he selected the melting point of ice and the boiling point of water as the fundamental points of his scale, fixed the tension of the air at the first point at 1000 and found it at the latter to be 1417, from which follows a coefficient of expansion of 0.417, in contrast to the 0.375 of Gay-Lussac. In a later experiment, Lambert[18] got 0.375. Lambert also graduated spirit thermometers by his air thermometer, and attached to the air thermometer, in view of the variations of barometric pressure, a moveable scale.

More than a century after Amontons, in the year 1819, two investigators, Clément and Desormes, without a knowledge of Amontons's researches, hit upon exactly the same idea of an absolute zero of temperature[19].

In recent times very perfect air thermometers have been constructed by Jolly and others. The most ingenious and original forms are those devised by Pfaundler. The description of them, however, does not fall within the scope of the present work, which is restricted to considerations of principle.

8. It is not surprising that the pronounced alterations of the volume of air when heated should have attracted attention first, and that the less conspicuous alterations of the volumes of liquids should not have been noticed until later. The difficulty of handling the first air thermometers and their dependence on the pressure of the atmosphere naturally led to the desire for some more convenient instrument. The philosophical impulse to extend the results of single observations to new cases, the impulse to generalize, was never wanting. Said Galileo[20]: "In the opinion of the schools of the philosophers it has been proved a true principle that the property of cold is to contract and the property of heat to expand." Reflections of this character must have prompted investigators to inquire whether the property observed in connection with the air could not be demonstrated also in connection with liquids. Possibly a French physician, Jean Rey (1631), was the inventor of the liquid thermometer[21]. Viviani attributed the invention to Ferdinand II,

Grand Duke of Tuscany, who in 1641 constructed sealed spirit thermometers. The oldest of these instruments registered twenty degrees in snow and eighty degrees at the greatest heat of summer. The degrees were marked with beads of enamel fused on the glass stem. The form is given in Figure 8.

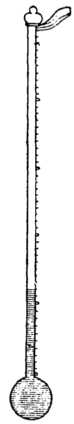


Fig. 8.

The shape and mode of division of these thermometers underwent considerable modification at the hands of the Florentine Academy. Sealed thermometers were first recommended in England by Robert Boyle[22], who also called attention to the importance of a comparable thermometic scale and to the constancy of the freezing point of water. As a fundamental point of reference, however, Boyle gave preference to the congealing point of aniseed oil, of which Halley seems to have made extensive use. The most rational division of the scale, in Boyle's opinion, is that which directly indicates the fractional increment of volume by which the spirit expands from the fixed point — a convention which dispenses with a second fundamental point.

In France, de la Hire (1670) conducted observations with a sealed thermometer constructed by Hubin. Dalencé (1688) selected two points of reference, to the importance of which attention had been called by Fabri. Dalencé's fixed points were the melting point of ice and the melting point of butter, the distance between which he divided into twenty equal parts.

Halley[23] determined the amounts of expansion of water, mercury, and air between the points of intense winter cold and the boiling of water. He observed on this occasion that the temperature of the boiling point was constant, and recommended mercury as a thermometric substance[24]. Simultaneous use of both the freezing and the boiling points for the graduation of thermometers was first made by Renaldini[25]. He also proposed the taking of mixtures of definite weights of ice-cold and boiling water as standards for the graduation of thermometers.

9. The first really good comparable spirit thermometers were constructed, according to Christian Wolff[26], in the year 1714, by Fahrenheit, who soon after also adopted mercury as his thermometric substance, and in 1724 made his method public.[27] Fahrenheit denoted the temperature of a mixture of water, ice, and salammoniac by 0, that of melting ice by 32, and that of the blood by 96. He probably kept silent about the use of constant boiling point of water.

Réaumur[28] chose the freezing and boiling points for the construction of his spirit thermometers, and divided the distance between them, which on the Fahrenheit scale occupies 180 divisions, into 80 divisions. Deluc retained Réaumur's scale, but substituted mercury for spirits. Celsius (1742) divided the interval between the fundamental points of the mercury thermometer into 100 parts, calling the boiling point 0 and the freezing point 100. Strömer subsequently reversed this order, and produced the scale now in common use.

10. It is most difficult to observe the expansion of solid bodies by heat. The first experiments in this direction were apparently conducted by the Accademia del Cimento[29]. It was found that bodies which fitted exactly in orifices before heating could not be passed through them at all after heating. The difficulty of determining linear expansion by the measuring rod was known to Dalencé (1688), Richer (1672), and others. Musschenbroek devised for this purpose in 1729 the well-known quadrant pyrometer, and 'sGravesande put the experiments of the Florentine Academy (the sphere and ring) into the form in which we now have them. Lowitz, in 1753, measured in a very crude manner the elongation of a twenty-foot iron bar exposed to the noonday sun, and found its expansion to be the 1/2500th part of its length[30]. In the case of solid bodies it was most natural to determine the linear expansion, whereas with liquids and gases the cubical expansion was

that most easily ascertained — this being equal, for slight expansions, to three times the linear.

11. A comparison of the volume expansion, which alone has meaning when applied to *all* bodies, brings out the wide differences in the behavior of bodies. From the thermal state of melting ice to that of boiling water, air (and gases generally) expand in round numbers 1/3rd of their bulk, water about 4/100ths, mercury about 2/100ths, lead not quite 1/100th, glass approximately 2/1000ths. It is thus intelligible why first the dilatation of air, then that of liquids, and lastly that of solids was more exactly investigated.

12. The researches above cited show distinctly the devious, laborious, and very gradual manner in which the fundamental facts of thermometry were reached. One inquirer discerns one important aspect, and a second only another aspect. Things discovered were forgotten and had to be rediscovered in order that they might become permanent acquisitions. With the researches mentioned, the period of preliminary tentative investigation ceases, and there succeeds a series of *critical* works, to which we shall next give our attention.

13. Boyle in 1662, and Mariotte in 1679, enunciated the experimental law that the product of the volume of a given mass of gas at constant temperature by the pressure which it exerts on unit of surface is constant. If a mass of air of volume v be subjected to a pressure p, it will assume, on the pressure's increasing to $p' = np$, the volume $v' = v/n$; whence

$$pv = np\frac{v}{n} = p'v'.$$

If we represent the v's as abscissae and the corresponding p's as ordinates, the areas of the rectangles formed by the p's and v's will in all cases be equal. The equation

$$pv = constant$$

gives as its graph an equilateral hyperbola, which is the visualization of Boyle's law (Fig. 9.)

The experiments which led to this law are very simple. In a glass siphon-tube having a closed limb at a and an open limb at b (Fig. 10), a

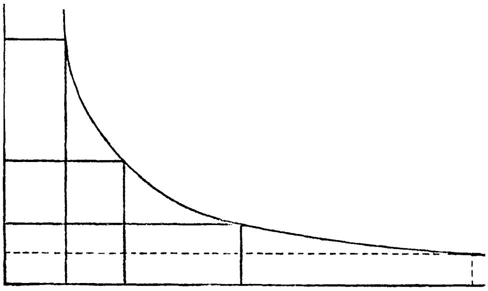


Fig. 9.

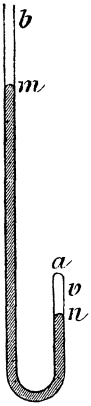


Fig. 10.

quantity of air v is introduced and shut off from the outside air by mercury. The pressure on the enclosed air is given by the height of the mercury-barometer *plus* the difference of level *mn* of the two surfaces of the liquid, and can be altered at will by adding or removing mercury.

14. Experiments in the testing of Boyle's law (which Boyle himself did not regard as absolutely accurate) were carried out through a wide range of pressures and for many different gases by Oerstedt and Schwendsen, Despretz, Pouillet, Arago and Dulong, and Mendelejeff — but most accurately by Regnault[31], and through the widest range of pressures by E. M. Amagat[32].

If the pressure in the apparatus represented in Figure 10 be doubled, the volume v of the gas will be diminished one half; if it be doubled again, it will be diminished one fourth. The errors in the readings increase greatly as the volume decreases, and to eliminate them Regnault resorted to an ingenious expedient. At *a* he attached a stop-cock through which air could be introduced under varying pressure; the volume of the enclosed air v could thus be always kept the same and subsequently compressed to $v/2$ by lengthening the column of mercury *mn*. With such an arrangement the measurements were always of like exactitude.

It appears that, to reduce a unit of volume under a pressure of one meter of mercury to 1/20th of its bulk, it is requisite in the case of air, carbonic acid gas, and hydrogen to increase the pressure to 19.7198, 16.7054, and 20.2687 meters of mercury respectively. The product pv, therefore, for high pressures, decreases for air and carbonic acid gas and increases for hydrogen. The two first-named gases are therefore more compressible and the last-named less compressible than the law of Boyle and Mariotte requires.

Amagat conducted his experiments in a shaft 400 m deep and increased the pressure to 327 meters of mercury. He found that as the pressure increases the product pv first decreases, and after passing through a minimum again increases. With nitrogen, for $p = 20.740$ meters of mercury, $pv = 50989$; for $p = 50$ m, $pv = 50800$, approximately a minimum; and for $p = 327.388$ m, $pv = 65428$. Similar minima are furnished by other gases. Hydrogen showed no minimum, although Amagat suspected the existence of one at a slight pressure.

We shall not discuss here the attempts that have been made by Van der Waals, E. and U. Dühring, and others to explain these phenomena by the molecular theory. It will be sufficient for us to remark that while the law of Boyle and Mariotte is not absolutely exact, it nevertheless holds very approximately through a wide range of pressures for many gases.

15. It was necessary to adduce the foregoing facts for the reason that the behavior of gases with respect to pressure is of importance in the consideration of their behavior with respect to heat — a subject which was first more minutely investigated by Gay-Lussac[33]. This inquirer made mention of the researches of Amontons, and also used the observations of Lahire (1708) and Stancari, from which the necessity of drying the gases clearly appeared. Gay-Lussac's procedure was as follows. A perfectly dry cylinder closed by a stop-cock is filled with gas and plunged into a bath of boiling water. After the superfluous gas has

been expelled, the cock is closed and the cylinder cooled in melting ice. On opening the cock under water, a part of the cylinder fills with water. By weighing the cylinder thus partly filled with water, afterwards completely filled with water, and again when empty, we obtain the coefficient of expansion of the gas from the melting point of ice to the boiling point of water. 100 volumes at 0 ° temperature of air, hydrogen, and nitrogen gave respectively 137.5, 137.48, 137.49 volumes at 100 °C. Also for other gases, and even for vapor of ether, Gay-Lussac obtained approximately the same coefficient of expansion, viz., 0.375. He stated that, fifteen years before, Charles (1787) knew of the equality of the thermal expansion of gases; but Charles had published nothing on the subject. Dalton[34] likewise had occupied himself with this question earlier than Gay-Lussac, and had both observed the equality of the thermal expansion of gases and given 0.376 as the coefficient of expansion.

For the comparison of different gases, Gay-Lussac also used two perfectly similar graduated glass receivers dipped a slight distance apart in mercury (Fig. 11). When like volumes of different gases were introduced into these receivers under like pressures and at like temperatures, both always appeared to be filled to the same marks of division.

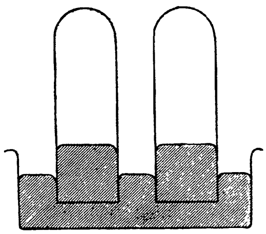


Fig. 11.

In another investigation, Gay-Lussac[35] employed a vessel shaped somewhat like a thermometer and having a horizontal tube in which the air was shut off from the atmosphere by a drop of mercury, the vessel being heated simultaneously with mercury thermometers. Between the melting point of ice and the boiling point of water the expansion of the air is very nearly proportional to the indications of the mercury thermometer.

16. The experiments above described were subsequently performed on a larger scale and with closer attention to sources of error, by Rudberg[36], Magnus[37], Regnault[38], Jolly[39], and others. Two methods were principally employed. The first consists (Fig. 12) in heating a glass vessel A to the temperature of boiling, repeatedly exhausting it, and then filling it with air that has passed over chloride of calcium. While still at boiling temperature, the tip S is hermetically sealed, the barometer noted, the vessel inverted and encased (B) in melting ice, with the tip under mercury. When cool, the tip is broken off, and the mercury rises into the vessel; the difference of level of the mercury within and without the tube is then noted, and the necessary weighing is carried out. It is the method of Gay-Lussac with the requisite refinements.

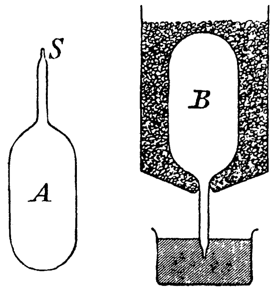


Fig. 12.

The second method (Fig. 13) consists in plunging a vessel A full of dry gas as far as the bend a of the tube first in a bath of melting ice and then in steam from boiling water, while simultaneously so regulating the height of the mercury column at n that the inside surface of the mercury constantly grazes the glass spicule s. The volume of the air is thus kept constant, and what is really measured is the increment of the tension of the gas when heated.

If a volume of gas v under a constant pressure p be raised from 0° to 100 °C, it will expand to the volume $v(1 + \alpha)$, where α is called the "coefficient of expansion". If that gas as it now is at 100 °C were compressed back to its original volume, it would exert, according to the law of Boyle and Mariotte, a pressure p', where

$$vp' = v(1 + \alpha)p.$$

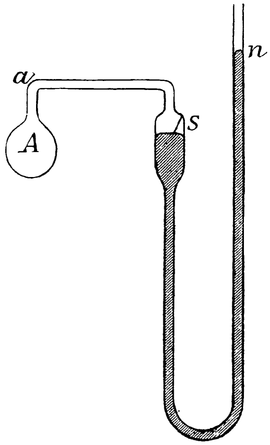


Fig. 13.

Whence it follows that

$$p' = p(1 + \alpha).$$

If Boyle's law held exactly, α would likewise be the coefficient of the increment of tension, or, more, briefly, the "coefficient of tension". But, as the law in question is not absolutely exact, the two coefficients are not identical. Calling the coefficient of expansion α and the coefficient of tension β, the values of these coefficients for the interval from 0° to 100 °C for a pressure of about one atmosphere are, according to Regnault:

	α	β
Hydrogen	0.36613	0.36678
Air	0.36706	0.36645
Carbonic acid gas	0.37099	0.36871

The coefficients of expansion increase slightly, according to Regnault, with the increase of the density of the gas. It further appears that the coefficients of expansion of gases which deviate widely from Boyle's Law decrease slightly as the temperature measured by the air thermometer rises.

Gay-Lussac showed that between 0° and 100 °C the expansion of gases is proportional to the indications of the mercury thermometer. Designating the degrees of the mercury thermometer by t and the 1/100th part of the coefficient of expansion as above determined by α, we shall have, at constant pressure,

$$v = v_0(1 + \alpha t),$$

and at constant volume

$$p = p_0(1 + \alpha t),$$

where v_0, p_0, v, p, respectively represent the volume and pressure of the gases at 0° and t°, and where the coefficients of expansion and tension are assumed to be the same. Each of these equations expresses Gay-Lussac's law[40].

17. Mariotte's law and Gay-Lussac's law are usually combined. For a given mass of gas the product $p_0 v_0$ at the definite temperature 0° has a constant value. If the temperature be increased to t °C and the volume kept constant, the pressure will increase to

$$p' = p_0(1 + \alpha t);$$

wherefore

$$p' v_0 = p_0 v_0(1 + \alpha t).$$

And if the pressure p and the volume v at t° be altered at will, the product will be $pv = p' v_0$.

Whence

$$pv = p_0 v_0(1 + \alpha t).$$

This last law is called the combined law of Mariotte and Gay-Lussac.

Mariotte's law was represented by an equilateral hyperbola. The proportional increase of the volume or the pressure of a gas with its temperature may be represented, conformably to Gay-Lussac's law, by a straight line (Fig. 14). Remembering that α is very nearly equal to 1/273, we may say that for every increase of 1 °C the volume or expansive force increases 1/273rd of its value at 0°, and the that there is likewise a corresponding decrease for every decrease of 1 °C. This increase may be conceived without limit. By taking 1/273rd away 273 times, we reach the expansive force, 0 or the volume 0. If therefore the

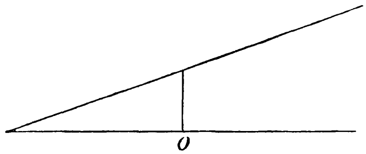


Fig. 14.

gas acted in strict conformity with the law of Mariotte and Gay-Lussac *without limit,* then at $-273\,°C$ of the mercury thermometer it would show no expansive force whatever and would present Amontons's "degree of greatest cold". The temperature $-273\,°C$ has accordingly been called the "absolute zero", and the temperature reckoned from this point in degrees Celsius viz., $T = 273 + t$ the "absolute temperature".

Even if this view of the matter is not taken seriously — and we shall see later that there are grave objections to it — still the presentation of the facts is simplified by it. Writing the law of Mariotte and Gay-Lussac as:

$$pv = p_0 v_0 (1 + \alpha t) = p_0 v_0 \alpha \left(\frac{1}{\alpha} + t \right) = p_0 v_0 \alpha T,$$

and considering that $p_0 v_0 \alpha$ is a constant, we have

$$\frac{pv}{T} = \text{const.},$$

the simplified expression of the law.

18. The law of Mariotte and Gay-Lussac likewise admits of geometric representation. Conceive that there be laid (Fig. 15) in the plane of the

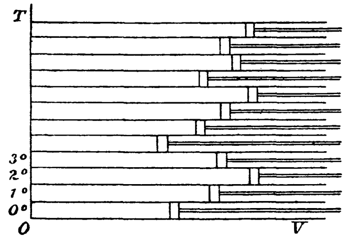


Fig. 15.

paper, a large number of long, similar, slender tubes filled with equal quantities of the same kind of gas. These tubes are made fast at one extremity to *OT* and closed at the other by moveable pistons. The first tube, at *OV*, has a temperature 0 °C, the next a temperature of 1 °C, the next 2 °C, etc., so that the temperature increases uniformly from *O* to *T*. We now conceive the pistons to be all gradually pushed inwards, mercury columns measuring the pressure *p* erected over each position of the pistons at right angles to the plane of their action, and through the upper extremities of these columns a surface drawn. The surface so obtained is imaged in Figure 16, and is merely a synthesis of the graphs of Figure 9 and Figure 14. Every section of the surface parallel to the plane *TOP* is a straight line, conforming to Gay-Lussac's law. Every section parallel to *POV* is an equilateral hyperbola, conforming to the law of Boyle and Mariotte. The surface as an aggregate furnishes a complete synoptic view of the tensions exerted by the same gaseous mass at any volume and at any temperature whatsoever.

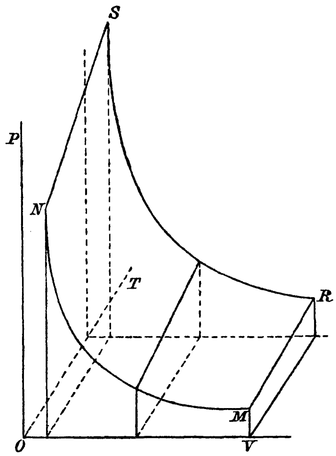


Fig. 16.

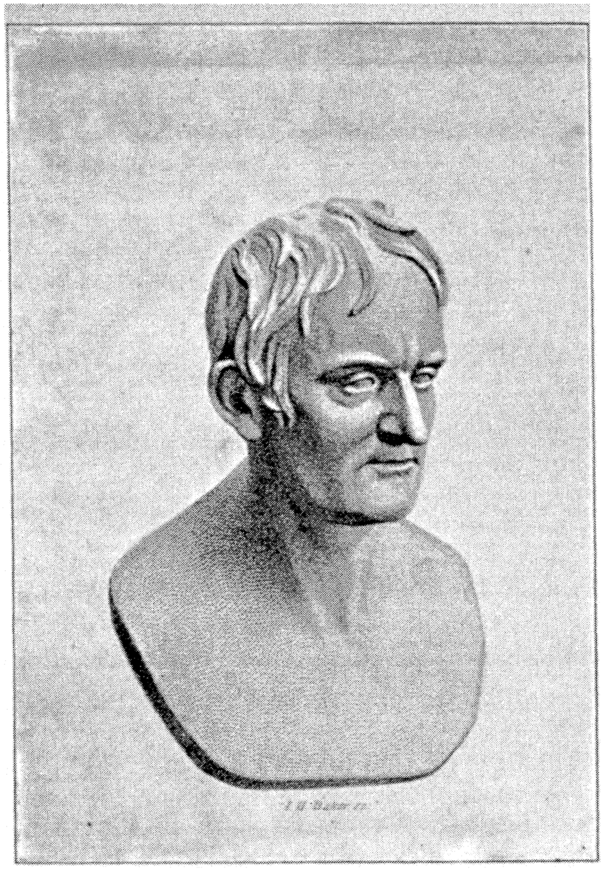
19. The laws in question are in part also applicable to vapors. According to Biot[41], J. A. Deluc[42] appears to have been the first to frame anything like a correct view of the behavior of vapors. H. B. de Saussure[43] knew from observation that the maximum quantity of vapor which a given space can contain depends not on the nature or density

of the gas filling the space, but solely on the temperature. Doubtless this suggested to Dalton[44] the idea of inquiring whether water really was *absorbed* by gases, as was then generally supposed. He caused the liquid to be vaporised in the Torricellian vacuum, and obtained for a given temperature the *same* tension as in air. *Air,* therefore, played no part in vaporisation. Priestley's discovery, that gases of widely differing specific gravities diffused into one another uniformly, combined with that just mentioned, led Dalton to the view that in a mixture of gases and vapors occupying a given space *each component behaved as if it alone were present.* Dalton's way of expressing this fact was by saying that the particles of a gas or vapor could exert pressure only on particles of its own kind.

The discovery that gases behave toward one another precisely as void spaces,[45] is one of the most important and fruitful that Dalton ever made. The way to it had been prepared by the observations above mentioned, and it really only furnishes a clear conceptual expression of the facts, such as science in the Newtonian sense requires. But the preponderance of the speculative element and of a bent for arbitrary constructions in Dalton, which became so fateful in the researches to be discussed farther on, made its appearance even here. Dalton could not refrain from introducing, together with his statement of the facts, an entirely redundant notion which impairs the clearness of his ideas and diverts attention from the main point. This is the "pressure of the particles of different gases on one another."[46] This hypothetical notion, which can never be made the subject of experimental verification, certainly does not impart clearness to the directly observable fact; on the contrary, it involved its author in unnecessary controversies.

20. Gay-Lussac[47] showed, by the experiment represented in Figure 11, that vapor of ether at a temperature above the boiling point of ether behaved exactly as air did on changes of temperature. The observations of de Saussure and Dalton mentioned in the preceding paragraphs, together with that just mentioned, indicate that vapors may occur in two states, viz., as "saturated" and as "non-saturated" or "superheated" vapors.

The conditions involved may be clearly illustrated by an experiment which presents in rapid and lucid succession the different cases before considered separately. We perform (Fig. 17) the Torricellian experiment, and introduce into the vacuum of the Torricellian tube a small



John Dalton.

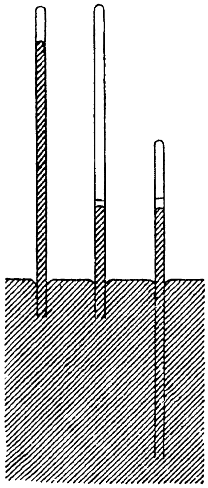


Fig. 17.

quantity of ether by means of a small curved tube. A portion of the ether vaporises immediately, and the mercury column is depressed by the pressure of the vapor, say, at 20 °C, a distance of 435 mm. If the temperature in the barometer tube be raised by a water bath, say to 30 °C, the column will show a depression of 637 mm; whilst in a bath of melting ice it will show only 182 mm. The pressure of vapors, therefore, increases with the temperature. If the tube containing the ether be plunged more deeply into the mercury, so as to diminish the space occupied by the vapor, the height of the surface of the mercury in the tube will still not be altered. The pressure of the vapor, therefore, remains the same. But it will be noticed that the quantity of liquid ether has slightly increased and that therefore a portion of the vapor has been liquefied. As the tube is withdrawn the quantity of liquid ether diminishes and the pressure again is the same.

A small quantity of air introduced into the Torricellian vacuum also causes a depression of the barometer column — say 200 mm. If the tube be now plunged in until the air space is reduced one half, the depression according to Boyle's law will be 400 mm. In precisely the same manner vapor of ether behaves conformably to Gay-Lussac's observation, provided the quantity of ether introduced into the tube is so small that *all* the ether vaporises and a still greater quantity *could* vaporise. For example, when at 20 °C a depression of only 200 mm is generated by the enclosed ether, the tube contains no liquid ether. Diminishing the Torricellian vacuum one half doubles the depression. The depression may be increased by further immersion to 435 mm. But still further immersion of the tube no longer augments the depression, and *liquid* ether now makes its appearance.

21. The preceding observations relative to vapors may be epitomized by a simple illustration. A long tube closed at *O* contains an adequate

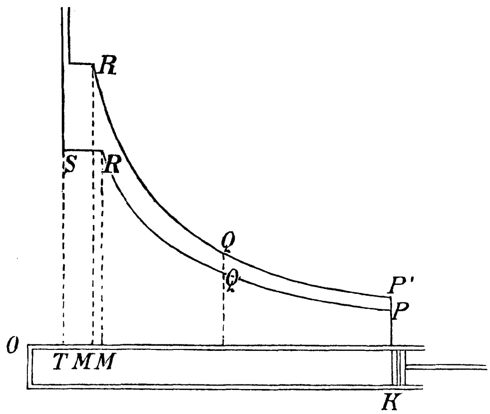


Fig. 18.

quantity of rarefied vapor. If the piston *K* be gradually pushed in and mercury columns measuring the pressures be erected at every point over which the piston passes, the extremities of these columns will all lie in the hyperbola *PQR*. But from a definite position *M* of the piston on, the increase of pressure ceases, and liquefaction takes place. If at the position *T* of the piston nothing but liquid remains in the tube, then a very great increase of pressure follows on the slightest further movement of the piston. Repeating this experiment at a higher temperature, we obtain increases of pressure corresponding to Gay-Lussac's law and the coefficient of tension (0.00367), as the curve *P′Q′R′* indicates. The liquefaction of vapors begins only at higher pressures and greater densities.

Vapors of sufficiently small density thus approximately conform to the law of Mariotte and Gay-Lussac. Such vapors are called "non-

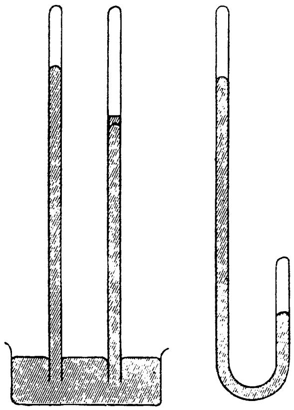


Fig. 19.

saturated" or "superheated" vapors. If the concentration of the vapors is continued, they reach a maximum of tension and density which cannot be exceeded for any given temperature, as every further diminution of the vapor space causes a partial liquefaction of the vapor. Vapors at the maximum of tension are called "saturated" vapors. Given enough liquid and sufficient time, this maximum of tension will always establish itself in a closed space.

22. The relationship between temperature and the tension of saturated vapors or between temperature and maximum tension has been investigated for different vapors by many inquirers. The methods they employed are reducible to two fundamental types. The first consists in introducing the liquid to be investigated into the Torricellian vacuum and in placing the latter in a bath of definite temperature. The amount of depression as contrasted with the barometer column gives the tension of the vapor. If the open end of a siphon barometer, which has been exhausted and charged with the liquid, be hermetically sealed and placed in a bath of given temperature, the mercury column will indicate the tension of the vapor independently of that of the atmosphere. This procedure is only a modification of the preceding one. The method here employed is commonly called the statical method.

Vapors are being constantly generated at the free surface of liquids. For a liquid to *boil,* that is, for bubbles of the vapor to form in its interior, expand, rise to the surface and burst, it is necessary that the tension of the hot vapor in these bubbles should at least be in equilibrium with that of the atmosphere. The temperature of boiling is therefore that temperature at which the tension of the saturated vapor — the maximum tension — is equal to the pressure of the atmosphere. If a liquid, therefore, be boiled under the receiver of an air pump, by means of which the air pressure can be raised or lowered at will (being kept constant by the cooling and re-liquefaction of the generated vapors) the temperature at which the liquid boils will give the temperature for which the air pressure produced is the maximum tension of the vapor. Thus in Figure 20, *B* is a large glass flask connected with an air pump, by which the air pressures are regulated. In *G* the liquid is boiled and the vapors generated; they are re-liquefied by cooling the bent tube *R*. This method is commonly called the *dynamical* method.

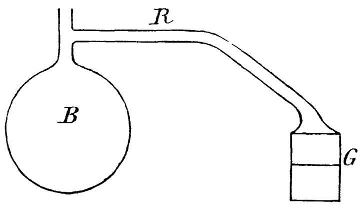


Fig. 20.

Experiments were conducted according to these methods by Ziegler (1759), Bétancourt (1792), G. G. Schmidt (1797), Watt[48], Dalton[49] (1801), Noe (1818), Gay-Lussac[50] (1816), Dulong and Arago (1830), Magnus[51] (1844), Regnault[52] (1847), and others.

For the same temperature the maximum tension varies greatly with the liquid, and it also increases rapidly with the temperature. Dalton had already sought a universal law for the dependence of maximum tensions on temperature, and his investigations were continued in recent times by E. and U. Dühring and others. The purpose and scope of our work preclude our discussing these researches. It was the investigations into *water* vapour, owing to their practical importance for the operation of steam engines, that were the most extensive. Regnault found the following relationship between temperatures and maximum tensions, expressed in millimeters of mercury:

°C	mm	°C	mm
0.00	4.54	111.74	1131.60
52.16	102.82	131.35	2094.69
100.74	777.09	148.26	3359.54

It will be seen from this extract from Regnault's table that the tension of water vapor from 0° to 100 °C increases by about one atmosphere; while from 100° to 150° it increases by more than three atmospheres. The rapid rise of the curve of tensions on increase of temperature, as represented in the graph of Regnault, renders this relationship even clearer.

A more extensive extract from this table in the vicinity of the vapor pressure of 760 mm is of value in ascertaining the influence of atmospheric pressure in the determination of the boiling point on thermometers.

23. The rapid increase of the tension and density of saturated vapors suggested to Cagniard de Latour[53] the idea that at high pressures and temperatures vapors could be produced the density of which varied only slightly from that of their liquids. He filled a portion of a musket-barrel nearly half full of alcohol put a flint ball in it and closed it. As the barrel was raised to higher and higher temperatures, the sound which the ball produced when shaken against the sides of the barrel suddenly changed. In a glass tube from which the air had been expelled a quantity of liquid alcohol nearly half filling the tube was rendered entirely invisible by heating. When the tube was cooled, it again made its appearance as a dense shower. The experiments were then continued with the tube shown in Figure 21. Ether was introduced at *a* and

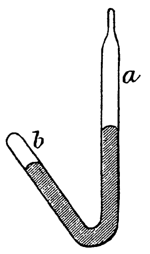


Fig. 21.

separated from the air in *b* by mercury. The compression of the air gave the pressure of the liquid, the thermometer of the bath in which the tube was immersed gave its temperature. Ether disappeared at 38 atmospheres and 160 °C, alcohol at 119 atmospheres and 207 °C, their vapors occupying something more than twice the space taken up by the liquid. Water disappeared at the temperature of melting zinc, and took up four times the space occupied by the liquid. Since the tubes when too small for the expansion did not burst immediately, Latour correctly concluded that the liquids were extremely compressible in this state and had very large coefficients of expansion.

Prompted by Davy, and perhaps also by the researches of Latour, Faraday[54] endeavored to liquefy chemically prepared gases confined in closed spaces — an undertaking in which he was in several instances successful. The idea of these experiments had, indeed, been clearly suggested by the proof which Gay-Lussac had furnished of the like behavior of gases and non-saturated vapors, as well as by Latour's experiment, showing that vapors at high pressure were liquefied by a slight diminution of temperature and revaporised by a slight increase of temperature. A simple example is that of the liquefaction of cyanogen, which occurs when mercuric cyanide is heated in one end *a* of a glass tube (Fig. 22), and the other end *b* of the tube is cooled in water. The

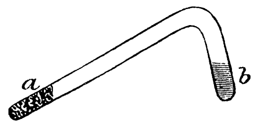


Fig. 22.

generated gas is liquefied at *b*. These experiments were continued on a larger scale with carbonic acid gas by Thilorier and Natterer[55], the latter of whom especially was successful in liquefying large quantities of carbonic acid gas by means of an appropriately constructed force pump. However, many gases — the so-called permanent gases — remained unliquefied.

24. The experiments of Andrews[56] first indicated the mode of procedure by which finally Cailletet and Pictet (1877) were enabled to liquefy all gases. Andrews compressed dried and de-aerated carbonic acid gas by means of mercury forced with a screw into a glass tube

G ending in a capillary prolongation g (Fig. 23). The phenomena occurring in g, which was plunged in a bath of any temperature, could thus be observed conveniently, whilst air confined in a similar tube and subjected to the same pressure served as a manometer. It was found that carbonic acid gas could not possibly be liquefied by any pressure at a temperature above 30.92 °C, whereas it was possible to liquefy it at temperatures below this point. Andrews called this temperature the "critical temperature", and it was demonstrated that every vapor and every gas possessed such a critical point, the sole difference being that the point in question was high for the so-called vapors and easily condensable gases, and very low for the so-called permanent gases. Utilising the results of Andrew's researches and employing extreme degrees of cold, Cailletet and Pictet succeeded in liquefying all gases.

Fig. 23.

Aeriform bodies above the critical temperature are, accordingly, in Andrews's conception, gases, and those under the critical temperature vapors. The very rapidity of the augmentation of the curve of maximum tension suggests that above a certain temperature this maximum tension will transcend all limits or become infinitely great. This limiting point actually exists; it is Andrews's critical temperature.

Mendelejeff called the critical temperature the "absolute boiling point." As the pressure increases, the temperature of boiling rises until the maximum tension of the liquid equals the pressure to which it is subjected. But at the critical temperature the pressure that could prevent the liquid from boiling is infinitely great; it boils under every pressure. Mendelejeff also showed that the surface tension of the liquid, which decreases as the temperature rises, disappears at the critical temperature.

The behavior of carbonic acid gas as shown by Andrews, and its deviations from the law of Mariotte and Gay-Lussac, are graphically represented in Figure 24. The curves correspond to those of Figure 18.

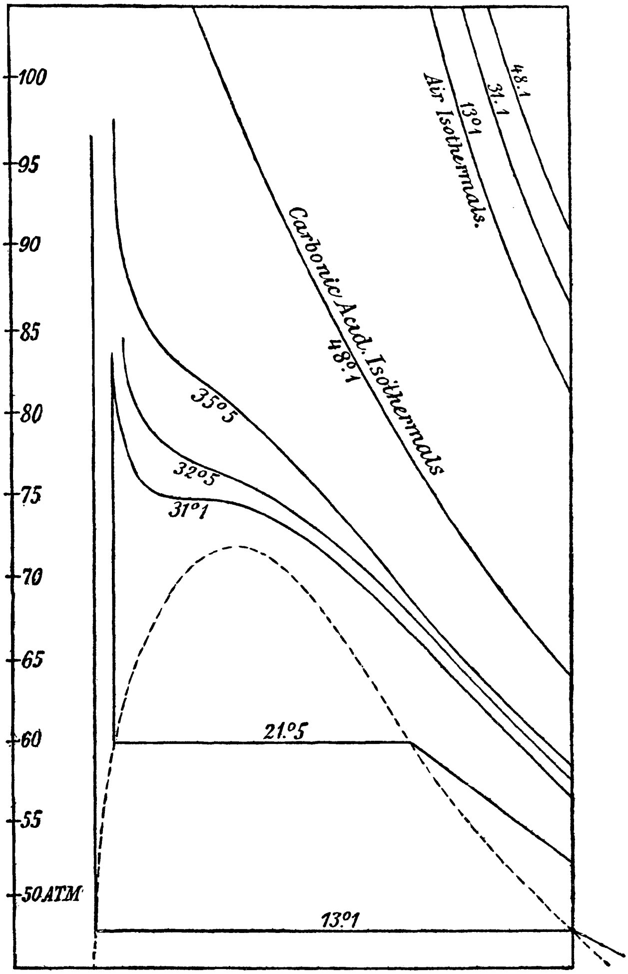


Fig. 24.

The abscissae represent the volumes. The curves of the figure extend from the second to the fourteen thousandth part of the volume of carbonic acid gas at 1 atmosphere of pressure and 0 °C. The dotted line bounds the region within which the carbonic acid gas can exist partly in a liquid and partly in a gaseous form.

25. Figure 16 may, by a slight modification, be made to represent the behavior of gases and vapors. This modification is shown in Figure 25. The pressure of the vapor at a given temperature ascends in the curve *mn*; but at *n* liquefaction begins. The pressure of the vapor at a higher temperature ascends by the curve *pg* to the greater maximum *g*; and so with the rest. To the right of the curve *ngrs,* the vapors behave as gases; to the left, liquefaction sets in. Conceiving a distant light with rays parallel to *VO* to cast a shadow of the curve *ngrs* on the plane *POT,* we should obtain Regnault's curve representing the increase of the maximum tension of the vapor with the temperature. The lowest temperature at which the curve *ut,* by which the rise of the pressure with diminishing volume is indicated, no longer cuts the curve *ngrs,* is the critical temperature. Accurately viewed, the sections of the surface of Figure 25 parallel to *POV* are not exact hyperbolas for either gases or vapors. This is approximately true only of the sections to the right of *ngrs* at some distance from this curve. In the vicinity of the curve and to the left of it, the forms appear which the graphs of Andrews in Figure 24 show.

26. Although the investigation of liquids furnished no such general results as that of gases, yet a few observations in connection with them must be mentioned. Even the Accademia del Cimento is said to have been familiar with the fact that water heated from the freezing point contracted at first and only later expanded.[57] Deluc[58] observed that the peculiar behavior of water thermometers was attributable to an anomaly of the water itself, and, without taking account of the expansion of the glass walls, fixed its point of greatest density at +5 °C. C. G. G. Hällstrom[59] was the first to examine this phenomenon more minutely by determining the loss of weight of a glass body of known coefficient of expansion in water at different temperatures. Hagen and Matthiessen followed the same method. Despretz[60] observed the temperature of the different layers of water when cooled in a vessel. The

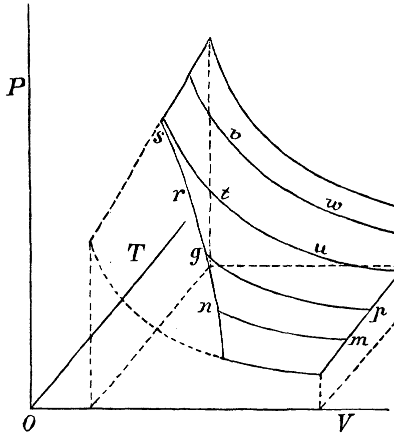


Fig. 25.

water of least density formed the uppermost layer, and consequently when the water first began to cool has the highest temperature. On passing through the temperature of the maximum density, this relation of things was reversed. F. Exner[61] augmented the delicacy of this method by using thermo elements instead of thermometers. Plücker and Geissler used a thermometer-like vessel partly filled with water. The most accurate determination of the temperature of the maximum density of water was in all probability that made by F. Exner, who found it to be +3.945 °C. The investigations just mentioned are of fundamental importance, since they overthrew the very natural belief in the uniform and parallel behavior of all bodies expanding under the action of heat.

There still remain to be mentioned, for the methods involved, the measurements of the expansion of solids which Lavoisier and Laplace jointly conducted, and which Roy completed after the manner of Ramsden. Lavoisier and Laplace[62] combined the quadrant pyrometer of Musschenbroek, which was rotated by the expanding rod, with a telescope set to a distant scale. The reading was considerably magnified, but every inaccuracy of the apparatus was also reproduced on an enlarged scale. Roy[63] employed three bars, all in ice (Fig. 26). The first carries two illuminated cross-threads, *F*, *F'*; the second, the one to be investigated, carries two microscopic objectives, *A*, *A'*; the third two eye-pieces with cross-threads, *B* and *B'*. The images of the cross-

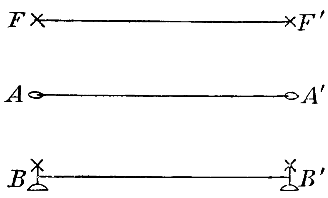


Fig. 26.

threads F, F' are aligned with the cross-threads of the eye-pieces. If the bar in the middle is now plunged in a bath of higher temperature, the distance between A and A' will be increased. By moving the bar in the direction A, A', the image of F can again be aligned with the cross-thread of the eye-piece B, and, by a micrometric displacement of A' along the bar, the image of F' can also be aligned with the cross-thread of eye-piece B'. This last displacement measures the linear dilatation of the middle bar.

28. Dulong and Petit enriched the thermometric knowledge of their predecessors by a number of careful experiments, and set forth the entire thermometry of their time in a classical work which was given a prize by the Paris Academy[64]. The labors of these physicists consist essentially in having made an accurate comparison of different thermometer scales within wide ranges of temperature. The thermal conditions being the same, the comparative behavior of mercury thermometers and air thermometers corrected with regard to the expansion of the glass is as follow:

When the mercury thermometer indicates	The air thermometer indicates
−36	−36
0	0
100	100
360	350

For reducing the indications of the mercury thermometer to those of the air thermometer, the foregoing table would be sufficient. But to compare the real expansions of air and mercury, additional experiments had to be made. A siphon tube *AB* (Fig. 27) was filled with mercury, and one of the arms *B* was plunged in a bath of melting ice, whilst the other *A* was immersed in a bath of oil and brought to higher tempera-

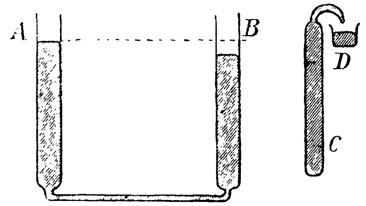


Fig. 27.

tures. The heights of the two columns of mercury, as measured by the cathetometer, were to each other directly as the volumes of the same mass of mercury at the two temperatures in question. The temperatures of the oil bath were determined by means of an air thermometer and a mercurial weight thermometer. This latter consisted of a vessel filled with mercury at 0 °C and terminating in a bent capillary prolongation, from which quantities of mercury determinable by weight were expelled as the temperature rose. The amount of mercury expelled, like the apparent cubical expansion of the ordinary mercury thermometer, was determined by the difference of the expansion of the mercury and the glass. Column *A* of the following table gives the temperature derived from the absolute expansion of the air, *C* that derived from the apparent expansion of the mercury (as determined by the weight thermometer), and *B* the mean absolute coefficient of expansion of the mercury between 0° and the temperature recorded.

A	*B*	*C*
0	0	0
100	$\frac{1}{5550}$	100
200	$\frac{1}{5425}$	204.61
300	$\frac{1}{5300}$	313.15

Designating the absolute cubical expansion of the mercury by α, that of the glass by β, and the apparent expansion of the mercury in the glass vessel by γ, we have $\gamma = \alpha - \beta$. So the table gives us the expansion of glass as well. Calling the temperature derived from the expansion of air A, that derived from the expansion of glass at the same thermal state D, and supposing the scales to be coincident at 0° and 100°, we would obtain:

A	D
100	100
200	213.2
300	352.0

Knowing the expansion of mercury and glass, there is nothing to prevent our inserting a small rod of iron in a glass thermometer and filling the remainder of the tube with mercury. Treating this arrangement as a weight thermometer and rendering the surfaces of the enclosed substances proof against amalgamation by oxidising, we obtain in a perfectly obvious manner the cubical expansion of iron or of any other metal. If v is the volume of the glass tube and v_1 the volume of the metallic rod at 0 °C and if α, β, γ be the coefficients of expansion respectively of mercury, glass, and the metal between 0° and t°, then the total volume of the mercury expelled at the temperature t° will be

$$\omega = v\alpha - v\beta + v_1\gamma,$$

from which γ is determinable.

From experiments like the foregoing, Dulong and Petit reached the following conclusions:

1. Deriving the temperatures from the indications of the air-thermometers, the coefficients of expansion of all other bodies are found to increase with the temperature.
2. Determining the temperatures by the indications of an iron thermometer, the coefficients of expansion of all other bodies are found to diminish as the temperature increases.

3. Measuring the temperatures by the absolute cubical expansion of mercury, the coefficients of expansion of iron and copper increase, while those of platinum and air decrease, as the temperature increases.

The expansion of air, iron, copper, and platinum corresponding to the same thermal states are given by the following table:

Air	Iron	Copper	Platinum
100	100	100	100
300	372.6	328.8	311.6

Hence, if several different bodies are subjected to the same thermal changes, their variations of volume are by no means proportional to one another, but each body exhibits an individual behavior peculiar to itself. Only gases, as Gay-Lussac showed, obey the same law of expansion. This result of the work of Dulong and Petit is fundamental importance for the theory of thermometry.

29. Deluc and Crawford early sought for a body whose expansions should be proportional to the quantities of heat[65] it absorbed. Dulong and Petit likewise granted the rationality of a temperature scale whose degrees would also measure the quantities of heat absorbed by the thermometric substance; and the same idea occurred, as we have seen, in a slightly different form to Renaldini[66]. But Dulong and Petit saw clearly that such a scale would be of value only if the heat capacity was independent of this temperature scale for other bodies as well; or, what comes to the same thing, only provided that the variations of the thermal capacities of all bodies for the same variations of thermal state were proportional to one another. This question, accordingly, was attacked experimentally.

The heat capacities of bodies were now investigated with greater accuracy and throughout wider ranges of temperature than ever before. Boiling water and boiling mercury were employed to raise the bodies to a definite temperature. Accurately weighed quantities of the different substances were then immersed in an equally accurately determined large body of water, the rise of the temperature of which indicated the quantities of heat given off by the bodies. The following table gives the results of these experiments:

	Mean capacity between 0° and 100°	Mean capacity between 0° and 300°
Mercury	0.0330	0.0350
Zinc	0.0927	0.1015
Antimony	0.0507	0.0549
Silver	0.0557	0.0611
Copper	0.0949	0.1013
Platinum	0.0355	0.0355
Iron	0.1098	0.1218
Glass	0.177	0.190

As will be seen not only do the capacities for heat increase with the temperature as recorded by the air thermometer, but they also increase in different proportions with different substances, and would also increase in like manner were the temperature recorded by the mercury thermometer. The law of the variation of capacity for heat is therefore peculiar to each substance.

Dalton imagined himself justified by the state of research of his time in formulating the following singular laws of temperature ("four most remarkable analogies"):

All pure homogeneous liquids, as water and mercury, expand from the point of their congelation, or greatest density, a quantity as the square of the temperature from that point.

The force of steam from pure liquids, as water, ether, etc., consititutes a geometrical progression to increments of temperature in arithmetical progression.

The expansion of permanent elastic fluids is in geometrical progression to equal increments of temperature.

The refrigeration of bodies is in geometrical progression in equal increments of time.[67]

Consonantly with these views, Dalton proposed a new scale of temperature, the degrees of which increased in length with the temperature. The mean between freezing and boiling water, or 122° on the new scale, corresponds to 110° on the Fahrenheit scale. If a quantity of air expands on being heated, in the ration of 1 to 1.0179, Dalton added 10° on his new scale; and when its volume diminishes in the ration of 1.0179 to 1, he subtracted 10°. The points 32 and 212 coincide on Dalton's and Fahrenheit's scale.

Studying in an unbiassed manner the portion of Dalton's treatise with which we are here concerned, one is struck by the irresponsible caprice with which he framed his assumptions and constructions. The clearness and precision of his exposition has suffered so much by the introduction of superfluous hypothetical elements that it is by no means easy at times to grasp clearly his meaning. He compared the heated body to a vessel, the heat it contains to the liquid the vessel holds, the temperature to the height at which the fluid stands. It was an indisputable fact for him that equal increments of the quantity of heat in any body correspond to equal increments of temperature. Since, however, according to his views the capacity increases with the volume, this view is again untenable. No precise definition of what he understood by temperature is found anywhere in the text. The properties of his new scale are determinable from his tables alone.

The following illustrates the way in which Dalton would adopt most hazardous constructions. The higher and more rarefied layers of the atmosphere are colder. On rarefaction, the air cools, and consequently gains, according to Dalton's opinion in capacity for heat. Dalton, in explanation of the coldness of the higher regions of the atmosphere, then calmly assumed that layers of air in contact tend, not towards equality of temperature, but towards equality of content of heat[68], per unit of volume.

As a matter of fact, Dulong and Petit[69], in consequence of their investigations, which showed the behavior of bodies to be in each case peculiar to themselves, and so subject to no general law, found themselves obliged to repudiate utterly the above-mentioned laws of Dalton. Even Dalton himself subsequently became convinced of the untenability of his laws.[70]

The researches of Dulong and Petit thus indisputably demonstrated, as these authors in their conclusion claimed, that all thermometric scales were dependent on the particular thermometric substance selected. Universal comparability was, they found, the property of gas thermometers only; and, without condemning all others, they recommended these thermometers as the best. We have now substantially reached the point of view which we shall assume in the following discussion. It is unnecessary for our purpose, which is the discussion of principle, and it would be quite pointless, to consider here the recent and more refined investigations in thermometry which Pernet and others have conducted.

33. The development of thermometry from the use of the first air thermometer (probably in 1592) to the attainment of considerable clarity in points of principle in this domain (1817) covered an interval of some 225 years. Manifold were the paths entered upon, and again and again were they forsaken and re-trodden before the fragments of our knowledge were all gathered and united into a comprehensive view of the whole. The air thermometer was invented. Its defects led to the employment of liquid thermometers, the insufficient comparability of which provoked new efforts and thus ultimately threw into full consciousness and light the quest for a rational scale of temperature. The search for fixed points and for a rational scale required much time and experimentation, the upshot of which was the reinstatement of the improved air thermometer as a standard instrument. We are now in a position to consider critically the results of our historical survey, which we shall next proceed to do.

CHAPTER II

CRITICAL DISCUSSION OF THE CONCEPTION OF TEMPERATURE

1. It appears from what has preceded that the volume of a body may be employed as a mark or index of its thermal state, and that consequently change of volume may be looked upon as indicating a change of thermal state. It is understood that the changes of volume here involved are not such as are determined by alterations of pressure or electric force, or by any other circumstances inducing change of volume known from experience to be independent of the thermal state. Concomitantly with the heat sensation which a body provokes in us, other properties of the body also undergo alteration — as, for example, its electric resistance, its dielectric constant, its thermoelectric motive force, its index of refraction, and so on. And not only might these properties be employed as indicators of the thermal state, but they actually have occasionally been so used. In preference for volume, as a measure of states of heat, therefore, there is involved, despite the manifest practical advantages of the choice, a certain arbitrariness; and in the general adoption of this choice, a convention.

2. A body employed as a thermoscope initially indicates only its own state of heat. But rough observation informs us that two bodies, *A* and *B*, which at first provoke in us unlike sensations of heat, after prolonged mutual contact excite in us precisely the same sensations; that is, they equalize the difference of their thermal states. Transferring this empirical discovery by analogy to volumes as indices of thermal states, we assume that a thermoscopic body indicates not only its own state but also that of any other body with which it has been sufficiently long in contact. But in so summarily proceeding we are acting without warrant. For sensation of heat and volume are two entirely disparate elements of observation. The fact of their connection has been learnt by experience; the manner and extent of their connection it also remains for experience to teach.

3. We may convince ourselves easily that volume and sensation of heat are indices of widely different sensitiveness and generally of different

character. By means of volume we can perceive changes of state that utterly escape our sensations of heat. And because of their dissimilar properties the thermoscope and the sensory organ of heat may give not only different but even diametrically opposed results. The examples quoted in §2 of Chapter I amply illustrate this fact. But the indications may also be different with respect to equalized thermal states. Two pieces of iron, after long contact, give the same sensations of heat. A piece of wood and a piece of iron after long enough contact also show on the thermoscope the same indications. But if both feel warm, the iron will feel the warmer of the two, no matter how long they have been in contact; and if both feel cold, it will feel the colder. This, as is well known, is due to the greater conductivity of iron, which imparts its thermal state to the hand more rapidly than wood.

Volume being a more sensitive index of the thermal state than sensations of heat, it is more advantageous and rational for us to resort for our empirical results to observations of volume, and to base all definitions on these. Observations based on sensations of heat may serve us for guidance, but to employ them outright and uncritically is, as we have seen inadmissible. We assume with this perception an entirely new point of view, and one which is essentially different from that occupied by the original founders of thermometry. The imperfect separation of these two points of view, which owing to the gradual transition of the one into the other was unavoidable, became, as we shall see, the occasion of many obscurities in the theory of heat.

The fact that a thermoscope shows an increase of volume when in contact with a body that is perceptibly warmer, and a diminution of volume when in contact with one that is perceptibly colder, is indisputable. But it is not within the power of our sensations of heat to inform us whether this continues to be so until the thermal states are completely equalized. On the other hand, we can, consonantly with our new point of view, arbitrarily lay down the following definition: *Those thermal states are to be regarded as the same in which bodies produce in one another no alterations of volume* (mechanical pressures, electric forces, and so on, excluded). This definition may be applied immediately to the thermoscope, which indicates the thermal state of the body it touches the moment mutual alteration of volume by contact ceases.

If two bodies A and B are, as the common phraseology goes, both as warm as, or both provoke the same sensations of heat as, a third body

C, then *A* is, in the same sense, just as warm as the body *B*. This is a logical necessity, and we are incapable of thinking it otherwise. The contrary would involve our holding two sensations to be at the same time alike and different. But we are not permitted by our definition to assume outright that if *A* and *B* both do not produce alterations of volume in *C*, *A* likewise will produce none in *B*. For this last result is an experience, whose outcome we have to await, and which is not involved in the two first-mentioned experiences. This is a simple consequence of the position above assumed.

But experience shows that if there be a series of bodies *A*, *B*, *C*, *D* . . . each of which has been sufficiently long in contact with that which follows, the thermoscope will give the same indication for each body. And, furthermore, we should be led into singular contradictions with our daily experience of heat were we to assume that the equality of the physical condition of *A* and *B*, and *B* and *C*, according to the above definition, did not likewise determine the equality of the physical condition of *A* and *C*. Inverting the order of the bodies, which now do not induce alterations of volume in one another, would result in new alterations of volume. But as far as our thermoscopic experience extends, this nowhere occurs.

To my knowledge, Maxwell is the first who drew attention to this point, and it may not be amiss to mention that Maxwell's considerations are quite similar to those which I advanced respecting the concept of mass.[1] It is extremely important to note that, whenever we impose a definition on Nature, we must wait and see whether she will accord with it. We may indeed frame our conceptions arbitrarily; but with the exception of pure mathematics we are bound, even in geometry and far more so in physics, to investigate minutely the extent to which realily conforms to our conceptions.

Any conception, therefore, of the experiences familiar to us, if it is to be free from contradiction, demands the assumption that two bodies *A* and *B* which are in the same thermal state as regards a third body *C* are in the same thermal state as regards each other.

4. The stronger the thermal sensation, the greater the volume of the thermoscopic substance. Hence again, by analogy, the following arbitrary definition may be set up: *Those thermal states are to be regarded as the more intense in which bodies produce in the thermoscope greater augmentations of volume.* By analogy with the thermal processes

observable by sensation, we should then expect that of two bodies A and B the one which produced in the thermoscope the greater augmentation of volume would on contact also induce, in the other, an augmentation of volume and, in itself, a diminution. But, while the analogy holds generally, it may lead us astray in special cases. Water furnishes an example of this. Two masses of water at +3 °C and +5 °C both show a diminution of volume on contact. Two masses of water at 10 °C and 15 °C present the normal case. Two masses at 1 °C and 3 °C present a case diametrically opposed to the analogy.

It will be seen from the foregoing that water as a thermoscopic substance could, under certain circumstances, give the same indication for two thermal states for which other thermoscopes would give different indications. The use of water as a thermoscopic substance, at least in the thermal field under consideration, is accordingly to be avoided.

5. Our sensations of heat, like thermoscopic volumes, form a simple series, a simple continuous manifold; but it does not follow from this that states of heat also form such a manifold. The properties of the system of symbols we employ are not decisive of the properties of the states symbolized. If we were to take, for example, as our criterion of the state of a body K the pull exerted by K on an iron ball E suspended from a balance, these pulls, the aggregate of which as symbols likewise constitute a simple manifold, could be determined indifferently by the electric, magnetic, and gravitational properties of K, and would be the symbolic correspondent of a threefold manifold. Investigation must determine in each case whether the symbolic system chosen is the appropriate one.

Let A, B, C, D, E be a series of bodies, of which each exhibits a lesser thermal state than that which follows (Fig. 28). As far as our experience goes, a body can be transported from the state of A to that of E only by way of the states B, C, D and the states intermediate to them. There is nothing in the domain of experience to suggest that this

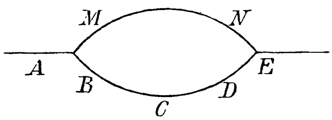


Fig. 28.

could also be effected through a succession of conditions *MN* situated outside the series *B*, *C*, *D*. The assumption of a simple continuous manifold of thermal states is sufficient.

6. It was remarked above that there was an arbitrary convention involved in the choice of *volume* as a thermoscopic index. There is a further arbitrary choice involved in adopting a thermoscopic *substance*. Yet if the substance selected were universally accepted, the resulting thermoscope would substantially accomplish everything that could be demanded of it. The thermoscope would be exposed to the greatest possible number of thermal states, established as invariable by cessation of change on the part of the thermoscope, and these points of cessation would be distinguished by marks and names; such as the freezing point of mercury, the melting point of ice, the congealing point of linseed-oil and aniseed-oil, the melting point of butter, blood-heat, the boiling point of water, the boiling point of mercury, and so on. These marks would then enable us not only to recognize a recurring state of heat, but also to reproduce a state already known to us. But in accomplishing this, the essential function of the thermoscope is achieved.

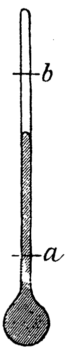


Fig. 29.

7. The inconveniences of such a system, which as a matter of fact long prevailed, would soon be manifest. The more delicate the inquiry, the more fixed points of this sort would be necessary; and ultimately they would no longer be attainable. Furthermore, the number of the names to be marked would be annoyingly augmented, and it would be impossible to discover from these names the order in which the thermal

states under consideration succeeded one another. This order would needs have to be specially noted.

But there exists a system of names which is at the same time a system of ordinal symbols, permitting of indefinite extension and refinement, viz, numbers. Substituting numbers for names as our designations of thermoscopic marks, all the inconveniences in question are eliminated. Numbers may be continued to infinity without effort; between two numbers any number of other numbers may be interpolated in an existing system; it is apparent immediately from the very nature of a number between what other numbers it lies. This could not escape the notice of the inventors of the early thermoscopes; and the idea was actually applied, though to varying extent and with varying appropriateness.

8. For the introduction of this more appropriate system, a new convention was necessary — a convention respecting the principle of coordination of numbers with the thermoscopic marks. And here new difficulties arise.

One of the methods used consisted in marking on the capillary tube of the thermoscopic container two fixed points (the melting point of ice and boiling point of water). The apparent voluminal increment of the thermometric substance (neglecting the dilatation of the vessel) was next divided into 100 parts (degrees), and this division was then continued beyond the boiling and melting points. By means of these fixed points and the principle of coordination referred to, every number appeared to be uniquely connected with a physically determined thermal state.

9. But this connection is immediately disturbed when some other thermoscopic substance or some other enclosing material is chosen. Laying off the volumes of any given substance as abscissae and erecting those of another in the same thermal states as ordinates, we obtain, according to Dulong and Petit, by joining the extremities of the ordinates, not a straight line but a curve similar to that pictured in Figure 30, and differing for every pair of substances. In point of fact, substances do not expand proportionally to one another when subjected to the same thermal changes, as we have already learned. Hence, on the same principle of coordination, sensibly different numbers are assigned to the same thermal states for each and every thermoscopic substance.

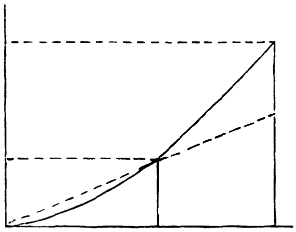


Fig. 30.

Even adopting mercury exclusively as our thermoscopic substance, the expansion of the glass of the containing vessel, which is not comparatively a vanishing quantity, exercises an appreciable influence upon the process of the apparent expansion; and this influence is peculiar to every different kind of glass. Therefore, even though the same principle of coordination be employed, strictly speaking the connection between numbers and thermal states is again peculiar to each thermoscope.

10. When attention was directed to the like behavior of gases under the same thermal conditions, the choice of a gas as a standard thermoscopic substance was, by reason of this property, regarded as less conventional and as having roots in the nature of things. But while it will appear that this opinion is erroneous, yet there are other reasons which make for this choice, which was a felicitous one though at the time it was made no one could have been aware of the fact.

One of the greatest advantages that gases offer is their remarkable expansibility and the consequent enhanced sensitiveness of the thermoscopes. Furthermore, the disturbing effect of the variable envelopes is very considerably reduced by this great expansibility. The expansion of mercury is only about seven times as great as that of glass. The expansion of the glass and the variation of this material find, therefore, very perceptible expression in the apparent expansion of the mercury. But the expansion of a gas is 146 times as great as that of glass.[2] Hence the expansion of the glass has only a very slight effect upon the apparent expansion of the gas, and the variations in the different kinds of glass a negligible effect. In the case of gas thermometers, therefore, when the fixed points and the principle of coordination have been determined, the connection between the numbers and the thermal states is far more exact than with any other thermoscope. The material of the container selected, or more briefly the individuality of the thermoscope, can have only a very inconsiderable influence upon this relationship; the thermoscopes are rendered in high degree comparable — a point which confirms the judgment of Dulong and Petit. We shall in what follows take an air-thermoscope as the basis of our inquiry.

11. That number which, conformably to any chosen principle of coordination, is coordinated with a volume indication of the thermoscope, and consequently uniquely with a state of heat, is called the temperature of that state. It will be usually denoted in what follows by t. The temperature numbers are dependent on the principle of coordination, $t = f(v)$, where v is the thermoscopic volume, and, consequently, for the same state of heat they will vary greatly according to the principle adopted.

12. It is instructive to note that different principles of coordination actually have been propounded, although only one has proved of actual practical scientific value and hence remained in use. One of these principles may be termed the Galilean. It makes the temperature numbers proportional to the real or apparent voluminal increments from a definite initial volume v_0, corresponding to a definite thermal state.

To the volume: $v_0, v_0(1 + \alpha), v_0(1 + 2\alpha), \ldots, v_0(1 + t\alpha)$,

corresponds

the temperature: $0, \quad 1, \quad 2, \ldots, \quad t$,

For α here we take the hundredth part of the coefficient of the volume increment from the melting point of ice to the boiling point of water (viz., 1/273), the temperature number 100 falling to the last-named point. The same principle admits of extension beyond the boiling and melting points, the temperature numbers in the latter case being reckoned negatively.

An entirely different principle of coordination is that of Dalton. It is as follows:

To the volume: $\dfrac{v_0}{(1.0179)^2}, \dfrac{v_0}{1.0179}, v_0, v_0 \times 1.0179, v_0 \times (1.0179)^2$,

corresponds

the temperature: $-20, \quad -10, \quad 0, \quad +10, \quad +20$

If we take, with Amontons and Lambert, the pressure of a mass of gas of constant volume as our thermoscopic index, and make the temperature number proportional to the pressure of the gas, we shall again have, strictly speaking, a different principle. But owing to the validity of the law of Mariotte and Gay-Lussac within wide limits, and the slight deviation of the pressure coefficient from the coefficient of volume expansion — facts which at the time this scale was proposed were only imperfectly known — it happens that the properties of Amontons's scale are not markedly different from those of Galileo's scale.

Calling p the pressure of a mass of gas of constant volume, p_0 the pressure at the melting point of ice, and k a constant, Amontons's principle of coordination is expressed by the equation $t = kp/p_0$. A second fundamental point is unnecessary on this scale.[3] Since p and p_0 depend in the same manner on the thermal states that v and v_0 do, the new scale has precisely the same properties as the old one. For $p = 0$, $t = 0$. Putting $k = 273$, the degrees assume their customary magnitude: for the melting point $t = 273$, for the boiling point $t = 373$. The new scale coincides absolutely with the old scale, if the zero point be placed on the melting point, and the temperature numbers downward be reckoned negatively.

13. The employment of the air thermometer involves, whether volumes or pressures be taken as the thermoscopic indicators, a definition of temperature. Starting from the equations $p = p_0(1 + \alpha t)$, or $v = v_0(1 + \alpha t)$, we arbitrarily posit that the temperature t shall be given by the equation

$$t = \frac{p - p_0}{\alpha p_0} \quad \text{or} \quad t = \frac{v - v_0}{\alpha v_0}$$

Amontons's temperature, which is called by way of distinction the "absolute temperature," and denoted by T, is defined by the equation

$$T = \frac{273p}{p_0};$$

and its relation with that first defined is indicated above.

14. It is remarkable how long a period elapsed before it definitely dawned upon inquirers that the designation of thermal states by

numbers rests on a convention. Thermal states exist in nature, but the conception of temperature exists only by virtue of our arbitrary *definition,* which might very well have taken another form. Yet until very recently inquirers in this field appear more or less unconsciously to have sought after a *natural* measure of temperature, a real temperature, a sort of Platonic Idea of temperature, of which the temperatures read from the thermometric scales were only the imperfect and inexact expression.

The conceptions *temperature* and *quantity of heat* were never kept clearly apart before Black and Lambert, and for both these ideas, between which we now distinguish, Richmann used the same word, "calor". At this stage, therefore, we cannot expect clearness. But the obscurity extends farther than we should have thought. Let us look at the facts.

Lambert[4] well characterized the state of opinion of his contemporaries when he said: "Inquirers doubted whether the *actual* degrees of heat were in reality proportional to the degrees of the expansion. And even granting that this were so, the further question arose as to the degree at which the counting should begin." He then discussed Renaldini's proposal to graduate thermometers by means of water mixtures, and he appears to have regarded this last scale as a natural one.

Dalton had the following passage:[5] "Liquids have been tried, and found to expand unequally, all of them expanding more in the higher temperatures than in the lower, but no two exactly alike. Mercury has appeared to have the least variation, or approach nearest to uniform expansion."

Gay-Lussac said:

> The thermometer, as it exists to-day, cannot indicate the exact relationships of *heat,* for we do not yet know what connection there is between the degrees of the thermometer and the quantities of heat which these degrees may indicate. It is generally believed, indeed, that the equal divisions of this scale represent equal tensions of the caloric; but this opinion is based on no very positive fact.[6]

Manifestly Gay-Lussac was in a fair way to overcome the obscurity of his contemporaries on this point, but he was nevertheless unsuccessful.

It is very singular that inquirers of the exactness of Dulong and Petit, who were the first to introduce clearness into this field, continually lapsed, in their expressions at least, to the old points of view. We read in one place:[7] "It will be seen, from the deviation that occurs at so low a temperature as 300°, how greatly the expansion of glass departs from uniformity." We ask in astonishment: "By what criterion is the 'uniformity' or 'lack of uniformity' of the expansion of glass to be estimated and measured?" The following passage is also characteristic:[8]

We are constrained to say, nevertheless, that the well-known uniformity in the principal physical properties of all gases, and especially the identity of their laws of dilatation, render it very probable that in this class of bodies the disturbing causes do not produce the same effects as in solids and liquids; and that consequently the changes of volume produced by the action of the heat are in the present instance more immediately dependent on the force that produces them.

This vacillation between a physical and a metaphysical point of view has not been entirely overcome, even to-day. In an excellent modern textbook by a distinguished inquirer in this field, we read: "The indications of the air thermometer are comparable. But it by no means follows from this that the air thermometer actually measures that which we conceive as temperature; it has, in fact, never been proved that the increase of the pressure of gases is proportional to the increase of the temperature, for hitherto we have only assumed this."

No less a man than Clausius has expressed himself as follows:

We may infer from certain properties of gases that the mutual attraction of their molecules is very weak at their mean distances and hence offers a very slight resistance to the expanson of the gases, so that it is the walls of the containing vessel that have to offset by their resistance nearly the entire effect of the action of the heat. The outward, sensible pressure of the gas, accordingly, forms an approximate measure of the dispersive force of the heat contained in the gas; and, therefore, conformably to the preceding law, this pressure must be approximately proportional to the absolute temperature. The correctness of this inference has, indeed, so much intrinsic probability that many physicists since Gay-Lussac and Dalton have assumed it outright, and based upon it their calculations (!) of the absolute temperature.[9]

In a valuable treatise on pyrometry we find the following:[10]

In view of Gay-Lussac's discovery, made as early as 1802, that all gases suffer, under the action of heat, like expansions for like increases of temperature, the hypothesis is well justified that the expansion in question is uniform for all degrees of temperature, inasmuch as it is more probable that the expansion should be uniform than that all gases should exhibit the same variability.

On the other hand, it is to be particularly noted, that W. Thomson, as early as 1848, in propounding his absolute thermodynamic scale of temperature, was very clear on this matter and went critically to the bottom of it, as we shall see in detail in a later chapter.

After these examples, the preceding exposition, however obvious it may appear to individual physicists, will not I trust be regarded as altogether redundant. We repeat, the question is always one of a scale of temperature that shall be universally comparable and that can be constructed with accuracy and certainty, and never one of a "real" or "natural" scale.

15. It could be easily shown, by analogous examples from other departments of physics, that men generally are inclined to hypostatise their abstract ideas, and to ascribe to them a reality outside consciousness. Plato, in his doctrine of Ideas, only made a somewhat free use of this tendency. Even inquirers of the rank of Newton, despite their principles, were not always careful enough in this respect; it will therefore repay the trouble to inquire upon what the procedure rests in the present case. We start in our investigations from the sensation of heat, and find ourselves later obliged to substitute for this original property of the behavior of bodies other properties. But between these properties, which differ according to circumstances, no exact parallelism obtains. For this very reason, latently and unconsciously, the original sensation of heat, which was replaced by these not exactly conforming properties, remains the nucleus about which our ideas cluster. Then, on our discovering that this sensation of heat is, in its turn, nothing but a symbol for the collective behavior of the body, which we already know and shall later know better,[11] our thinking compels us to group these varying phases of collective behavior under some single head and to designate them by a single symbol called state of heat. Scrutinising our procedure closely, we again discover as shadowy nucleus of the symbol this same sensation of heat, which is the initial and the most natural representative of the whole group of conceptions. And to this symbol, which is after all not entirely our arbitrary creation, we appear to be forced to attribute reality. Thus, the impression arises of an "actual temperature," of which that read from the thermoscope is only a more or less inexact expression.

Newton's ideas of "absolute time," "absolute space," etc., which I have discussed in another place,[12] originated in a quite similar manner. In our ideas of time the sensation of duration plays the same part with regard to the various measures of time as the sensation of heat played in the instance just mentioned.[13] The case is similar with respect to our ideas of space.

16. Once we have clearly comprehended that by the adoption of a new, arbitrarily fixed, more sensitive and more delicate criterion of the thermal state an entirely new point of view has been assumed, and that henceforward the new criterion alone is the basis of our investigations, the entire illusion will be dispelled. This new criterion, or mark, of the thermal state is the temperature number, or more briefly, the temperature, which reposes on an arbitrary convention in three respects — first with regard to the selection of volume as the indicator, secondly with regard to the thermoscopic substance employed, and thirdly with regard to the principle by which the numbers are coordinated with the volume.

17. An illusion of another sort is involved in a peculiar and almost universally accepted process of reasoning which we will now discuss. Taking the temperature-numbers as proportional to the pressure of a mass of gas at constant volume, it will be seen that while the pressures and the temperatures may increase without limit, they can never fall below zero.
The equation

$$p = p_0(1 + \alpha t)$$

asserts that for every degree increase of temperature the pressure increases by 1/273 of its amount at the point of melting ice; or rather, contrariwise, that when the pressure increases 1/273rd, we reckon the temperature one degree higher. For temperatures below the point of melting ice, we should have

$$p = p_0(1 - \alpha t),$$

from which it will be apparent that, if 1/273rds of the pressure p_0 be deducted 273 times, and the temperature −273 °C attained, the pressure will be zero. And one is inclined to think that when a gas has been cooled to this point it no longer contains any "heat"; that consequently any further cooling below this temperature is impossible; that, in other words, the thermal states have apparently no upper limit, but possess a lower limit at −273 °C.

The principle of coordination employed by Dalton[14] did not remain in use, but not the slightest objection can be made to its admissibility. On this principle, when the pressure of the gas increases by 1.0179, the temperature increases ten Daltonian degrees. When the pressure diminishes by 1.0179, the temperature sinks ten degrees. We can repeat this last operation as often as we wish without ever reaching a pressure zero. If Dalton's scale were used, the idea need never have occurred to us that a thermal state could exist which had the gas pressure zero — that the series of thermal states had a lower limit. The possibility of a gas pressure zero would not, indeed, have been affected by this fact, for the reason why Dalton does not reach the lower limit is that he moves toward it, like Achilles toward his tortoise in the famous paradox, with steps of diminishing magnitude. The essential point to be emphasized here is the precariousness of regarding outright the properties of a system of symbols as the properties of the things symbolized by them.

18. Amontons, in propounding his scale of temperature, started from the idea that the pressure of a gas is produced by "heat". But his absolute zero-point is not the only one that has been proposed, nor is it the only one that could be proposed on the ground of equally sound ideas. Taking the coefficient of expansion of mercury, and pursuing the same train of reasoning as with air, we should obtain −5000 °C as our absolute zero. As with air and with every other body, so likewise here with mercury, the coefficient of pressure might be employed instead of the coefficient of expansion, in order to eliminate the distressing idea of a body losing its volume when it loses its heat.

Dalton's[15] idea was that a body contains a certain quantity of caloric. Increasing the caloric raises the temperature; withdrawing it altogether reduces the body to the absolute zero-point. This idea of heat as a substance (caloric) was derived from Black, although Black was no friend of such conjectures as we are now discussing. If ice at 0 °C is converted into water at 0 °C, and for every kilogram in this process eighty kilogram-calories are absorbed, Gadolin[16] and Dalton contended that, owing to the doubling of the capacity for heat by the liquefaction of the water, the entire loss of caloric from the absolute zero-point to 0 °C is compensated for by the eighty thermal units in question. Whence it follows that the absolute zero-point lies at $2 \times 80 = 160$ °C below the melting point of ice. The same zero-point is obtained, by the same reasoning, for many other bodies. But for mercury, which has a low melting point and which exhibits a very slight difference of specific heat in its solid and liquid conditions, 2021 °C below the melting point of ice is obtained as the absolute zero. If two bodies, A and B, of like temperature be mixed together, and the mixture $A + B$ shows an alteration of temperature, we can in an analogous manner, after determining the specific heats of A and B and $A + B$, deduce the absolute zero-point from the change in the temperature. By mixing water and sulphuric acid, Gadolin found the absolute zero-point to lie between −830 °C and −1720 °C. Other mixtures, and also chemical combinations, have been similarly treated, and have again yielded different results.

19. We have thus a multitude of different absolute zeros. To-day only one of these is in use, that of Amontons which, in accordance with the dynamic theory of gases, has been connected with the nullified velocity of the gas molecules. But all these deductions alike rest on hypotheses regarding the processes by which we imagine the phenomena of heat to be produced. Whatever value we may attribute to these hypothetical ideas, we must yet admit that they are unproved and unprovable, and cannot antecedently determine facts which may at some time be rendered amenable to observation.

20. We now revert to the point which we were discussing. The pressure of gases are signs of the thermal states. When the pressures vanish, the signs likewise vanish; our gas is rendered unserviceable as a thermoscope and we must seek another. That the thing symbolized also disappears does not at all follow. For example, if a thermo-electromotive force, on approaching a certain high temperature, should diminish or become zero, it would doubtless be thought extremely rash were this temperature to be regarded as indicating an upper limit of the states of heat.

The temperature numbers, again, are symbols of the symbols. From the fact that our fortuitously chosen system of symbols has a limit, nothing whatever follows as to the limits of the thing symbolized. I may represent sensations of tone by rates of vibration. These rates as positive numbers, have a lower limit at zero, but no upper limit. I may also represent sensations of tone by the logarithms of the rates of vibration, and obtain a much better image of the musical intervals. In which case, my system of symbols (running, as they do, from $-\infty$ to $+\infty$) has neither a lower nor an upper limit. But the system of tone-sensations is not a whit disturbed by this: it has both an upper and a lower limit. I may define an infinitely high or an infinitely low tone by my system of symbols, but it in no wise follows from this that such a tone exists.

The entire train of reasoning reminds one vividly of the so-called ontological proof of the existence of God; it is scholastic to a degree. A concept is defined, and existence is among its attributes; whence follows forthwith the existence of what has been defined. It will scarcely be gainsaid that a similar logical looseness is not permissible in modern physics.

We may accordingly assert that, even granting that it were possible by cooling a gas to reduce its pressure to zero, this result would simply prove the unfitness of gases as thermoscopic substances from this point downward. But that the thermal states have or have not a lower limit would in no wise follow from it.

And, similarly, nothing follows as to an upper limit for thermal states from the fact that the pressure of a gas may be imagined to increase without limit, or from the fact that the numbers expressing the temperatures have no upper limit. A body melts and boils at certain temperatures. And the question arises whether a gas can attain indefinitely high temperatures without suffering important alterations of character.

21. *Experience alone can determine whether the series of thermal states has a lower or an upper limit. Given a body of definite thermal conditions and supposing no other can be produced that is hotter or colder than it, then and then only can such a limit be established.*

The view here taken does not exclude our conceding to Amontons's zero the role of a fiction, or our investing the law of Mariotte and Gay-Lussac with the simple expression before referred to,[17] whereby many discussions to be later developed are very materially simplified.

22. From the foregoing it will be readily seen that temperature is nothing but the characterization or designation of a thermal state by a number. This temperature number has exclusively the properties of an inventory number, by means of which the same thermal state can again be recognized, and if necessary sought for and reproduced. This number likewise informs us in what order the designated thermal states succeed one another and between what other states a given state is situated. In the investigations to follow, it will appear that the tempera-

ture number fulfils still other, and indeed extremely comprehensive functions. But this was not due to the acumen of the physicists who propounded the system of temperature numbers, but was the outcome of several fortunate circumstances which no one could foresee and no one control.

23. The conception of temperature is a conception of level, like the height of a heavy body, the velocity of a moving mass, electric and magnetic potential, and chemical difference. Thermal action takes place between bodies of different temperature, as electric action does between bodies of different potential. But, whilst the conception of potential was deliberately framed in perfect consciousness of its advantages, in the case of the conception of temperature these advantages were a matter of good luck and accident.

In most departments of physics the differences alone of the level values play a determinative part. But temperature appears to share, in common with chemical level, the property that its level values are *per se* determinative. The fixed melting points, boiling points, critical temperatures, temperatures of combustion and dissociation, are obvious instances.

CHAPTER III

ON THE DETERMINATION OF HIGH TEMPERATURES

1. Reference must here be made, in connection with our discussions of the conception of temperature, to "pyrometric" methods or means of determining high temperatures. Newton[1] was the first to devise a method of this kind, and we shall simply state his ideas without at present making any critical comment.

Newton observed, by the aid of a linseed-oil thermometer, that the loss of temperature of a hot body exposed to a uniform current of air was, for the same interval of time, proportional to the difference of temperature between the body and the air; and he assumed that this relation held universally for all temperatures, however high. Imagine two bodies, A and A', alike in all respects, save that the difference between the temperature of the air and that of A' is twice the corresponding difference for the air and A. Allowing these bodies to cool during the same element of time τ_1, A' will lose twice as much as A, and the excess of its temperature above that of the air will, at the end of time τ_1 be again twice that of A. The same reasoning holds true for the succeeding element τ_2, and so for the rest. Hence, in the process of cooling during any interval of time t, A' will lose twice as much as A. The generalization is obvious.

Now let a body A at a very high temperature cool, and call the equal intervals into which the total time of cooling is divided, $t_1, t_2, \ldots t_{n-1}, t_n$. Suppose the excess of temperature of the body at the beginning of the last interval t_n is $2u$, but at the end of it is u, then, on the preceding assumption, it follows that, at the beginning of the equal intervals t_{n-1}, $t_{n-2}, t_{n-3} \ldots$, it would show respectively the excesses of temperature, $4u = 2^2u$, $8u = 2^3u$, $16u = 2^4u$. Newton ascertained the time t_n and the value of u by means of a linseed-oil thermometer, and was thus able to assign the temperature at every other prior period of the cooling.

The body A was a red-hot mass of iron exposed to a current of air. On it particles of different metals and their alloys were placed and the time noted at which they congealed, the idea being to determine the temperatures of congelation. From the melting point of tin downwards the process of cooling could be following with a linseed-oil thermometer. Newton made the temperature numbers of this thermometer proportional to the volume increment of the linseed-oil above the melting point of ice.

According to Newton, the temperature of boiling water is not quite three times (2.83) that of the human blood (37 °C), whence 104 °C would follow for the temperature of boiling. For the melting point of tin (5.83 × 37) he obtained 215 °C (new researches give 230°); for the temperature of lead (8 × 37) he got 296° (new determinations give 326°), and for the temperature of red-heat (16.25 × 37), 600 °C.

At the conclusion of his paper Newton remarked that, owing to the uniformity of the air current, the same number of air particles was heated in equal intervals of time, by an amount proportional to the heat of the iron, and that therefore the losses of heat suffered by the iron must be proportional to its heat. But, since these losses are in point of fact also proportional to the indications of the linseed-oil thermometer, therefore we are justified in assuming that the heat of a body is proportional to the increase of volume of the linseed-oil thermometer.[2] From this reasoning, in which by the way no distinction was made between the conceptions "temperature" and "quantity of heat," it would appear that Newton, here as elsewhere, is guided in his enunciations party by instinct and partly by observation, making the suggestions of the one correct those of the other. It appeared to him antecedently obvious that the "losses of the heat" should be proportional to the "heat", and likewise that the "expansion" should be proportional to the "heat." Observation tallied with these views, and so the conceptions were retained.

2. Critically viewed, matters stand as follows. The temperature numbers are based on an arbitrary convention. They may be taken proportional to the volume increments or they may not. But after a decision regarding them has been reached, observation alone can decide whether the losses are proportional to the temperatures. On the other hand, the temperature numbers could be so chosen that the losses would be proportional to the temperatures, even on the assumption of some different law of cooling from that actually obtaining.

There is thus no necessary connection between Newton's propositions. Nothing whatever follows from his observations regarding the correctness or incorrectness of his scale of temperature. Dulong and Petit have in fact shown, as we shall see later, that the harmony between

Newton's assertions is immediately ruptured if the observations on cooling are made with a thermometer within somewhat wider limits of temperature and with greater precision than Newton bestowed upon them. Newton's two assumptions contain, so to speak, two different scales of temperature.

But nothing would prevent our employing Newton's pyrometric principle as a definition of a scale of temperature, by considering on some principle of co-ordination the times counted backwards as inventorial numbers of the corresponding thermal states of the cooling body. Whether this definition is or is not independent of the nature of the bodies, and what is the relation of the this scale to any other now in common use, could be ascertained only by special experiments and only to the extent to which the two scales under comparison were actually and simultaneously accessible (without extrapolation) to experiment.

3. Another pyrometric method, early devised by Amontons[3] in imperfect form, was employed by Biot. Biot[4] showed, by experiment and by theoretical considerations, that in a very long metal bar one end of which has been exposed sufficiently long to a constant source of heat, the excesses of the temperature of the bar over that of the air decrease in geometrical progression as we move away in arithmetical progression from the heated end — as far at least as the process can be followed with a thermometer. Ascertaining the ratio of the progression at the colder end and assuming that the law holds without limit for all temperatures, however high, we can infer the temperatures of the places which, by reason of their great heat, are inaccessible to direct thermometric examination. Amontons had assumed that the temperatures increased from the cold to the hot end according to the law of a straight line. But, since the ratio of the above-mentioned progression depends on the dimensions and the material of the bar, it will be seen that the temperature numbers obtained by Amontons' principle would depart very considerably from those obtained by Biot's. Examining Biot's case in wider ranges of temperature and with greater exactness, as Forbes[5] has recently done, it appears that even within the limits accessible to a thermometer the ratio of the geometrical progression depends on the temperature. Thus Biot's pyrometric principle also, if it is to be consistently maintained, involves a new definition of temperature; and what was said regarding Newton's principle holds true substantially regarding Biot's. As for the rest, the relation between the two methods is simple.

In Newton's method the temperatures to be determined succeed one another, in Biot's they occur side by side. The temperature numbers employed as inventorial numbers are obtained in the first instance as measures of time and in the second as measures of length. Newton's idea may have suggested Biot's. Lambert[6] had already corrected Amontons' principle after the manner of Biot[7].

4. Black also devised a pyrometric method, based on his researches in calorimetry. If a body of mass m be cooled in a quantity of water M from the temperature u_1 to the temperature u, then, as thermometric observation shows, the water M will be heated by an amount proportional to the product $ms(u_1 - u)$, where s is a constant peculiar to the cooled body (viz., its specific heat). If M be the mass of the water and u_2 its initial temperature, the equation

$$ms(u_1 - u) = M(u - u_2)$$

subsists, and from this follows, for the initial temperature u_1 of the cooled body,

$$u_1 = u + \frac{M(u - u_2)}{ms}.$$

If m and s be small and M large, u and u_2 will remain within reach of the ordinary thermometric scale, even when the body to be cooled has been heated to a degree far beyond it. Assuming with Black the unlimited validity of the principle, the initial temperature u_1 can still as ascertained from the above equation. For example, we can cool in a large mass of water a piece of iron of known weight and specific heat which has been taken from a furnace, and ascertain in this way the temperature of the furnace. Inasmuch as the careful inquiries of Dulong and Petit have demonstrated that s depends on the temperature even within the limits of the ordinary scale, and since any investigation of s outside the limits of this scale is impossible, it will be seen that Black's pyrometric principle also involves a new definition of temperature. Substantially the same remarks may be made with respect to this method as were advanced regarding the methods discussed above.

5. A pyrometric method can be constructed on the basic of any physical property which varies with the thermal state. Pyrometers have been devised that rest on variations of volume or pressure; and others

have been conceived which indicate the thermal state by melting, boiling, dissociation, and alterations of viscosity. The spectral photometer and the polaristrobometer have also been put to pyrometric use. Acoustic pyrometers are based on the changes in the pitch and the wavelength of a note with the temperature. Finally, change of magnetic moment has been thought of in connection with temperature, and attempts have been made to put to pyrometric use the dependence of electric resistance on the temperature, as well as the alteration of thermo-electromotive force with the temperature. The writings of Weinhold[8], Bolz[9], Holborn and Wien[10], as well as the more recent work of Barus[11], contain explicit information on all these points, including a full bibliography.

After the foregoing, there will be no doubt that each individual pyrometric method simply furnishes an indication of a thermal state by means of which that state can again be recognized and reproduced. For many practical purposes this is in itself very valuable and is often quite sufficient. The number which is the result of any pyrometric observation has no other significance than that of an inventorial number. If from three observations we obtain three numbers, $a < b < c$, all the information that these numbers furnish is that the thermal state to which b belongs lies between the two states to which a and c belong. It is antecedently unreasonable to expect any agreement between the numbers obtained by the different pyrometric methods, for the reason that in general every pyrometric method involves a special definition of temperature. The reduction of pyrometric numbers to the Celsius scale can only be performed to the extent within which this method can be employed simultaneously with the air thermometer. Reductions of this kind have been attempted by Weinhold, Holborn and Wien, to mention only the most important.[12] Sir William Siemens[13] spoke of the calculations of the temperature of the sun which were made by Secchi, Zöllner, and others, and which amounted respectively to 10,000,000 °C and 27,700 °C. Apart from the objections which may be raised against the premisses of this calculation and the methods of computation, it is to be remarked that indications in degrees Celsius far outside the possible limits of employing the air thermometer have absolutely no meaning whatever.

CHAPTER IV

NAMES AND NUMBERS

1. A domain of knowledge like physics is possessed, in its control of experiences, of a constant and efficacious means of refining its doctrines. After the results of the foregoing investigations psychological analyses, and logical analyses which are founded on them, will not be considered as quite superfluous even in this domain. We will, then, now discuss some questions particularly of the latter kind which, treated at length, would have only disturbed the connection of the previous inquiry. The significance of names and numbers — what they have in common, and in what way they differ from one another — has made itself felt in our consideration of thermometric scales. What are names? What are numbers?

2. A name is an acoustic attribute, which I add to the other sensory attributes of a thing or complex of phenomena, and which I engrave in my memory. Even in themselves alone, names are important. Of all the attributes of a complex of phenomena, they are the most invariable. They constitute thus the most convenient representative of that complex as an entirety, and around them the remaining and more or less variable attributes cluster in memory as around a nucleus.

But the facility with which these attributes called names permit of being transferred and communicated is more important still. Each observer may discover different attributes in a thing; one person will notice this, another will notice that — with the result that they will not necessarily come to an understanding regarding the thing, or for that matter even be capable of coming to an understanding. But the name, which always remains the same, is imprinted as a common attribute in the memories of all persons. It is like a label that has been attached to a thing and is understood by all. It is not only attached to things; it is preserved in the memories of men and leaps forth at the sight of these things, of its own accord.

3. The importance of names in technical fields has never been a subject of doubt. The possibility of procuring things which are not

within our immediate reach, and of producing effects at a distance through a chain of human beings, is attributable to names. The ethical achievements of names are perhaps even more important still. Names particularize individuals; they create personalities. Without names there is neither glory nor disgrace; neither defensible personal rights, nor prosecutable crime. And, by the use of written names, all this has been enhanced to a stupendous degree.

When two persons part company, each soon shrinks for the other to a mere perspective point. Without names it would be almost impossible for the one to find the other. The fact that we know more of some men than of others, that some men mean more to us than others, is owing to names. Without names we should be strangers to one another, as are animals.

Fancy for a moment how I should be obliged to mimic, caricature, and portray a person whom I was seeking, in order that some small group of people, who were perfectly familiar with my methods, could assist me in my search. But if I know that the name of the person I am seeking is F.M. and he lives in France, and in addition in Paris, at No. 45, Rue S., then I am always in a position to find him by means of these names — names which countless numbers of different individuals associate with the same objects, although they may know these objects under entirely different aspects and in greatly varying degree, sometimes by name only. I can thoroughly appreciate the marvelous achievement involved in these performances by imagining myself making such a search without a knowledge of names. I should then have to travel from country to country and from city to city, like the people in the *Arabian Nights,* until I found by accident the person whom I was seeking — which happens only in fairy tales. I should be in the situation of the lost child who could tell no more than that she belonged to "Mother" who "lived at home".

A name is the product of a convention, reached without our volition under the favoring influence of accident, by a limited circle of people having common interests, and gradually communicated by that circle to wider groups.

This significance of names in the narrowest professional domain is point by point illustrated by what we have said about the establishment of the thermometric scale.

4. What are numbers? Numbers are also names. Numbers would never

have originated had we possessed the capability of picturing with absolute distinctness to ourselves the members of a set of like objects as different. We count where we desire to make a distinction between a set of homogeneous things; in doing so, we assign to each of the like things a name, a distinguishing sign. If the distinction to be made between the things is not effected, we have "miscounted". To accomplish our purpose, the signs employed must be better known and must admit more readily of distinction than the things to be designated. Counting, accordingly, begins with the corrclation of the familiar objects known as fingers, the names of which have in this manner gradually come to be the names of numbers.[1] The correspondence of the fingers with the things is accomplished, without effort or design, in a definite *order.* In this manner, numbers are transformed quite without our volition into *ordinal symbols.*[2] As a consequence of this invariable order, and as a consequence of it alone, the last sign associated with the things comes to represent all the previous correspondences; this last sign is the *number* (*Anzahl*), of the things counted.[2]

If there are not enough fingers to associate with the things, the original series of correspondences is simply repeated, and the several series of correspondences so obtained are then themselves supplied with ordinal symbols, as before. Our system of numbers becomes in this manner a system a purely ordinal signs, which can be extended at pleasure. If the objects counted have distinguishable homogeneous parts, and in each of these parts there be discovered parts which again are alike, and so on, the same principle may be employed for the enumeration of these parts of parts. Our system of ordinal signs, accordingly, admits of indefinite refinement. Numbers are an orderly system of *names* which admit directly and readily of indefinite extension and refinement.

5. Where a few objects only are to be designated, and these are readily distinguished from one another by salient attributes, proper names as a rule are preferred; countries, cities, friends, are not numbered. But objects that are numerous and which constitute in any way a system in which the properties of the individual things forming the system constitute a gradation, are always numbered. Thus numbers and not names are given to the houses of a street; and, in regularly laid out cities, also to the streets themselves. Degrees on a thermometer are numbered, and proper names are given to the freezing and boiling points only. The

advantage here, in addition to the mnemotechnic feature of the plan, consists in the fact that one can easily discover by the sign of the thing the position which it occupies in the system — an advantage not appreciated by the inhabitants of small towns, where the houses are unnumbered and where there are consequently no municipal coordinates to assists a stranger in finding his way.

6. The operation of counting may again be applied to the numbers themselves; in this manner, not only is the development of the number system carried to a point considerably beyond that of its original simplicity, as by the formation of the decimal system of writing and of performing operations with numbers, but the entire science of arithmetic, even the entire science of mathematics, arises from this application. The perception, for example, that $4+3=7$, arises from the application of the ordinal signs or numbers of the upper horizontal row of the following diagram to the numbers of the row which is beneath:

$$\begin{array}{ccccccc} 1 & 2 & 3 & 4 & 5 & 6 & 7 \\ 1 & 2 & 3 & 4 & 1 & 2 & 3 \end{array}$$

I regard the truths of arithmetic to be propositions that have been reached by experience, understanding by experience here inner experience; and I long ago characterized mathematics as a system of economically ordered experience of counting, made ready for immediate use, and designed to replace direct counting, which is frequently impossible, by operations previously performed, and hence accomplishing a great saving of time and trouble.[4] My views are here substantially in accord with those which Helmholtz expressed in 1887.[5] This is of course not as yet a theory of mathematics, but merely a program of such a theory. What interesting psychological question are presented here may be seen from the work of E. Schröder[6] who was the first to inquire why the number of the objects is independent of the order in which they are counted. As Helmholtz remarked[7], in any succession of objects that have been counted in a definite order any two adjacent objects may be interchanged, whereby ultimately any order of succession whatever of the objects may be produced without changing the succession of the numbers, or causing either objects or numbers to be dropped. The nondependence of the sum on the order of the things added follows from this consideration. But this inquiry cannot be pursued further here.

7. Although, in the first instance, counting supplies the necessary means of distinguishing objects which are in themselves difficult to distinguish, it is nevertheless afterwards applied to objects which, while clearly distinguishable, are yet in some certain respect regarded by us as the same, and so are interchangeable in this respect. The properties with respect to which objects may be considered to be the same differ greatly, and vary almost from mere existence at a given point of space or moment of time to absolute undistinguishability. We count different objects as the same only in so far as they are like; francs, marks, and gulden are counted, not as such, but as coins. Thermometers and induction coils are counted as physical apparatus, or as items of an inventory, but not together *as* thermometers and induction coils.

8. Objects counted, which are alike in some particular respect, and which may replace one another in this respect[8], are called units. What is it that is counted, for example, by the number representing a temperature? In the first place it is the divisions of the scale, the real or apparent increments of volume or of tension of the thermometric substance. Geometrically or dynamically regarded, the objects here counted may be substituted for one another indifferently; but with reference to the thermal state these object are signs or indices merely of that state, and not equivalent, enumerable parts of a universal property of the thermal state itself.

This becomes clear at once when we consider that the number measuring a potential, for example, does quantitatively determine a universal property of the potential. If I cause the electric potential of a charged body to sink from 51 to 50 or from 31 to 30, I am able by so doing to raise the charge of any other having the same capacity one degree, indifferently whether it be from 10 to 11 or from 24 to 25. Different single degrees of potential may be substituted for one another.

A relation of like simplicity does not exist for scales of temperature. A thermometer is raised approximately one degree of temperature when some other thermometer of the same capacity is lowered one degree of temperature in some other part of the scale. But this relation is not exact; the deviations vary with the thermometric substance selected for either one or both thermometers, and with the position of the degrees in the scale; the deviations are furthermore individual in character, according to the substance and to the position in the thermometric scale; they are vanishingly small only in the gas scale. We may say that by cooling off a gas thermometer one degree in any part of the scale, any other body may be made to receive always the same alteration of thermal state. This property might have served as a definition of equal degrees of temperature. Yet it is worthy of remark that this property is not shared by all bodies whatsoever that pass through the course of temperature changes indicated by the gas thermometer, for the reason that their specific heat is in general dependent upon the temperature. It should also be noted that this principle was not intentionally embodied in the construction of the temperature scale, but was shown incidentally later to be approximately fulfilled. The conscious and rational introduction of a scale of temperature having universal validity analogous to the potential scale was first made by Sir William Thomson (Lord Kelvin): of this we shall speak later. The temperature numbers of the common scale are virtually inventorial numbers of the thermal states.

CHAPTER V

THE CONTINUUM

By a "continuum" is understood a system or manifold of terms possessed in varying degree of one or many properties A in such a way that, between any two terms which show a finite difference with respect to A, an infinite number of other terms may be interpolated, of which those that are immediately adjacent to one another exhibit only infinitely small differences with respect to the property A.

There can be no objection to such a system, considered as a fiction merely, or as a purely arbitrary ideal construct. But the natural scientist, who is not exclusively concerned with the purely mathematical point of view, is compelled to inquire whether there is anything in nature that corresponds to such a fiction. Space, viewed in its simplest form as a succession of points in a straight line; time; the succession of the elements of a uniformly sounding musical note; and the succession of colors shown by the spectrum with the Fraunhofer lines blotted out, are typical instances of the continua presented in nature. If we consider such a "continuum" without prejudice, it will be seen that there is nothing perceptible by the senses corresponding to an infinite number of terms or to infinitely minute differences. All we may say is that, in traversing such a succession, the distinguishability between the terms increases, as the terms move away from each other, until ultimately this distinguishability admits of not the slightest doubt; and again, that, as the terms approach each other, the distinguishability decreases, it becomes alternately possible and impossible to distinguish them, according to chance and circumstances; and finally it is altogether impossible to do so. *Points* of space and time do not exist for sense-perception; for there exist only spaces and times so small as not to admit of more minute division perceptible to the senses, or so small that we voluntarily neglect their size, although on increased attention they might admit of resolution into component elements. The possibility of a property A passing imperceptibly and uninterruptedly to a property A' clearly distinguishable from A is the important point. The fact is that any two terms, on a given trial, are either distinguishable or indistinguishable.

It is possible to remove a large number of terms from a given sensory continuum and the system will still give the impression of a continuum. If we imagine a large number of narrow equidistant bands of color cut out of a spectrum, and the remainder pushed together until the parts touch, the spectrum will still give the impression of a color continuum, in spite of the interruption of continuity in the wave-lengths of the lines. In like manner, an ascending musical note, if the intervals between the rates of vibration be sufficiently small, may be regarded as a continuum, and the jolting movement produced by a sufficiently large number of successive but detached stroboscopic or kinematographic pictures may also be made to appear as a continuous movement.

If the terms of a sensory continuum stood forth as individual entities and were distinguishable with absolute clearness, the employment of artificial expedients, such as the use of measuring rods for comparing homogeneous continua of the same kind and the use of dividing lines for rendering imperceptible differences of space distinct by means of conspicuous differences in color, and so on, would be superfluous. But the moment we introduce such artifices as being superior physically for the indication of the differences, we abandon the domain of immediate sense-perception, and pursue a course in every respect similar to that of substituting the thermometer for the sensation of heat. All the observations made there for the special case can be applied here to the general one. A distance in which the measure is contained twice or three times, is then twice or three times that in which it is contained once; and the hundredth part of the measure corresponds to a hundredth part of the difference, although it may not be said that this holds good for direct perception. With the introduction of the measure, a new definition of distance or difference has been introduced. Judgments of difference are now no longer formed from simple sense-perception, but are reached by the more complex reaction involved in the application of the measure; and the result depends upon the issue of the experiential test. The attention of that still large body of learned people who refuse to admit that the fundamental propositions of geometry are the results of experience — results not given by direct perception when metrical conceptions are introduced — may profitably be called to the consideration last mentioned.

The employment of measures suggests the employment of numbers, but the use of numbers is not necessarily entailed until it is resolved to employ only one measure, which is multiplied or subdivided according as the necessity arises for a larger or smaller continuum of comparison. In using a measure divided into absolutely equal parts, we are immediately enabled to employ all the numerical experiences of counting which we have gained from our study of discrete objects. This is not the place for a detailed discussion of the manner in which operations of counting themselves gave rise to the necessity of new numerical conceptions far transcending the bounds of the original system of integral positive numbers, and of the gradual manner in which negative and fractional numbers, and finally the entire system of rational numbers, came into being.

If a unit is to be divided, it must either exhibit natural parts for such a division, as for example many fruits do, or it must at least permit of being conceived as made up of perfectly homogeneous equivalent parts. The early appearance of unit fractions is a probable indication that division was learned by experiences of the first-mentioned kind, and that the skill acquired in that field was carried over to cases of the second class, namely, to the division of continua. It is here apparent from the simplest instances that the number system which originated from the consideration of discrete objects is inadequate for the representation of fluent or continuous states. For instance, the common fraction 1/3 is equal to 0.333333 . . . A point of trisection, in other words, can never be found exactly by decimal subdivision, however minute. The ratios of certain line segments, as that of the diagonal to the side of the square, are absolutely unrepresentable by rational numbers, as Pythagoras long ago discovered,[1] and lead immediately to the concept of the irrational.[2]

The cases of this are innumerable. It may be expressed by saying that "the straight line is infinitely richer in point individuals than the domain of rational members is in number individuals."[3] But the remark is applicable, as the illustration given above of the point of trisection shows, quite irrespective of the irrational feature, to every special number system. We might say 1/3 is a relative irrational number, as compared with the decimal system.

Numbers, which were originally created for dealing with discrete objects, accordingly prove themselves to be inadequate for treating continua which are conceived as inexhaustible, be these real or fictitious. Zeno's assertion of the impossibility of motion on account of the infinite number of the points that had to be traversed between the initial and terminal stations, was admirably refuted in this sense by

Aristotle, who remarked that "a moving object does not count as it moves".[4] The idea that we are obliged to exhaust all things by counting is due to the inappropriate employment of a method which, for a great many cases, is quite appropriate. A pathological phenomenon of what might be called the count-mania actually makes its appearance here. No one will be inclined to discover a problem in the fact that the series of natural numbers can be continued upwards as far as we please, and consequently can never by completed; and it is not a whit more necessary to discover a problem in the fact that the division of a number into smaller and smaller parts can be continued *ad libitum* and consequently never completed.

At the time of the founding of the infinitesimal calculus, and even in the subsequent period, people were much occupied with paradoxes of this character. A difficulty was found in the fact that the expression for a differential was never exact, save when the differential had become infinitely small — a limit which could never be reached. The sum of non-infinitely small elements, it was thus thought, could give only an approximately correct result. It was sought to resolve this difficulty in all sorts of ways. But the actual practical uses to which the infinitesimal calculus is put are totally different from what is here assumed, as the simplest example will show, and are affected in no wise whatever by the imaginary difficulty in question.

If $y = x^m$, I find for an increment dx of x the increment

$$dy = mx^{m-1} \cdot dx + \frac{m(m-1)}{1 \cdot 2} x^{m-2} \cdot dx^2 + \\ + \frac{m(m-1)(m-2)}{1 \cdot 2 \cdot 3} x^{m-3} \cdot dx^3 + \ldots$$

Having this result, it will be seen that the function x^m reacts in a definite manner in response to a definite operation, namely, that of differentiation. This reaction is a characteristic mark of x^m, and stands on precisely the same footing as the bluish-green coloring which arises from dissolving copper in sulphuric acid. The number of terms that remain in the series is in itself indifferent. But the reaction is simplified by taking dx so small that the subsequent terms vanish in comparison with the first. It is on account of this simplification only that dx is considered very small.

In a curve with the ordinate $z = mx^{m-1}$, it is seen that on increasing

x by dx, the quadrature of the curve is increased by a small amount of surface, the expression for which, when dx is very small, is simplified by reduction to the form $mx^{m-1}\,dx$. In response to the same operation as before, and under the same simplifying circumstances, the quadrature reacts as the familiar function x^m reacts. We recognize the function, thus, by its reaction.

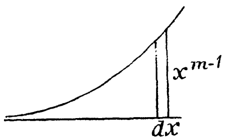


Fig. 31.

If the mode in which the quadrature reacted did not accord with the mode of reaction of any function known to us, the entire method would leave us in the lurch. We should then have to resort to mechanical quadratures; we should actually be compelled to put up with finite elements; we should have to sum up finite numbers of these elements, and in such an event the result would be *really inexact*.

The twofold *salto mortale* from the finite to the infinitely small, and back again from this to the finite, is accordingly nowhere actually performed; on the contrary, the situation here is quite similar to that in every other domain of research. Acquaintance with mathematical and geometrical facts is acquired by actual working with those facts. These facts, on making their appearance again, are recognized; and, when they appear in part only, they are completed in thought, in so far as they are uniquely determined.[5]

The manner in which the conception of a continuum has arisen will now be clear. In a sensory system, the parts of which exhibit flowing characteristics not readily admitting of distinction, we cannot retain the single parts either in the senses or in the imagination with any certainty. To be able to recognize definitely, therefore, the relations obtaining between the parts of such systems, we have to employ artificial devices such as measures. The mode of action of the measures is then substituted for the mode of action of the senses. Immediate contact with the system is lost by this procedure; and, furthermore, since the technique of measurement is founded on the technique of counting, numbers are substituted for the measures precisely as the measures were substituted for direct sense-perception. After we have once performed the operation of dividing a unit into component parts, and after we have once noticed that the parts exhibit the same properties as the original unit, then no obstacle presents itself to our continuing in thought to infinity the subdivision of the number which stands for the measure. But in doing so, we imagine that we have also divided both the measure and system that is measured to infinity. And this leads us to the notion of a continuum having the properties which we specified at the beginning of this chapter.

But it is not permissible to assume that everything that can be done with a sign or a number can also be done with the thing designated by that sign or number. Think of the considerations advanced in the above criticism of the conception of temperature. Admitting that the number which is employed to specify a distance can be divided to infinity without any possibility whatever of meeting with obstacles, still the possibility of such division by no means necessarily applies to the distance itself. There is nothing that presents the appearance of a continuum but may still be composed of discrete elements, provided only those elements be sufficiently small as compared with out smallest practically applicable measures, or provided only they be sufficiently numerous.

Wherever we imagine we discover a continuum, all we can say is that we can institute the same observations with respect to the smallest observable parts of the system in question as we can in the case of larger systems, and that we observe that the behavior of those parts is quite similar to that of the parts of larger systems. The length to which these observations may be carried can be decided by experience only. Where experience raises no protest, we may hold fast to the convenient fiction of a continuum, which is in no wise injurious. In this sense we term the thermal state a continuum.

CHAPTER VI

HISTORICAL SURVEY OF THE THEORY OF CONDUCTION OF HEAT

1. The fact of the conduction of heat, or the reciprocal effect of the temperatures of the parts of a body on one another, presents itself to observation as it were of its own accord. But the clarification of the quantitative ideas concerned proceeded very slowly. Amontons[1] heated one end of a thick iron bar red-hot and determined the temperatures of various points in the neighborhood of the other end with the air thermometer. Assuming that the temperature increases proportionally to the distance from the colder toward the hotter end, he found the places where tin, lead, and so on just melt, computed the melting temperatures from this principle, and, on the basis of this experiment, disputed the correctness of Newton's assertions respecting the melting points in question. Similarly, Amontons inferred the temperature of the heated end. Here was expressed the first quantitative but, as it proved, incorrect conception with regard to the process of conduction.

2. Lambert[2] had an idea, clear in principle, concerning the state of things in the same case of a bar lying with one end in the fire.

> This rod is therefore heated at only one end. The heat, however, penetrates by degrees into the more remote parts, and ultimately passes away out of every part into the air. If, now, the fire burns long enough and is maintained with the same intensity, every part of the bar finally acquires a *definite* degree of heat, since each part continually receives as much heat from the end lying nearer the fire as it communicates to the more remote end and to the air. This constant state I shall now examine.

In the calculation which followed this, Lambert no longer expressed himself with the same clearness. The decrease of temperature, du, which corresponds to the length dx of the rod was taken for the loss of heat of this portion of the rod to the air and was put proportional to the temperature-excess u above the air. Indeed, it follows, from the equation

$$\partial u/\partial x = \kappa u,$$

that u diminishes according to an exponential law — and Lambert

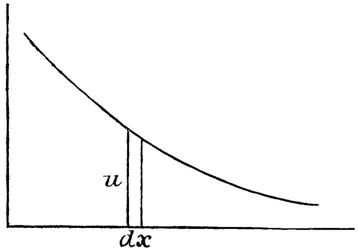


Fig. 32.

found this confirmed by experiment. The result is thus correct but not the method of deriving it, since the temperature gradient at a point determines only the *intensity of the current of heat* through the cross-section of the bar.

3. Franklin[3] suggested measuring the conductivity of bars of different metals, heated in the same way at one end, by the distance in which the melting temperature of wax advanced in a certain time. Ingenhousz[4] carried out the experiment. J. T. Mayer,[5] regarding that body as the best conductor which gave up its excess of heat to the air most quickly, drew from the above-mentioned experiments the opposite conclusion from that deduced by Ingenhouss. This is attributable to the fact that the two conceptions "internal conductivity" and "external conductivity" were not yet separated from one another.

4. The stationary state in a bar heated at one end was first correctly treated, experimentally and theoretically, by Biot.[6] He used Newton's law of cooling as a starting point.

In order to set up calculation according to this law, it is necessary to consider that every point of the bar receives heat from the one preceding and communicates it to the following one. The difference is what remains to it according to its distance from the source of heat; and, of this, a part disappears in the air, either by immediate contact with this fluid or by radiation .... Thus, in the state of equilibrium, when the temperature of the bar has become stationary, the increment of heat which each point of the bar receives by virtue of its position is equal to that lost by contact with the air and by radiation, the loss being proportional to its temperature.[7]

On the basis of this proposition, said Biot, a differential equation may be formed whose integral gives information concerning all relations subsisting in this case. In the article just referred to, Biot did not produce this equation, but confined himself to the reporting of experiments, and merely remarked that, in this whole investigation, he had been helped by Laplace. But, in another place,[8] Biot said that, according to an observation of Laplace's, the differential equation can be obtained only if we suppose a communication of heat between points in the bar of *finite* (though very small) distance from one another. In considering infinitely near points, it is evident that their differences of temperature and the quantity of heat interchanged between them are *infinitely small,* while the quantity of heat given up to the next colder layer must be equal to the entire *finite* quantity which all the succeeding colder parts of the bar lose to the air. For support of this assumption, Laplace referred to the translucency (previously observed by Newton) and hence the penetrability to heat-rays, of very thin metal leaves. Later, Fourier[9] continued these investigations further.

In the case of an iron bar which had been kept with one end in a bath of water of mercury at a definite temperature (60° or 82 °C) for ten hours, Biot proved that to steps proceeding in *arithmetical* progression toward the cold end of the bar correspond decreases in geometrical progression of the excesses of temperature above the surroundings. Like Amontons and Lambert, Biot utilised this principle for pyrometric purposes, and determined in this way the melting-point of lead, for example, as 210 °C.

5. We easily see how the law formulated by Biot can be arrived at by very simple considerations — like those undoubtedly employed by Fourier in his first attempts to establish the theory of the conduction of heat.[10] Imagine a succession of equal small particles (for example elements of a bar) whose excesses of temperature above the surroundings diminish according to the law of a geometrical progression. Let

$$u, \alpha u, \alpha^2 u, \alpha^3 u, \ldots \alpha^n u$$

be the sequence of these excesses of temperature, where u denotes the temperature excess of the first particle above the surroundings and α a constant proper fraction. Consider any three consecutive particles, for example, those with the temperature excesses

$$\alpha^{m-1} \cdot u, \; \alpha^m \cdot u, \; \alpha^{m+1} \cdot u;$$

then the middle particle gains from the one on the left a quantity of heat proportional to

$$(1 - \alpha)\alpha^{m-1} \cdot u$$

and suffers a loss proportional to

$$(1 - \alpha)\alpha^{m-1} \cdot u$$

to the particle on the right. Its total gain is thus proportional to the difference

$$(1 - \alpha)^2 \alpha^{m-1} \cdot u.$$

For the particle with the excess $\alpha^p \cdot u$ there is the analogous total gain $(1 - \alpha)^2 \alpha^{p-1} \cdot u$. The ratio of the total gain is thus

$$\frac{(1 - \alpha)^2 \alpha^{m-1} \cdot u}{(1 - \alpha)^2 \alpha^{p-1} \cdot u} = \frac{\alpha^m \cdot u}{\alpha^p \cdot u};$$

the same, therefore, as that of the temperature-excesses. But the losses to the surrounding air bear the same ratio to one another. Consequently the temperatures will rise until the total gains are just balanced by the losses to the air, and then the law of the geometrical progression of the temperature excesses will be satisfied. The law of the geometrical progression is not, of course, affected by the size of the intervals. Leave out, for example, two particles for each one taken: then the series becomes

$$u, \beta u, \beta^2 u, \beta^3 u, \ldots,$$

where $\beta = \alpha^3$.

If l be the distance between the middle points of each two adjacent particles, the temperature gradients between each pair of particles:

$$\frac{(1 - \alpha)u}{l}, \alpha \frac{(1 - \alpha)u}{l}, \alpha^2 \frac{(1 - \alpha)u}{l}, \ldots,$$

again from a geometrical series with the same exponent. Denoting the terms of the above series by $u_1, u_2, u_3, \ldots$, and forming the expressions

$$\frac{u_1 - u_2}{l}, \frac{u_2 - u_3}{l}, \ldots$$

which measure the velocities of the decrement of the gradient, these velocities again form a geometrical series:

$$\frac{(1-\alpha)^2 u}{l^2}, \alpha\frac{(1-\alpha)^2 u}{l^2}, \alpha^2\frac{(1-\alpha)^2 u}{l^2}, \ldots$$

with the same exponent. The properties of Biot's stationary state are, thus, very easily derived.

6. Fourier's contributions to the theory of the conduction of heat began in 1807 and ended, in essentials, with the publication in 1822 of his chief work already mentioned. In this work, the phenomena of the conduction of heat were deduced from the assumption that the parts in the interior of a conducting body which lie very near one another interchange quantities of heat proportional to their differences of temperature. This proposition is easily obtained from observations on the communication of heat; and, conversely, it may be regarded as established by the quantitative agreement of the results derived from it with experience. The entire theory of Fourier is, then, merely a comprehensive mathematical presentation of the facts of conduction of heat.

7. Fourier started from a very simple idea.[11] Let a heat conducting body (such as copper) completely fill the space between two infinite parallel planes (I, II). Plane I is supposed to be bathed constantly with the steam of boiling water and kept at the invariable temperature u_1(100 °C), while plane II remains continually in contact with melting ice and at the temperature u_2(0 °C). It is assumed that a distribution of temperature has established itself in the conducting plate, in virtue of which the temperature decreases proportionally to the distance from I to II according to the law of a straight line, and thus diminishes from u_1 to u_2; then this state remains stationary as long as I is maintained at u_1 and II at u_2. For, imagine a thin layer M, parallel to I and II, singled out of the conducting body and in this layer a particle m; then, for every warmer particle, m' lying to the left there exists a particle m'' lying to the right, symmetrically placed with respect to m and just so much colder than it as m is than m'. Thus m receives from m', in the same time, the same quantity of heat that it yields to m''. Therefore the temperature of m and of the whole layer M — and so too of any other layer — cannot change. In this investigation, only those particles are

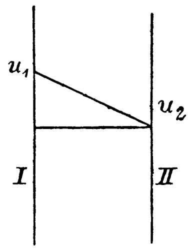


Fig. 33.

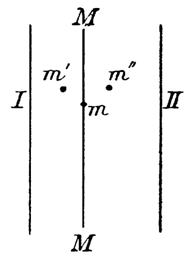


Fig. 34.

considered which lie near enough to m to enter into an exchange to heat with it.

But though the temperature of the particles lying in the plane M does not change, yet heat traverses the plane. The quantity w of heat which flows through the area q of the plane M in the time t is

$$w = k \cdot q \frac{u_1 - u_2}{l} t$$

That is to say, were the difference $u_1 - u_2$ doubled, the thickness l of the plate (I, II) remaining the same, then all differences of temperature of the particles taking part in the interchange would be doubled. The doubling of l would have the opposite effect. Obviously, the quantity of heat traversing the plane increases with t and q, and is, under otherwise similar conditions, dependent upon the material of the plate (copper, iron). This is indicated by the coefficient k, called by Fourier the "internal conductivity". The expression $(u_1 - u_2)/l$ is called the "temperature-gradient". Fourier rightly laid great stress on this idea of the *flow of heat,* upon which all further developments were founded.

8. In order to make clear the significance of k, we get from the above equation

$$k = \frac{w}{q \dfrac{u_1 - u_2}{l} t}$$

It we put $q = 1$, $(u_1 - u_2)/l = 1$, and $t = 1$, then k signifies the quantity of heat which, in the material with which we are concerned, flows through the unit of surface in the unit of time, provided the temperature gradient is unity and is perpendicular to this surface.

9. It follows also from the idea of the flow of heat that the above-mentioned stationary distribution of temperature actually establishes itself if only I and II are maintained at constant temperatures. Should the temperature gradient not be the same throughout but less on the left of M (Fig. 35), then less heat flows to M than flows away from it in the

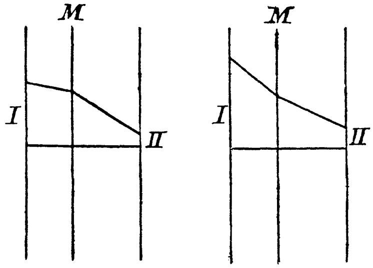


Fig. 35.

same time, so that the temperature of M sinks. The opposite occurs when the gradient on the left of M is greater than that on the right. It is evident, now, that if the temperature distribution is represented by any curve (Fig. 36) at all parts of the curve convex towards the axis of abscissae the temperature increases, and at all parts concave to the axis it decreases; so that the curve levels itself of its own accord and passes into a straight line. The above-mentioned stationary state is then attained. We may also say that, in this, every part assumes the *mean temperature* of the surroundings; and this is to be expected from the known properties of heat.

Suppose there is so slight a curvature of the temperature curve that the part of the curve which belongs to a part of the conducting medium

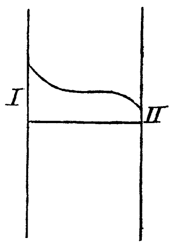


Fig. 36.

which is still just penetrable by heat-rays may be regarded as straight, the expression for the flow of heat referred to the unit of time may be written

$$w = -kq\frac{\partial u}{\partial x},$$

where k and q have the foregoing significations, x indicates the direction in which the temperature varies, and $\partial u/\partial x$ is the temperature-gradient at the point question. The sign $(-)$ shows that the stream of heat flows in the sense of diminishing temperatures.

10. We will now proceed to examine a variable (not stationary state of temperature. The temperature varies in the x-direction (perpendicular to I, II) according to some law not that of a straight line. We fix our attention upon some layer M (parallel to I, II) of the thickness dx (Fig. 37). From the left there enters through the surface q in the time dt the quantity of heat $-kq(\partial u/\partial x)\,dt$, while on the right, since $\partial u/\partial x$ varies with x, the quantity

$$-kq\left(\frac{\partial u}{\partial x} + \frac{\partial^2 u}{\partial x^2}\,dx\right)dt$$

of heat flows away.[12] The quantity of heat that accrues to M in the time dt is therefore $kq(\partial^2 u/\partial x^2)\,dx\,dt$. The volume of the layer lying upon the surface q is $q\,dx$, its density is ρ and its specific heat c; accordingly its capacity for heat is $q\,dx\,\rho c$. If $(\partial u/\partial t)\,dt$ be the increase of temperature in the time dt, the accrued quantity of heat is also

$$q \cdot c \cdot dx \cdot \rho\,\frac{\partial u}{\partial t}\,dt$$

Therefore the equation

$$q \cdot c \cdot dx \cdot \rho\,\frac{\partial u}{\partial t}\,dt = k \cdot q \cdot \frac{\partial^2 u}{\partial x^2}\,dx \cdot dt,$$

or

$$\frac{\partial u}{\partial t} = \frac{k}{c\rho}\,\frac{\partial^2 u}{\partial x^2},$$

subsists, in which, therefore, u is a function of x and t, whose properties are expressed by this partial differential equation.

11. If the temperature in an infinite heat-conducting body is supposed to be different from point to point and thus, in general varying in all three coordinate directions x, y, z, the corresponding equation follows

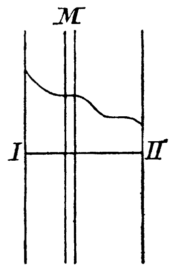


Fig. 37.

quite analogously. We consider an infinitely small parallelepiped of sides dx, dy, dz. In each coordinate direction a current goes in and out. For the currents in the x-direction $dy\,dz$ replaces q. In consequence of these currents, the increase in the quantity of heat in the time dt in the volume-element $dx\,dy\,dz$ is

$$k \cdot dy \cdot dz \frac{\partial^2 u}{\partial x^2} dx \cdot dt;$$

and similarly for the other two directions of the current,

$$k \cdot dx \cdot dz \frac{\partial^2 u}{\partial y^2} dy \cdot dt,$$

and

$$k \cdot dy \cdot dx \frac{\partial^2 u}{\partial z^2} dz \cdot dt.$$

On the other hand, the increase in the quantity of heat in the volume element is

$$dx \cdot dy \cdot dz \cdot \rho c \frac{\partial u}{\partial t} dt.$$

Hence we get the equation

$$\frac{\partial u}{\partial t} = \frac{k}{c\rho}\left(\frac{\partial^2 u}{\partial x^2} + \frac{\partial^2 u}{\partial y^2} + \frac{\partial^2 u}{\partial z^2}\right),$$

in which u is therefore a function of x, y, z, and t, whose properties are determined by this equation.

For a sphere, whose temperature u varies only with the distance r from the center, this equation takes the form

$$\frac{\partial u}{\partial t} = \frac{k}{c\rho}\left(\frac{\partial^2 u}{\partial r^2} + \frac{2}{r}\frac{\partial u}{\partial r}\right);$$

and for a cylinder, whose temperature u depends upon the distance r from the axis, it is

$$\frac{\partial u}{\partial t} = \frac{k}{c\rho}\left(\frac{\partial^2 u}{\partial r^2} + \frac{1}{r}\frac{\partial u}{\partial r}\right).$$

Both of these equations may easily be derived immediately from the general equation as well as analogously to this general equation.

12. In reality, we do not have to do with bodies unbounded upon one or all sides. On the contrary, the conducting bodies are bounded and usually immersed in another conducting medium (the air). Consequently, the processes at the surfact of a heat conducting body require special investigation. The quantity of heat w which a body loses through a surface area ω, maintained at the temperature excess u above the surroundings (the air), in the time t, is

$$w = h\omega ut,$$

and therefore proportional to ω, u, and t. The factor h depends upon the conducting body and the surrounding medium and was called by Fourier the "external conductivity". If we write the above equation in the form;

$$h = \frac{w}{\omega ut},$$

and put ω, u, and t equal to *unity*, then we see that the *external* *conductivity is determined by the quantity of heat which is lost to the surroundings with the unity of temperature excess through the unit of surface in the unit of time.*

13. In order to represent the conduction of heat in a bounded body, Fourier employed a highly ingenious method of investigation. Instead of the bounded body, he imagined first an unbounded one in which the boundary surface of the former is drawn. Since the temperature can vary from point to point, the temperature gradient at any point can also have any value whatever in *one* direction. Fourier now supposed the temperature gradient at any point of this boundary surface so chosen in a normal direction outwards (into the unbounded body) that the same heat-currents flow through the elements to the surface as would correspond to the coolings by the surrounding medium. Then the same processes take place in the part of the unbounded body imagined to be enclosed as in the corresponding bounded body. This consideration leads to the equation

$$-k\omega \frac{du}{dn} = h\omega u,$$

or

$$\frac{du}{dn} + \frac{h}{k} u = 0,$$

in which n denotes the normal direction of the surface element. Here

$$\frac{du}{dn} = \frac{\partial u}{\partial x}\frac{dx}{dn} + \frac{\partial u}{\partial y}\frac{dy}{dn} + \frac{\partial u}{\partial z}\frac{dz}{dn}$$

or

$$\frac{du}{dn} = \frac{\partial u}{\partial x}\cos\alpha + \frac{\partial u}{\partial y}\cos\beta + \frac{\partial u}{\partial z}\cos\gamma.$$

The angles between the normal and coordinate axes are here denoted by α, β, γ. If the equation of the surface, $F(x, y, z) = 0$, is given, the cosines may be expressed at once in the usual way by $\partial F/\partial x$, $\partial F/\partial y$, $\partial F/\partial z$. This concludes the fundamental part of Fourier's work.

14. Fourier was the first the point out that, if an equation be not merely a numerical contingency, but expresses an actual geometrical or

physical relation, its terms must be *magnitudes of the same kind,* or as he said, magnitudes of the same *dimension.*[13] Only then is the subsistence of the equation independent of the fortuitous choice of the units. I have elsewhere presented the theory of dimensions, and shall not discuss it here again.[14]

15. After clearly defining the conception "internal conductivity", the determination in a rational way of the constant k relating to it may be proceeded with. This has been attempted by Fourier[15] and Péclet.[16] Both methods are based upon the experimental ascertainment of the quantity of heat which traverses a plate of given thickness and surface in a definite time, a definite difference of temperature between the two surfaces being maintained. Imagine two large and known masses of water of different temperatures well protected against outward loss of heat separated by a metal plate of given dimensions. The quantity of heat which has traversed the plate is given immediately by the changes of temperature which take place. Into the details of this experiment, simple in principle but difficult of accomplishment and therefore defective, we shall not enter.

On the other hand, Biot's case, which is at the same time a good example of Fourier's theory, will be discussed more closely. For a plate (I, II) in which the temperature varies in only one direction (x) the equation

$$\frac{\partial u}{\partial t} = \frac{k}{c\rho}\,\frac{\partial^2 u}{\partial x^2}$$

holds. When the stationary state is reached $\partial u/\partial t = 0$, and therefore also

$$\partial^2 u/\partial x^2 = 0$$

The integral of this equation:

$$u = ax + b,$$

gives the already known temperature distribution according to the law of a straight line. The constants of integration, a and b, are determined by the conditions $u = u_1$ for $x = 0$ and $u = u_2$ for $x = l$ (the thickness of the plate); whence

$$u = \frac{u_2 - u_1}{l}\, x + u_1 .$$

The same law of stationary temperature distribution as that erroneously assumed by Amontons for any bar would hold for a bar heated at one end and protected from loss outwards.

A more detailed examination is necessary for Biot's case even if, for the sake of simplicity, we regard the temperature as the same throughout the section of the bar, supposed to be thin. From Fourier's fundamental formulae there follows the equation

$$q \cdot dx \cdot \rho c \frac{\partial u}{\partial t} dt = kq \frac{\partial^2 u}{\partial x^2} dx \cdot dt - h \cdot p \cdot dx \cdot u \cdot dt,$$

or

$$q \frac{\partial u}{\partial t} = \frac{kq}{c\rho} \frac{\partial^2 u}{\partial x^2} - \frac{hp}{c\rho} u,$$

where p denotes the perimeter of the section of the bar, and all the other letters have the known signification. For the stationary state $\partial u / \partial t = 0$, or

$$\frac{\partial^2 u}{\partial x^2} - \frac{hp}{kq} u = 0.$$

The general integral of this is

$$u = A e^{x\sqrt{\kappa}} + B e^{-x\sqrt{\kappa}},$$

in which A and B are the constants of integration, and, for brevity, κ is put for hp/kq. From the conditions that $u = 0$ for $x = \infty$ and $u = U$ — the temperature of the bath — for $x = 0$, the integral takes the form

$$u = U e^{-x\sqrt{\kappa}}$$

which gives the geometrical progression of the temperature-excesses. For steps of length $x = 1$, we have $(1/e)^{\sqrt{\kappa}}$ as the ratio of the progression. If we determine this by trial, we get $-\kappa = hp/kq$. If we take bars of different materials but of the same dimensions and with the same covering (varnish or silver-plating) in order to make h the same, as C. Despretz[17] has done then for different materials $\kappa/\kappa' = k'/k$.

17. J. D. Forbes[18] carried out an absolute determination of k by a method suggested by Fourier's[19] derivation of it. This derivation is based upon the following idea. If the exponent for the stationary state has been determined, the temperature at all points of the bar and also

the temperature gradient are known. Let the temperature gradient at any point be $\partial u/\partial x$, then there flows through the section q in the unit of time the quantity of heat $kq(\partial u/\partial x)$. This is just as great as the loss of heat of the entire bar lying behind (in the sense of flow) the section considered.

This loss of heat is directly determined by a second special distinct experiment. The entire bar is heated to u and afterwards allowed to cool. If the decrease of temperature is observed from minute to minute, the loss of temperature u' belonging to each temperature u in the unit of time is known. Here u' is proportional to u. If l is the very small length of a portion of the bar, then $qlc\rho u'$ is the quantity of heat lost by it in the unit of time. Since the distribution of temperature in the whole bar is given, the quantity of heat lost by any portion of the bar in the unit of time may easily be specified for the observed stationary state.

By the process of Forbes, a somewhat different value for k is obtained according as the section is taken through a place of higher or lower temperature. Consequently k, instead of being a constant, depends in a slight degree upon the temperature, as Fourier[20] regarded as possible.

The theory, therefore, requires modification with respect to this circumstance.

If 1 cm is chosen as unit of length, 1 °C as unit of temperature, 1 minute as unit of time, and a gram-calorie as unit of quantity of heat, then, according to Forbes, $k = 12.42$ for iron at 0 °C but $k = 7.44$ for iron at 275 °C.

F. Neumann,[21] also in continuation of Fourier's work, has found, with the same units,

	k
Copper	66.47
Zinc	18.42
Iron	9.82

18. Important as was the clarification of the ideas of the conduction of heat and the solution of a wide range of problems which resulted from Fourier's works, yet vastly more important was the development and transformation of the methods of mathematical physics which was caused by them. In order to describe this transformation, the way for

which was already to some extent prepared, we must go back somewhat in our account.

19. Through various investigations concerning the vibrations of cords, clearer ideas of the nature of partial differential equations had been gained, and mathematical experience, which Fourier knew how to utilise in the most fruitful way, accumulated. The first attempt to treat the vibrations of cords mathematically was made by Brook Taylor[22]. Taylor considered a stretched string to which the very feeble bending

$$u = a \sin \frac{\pi x}{l}$$

is given.[23] All elements of the string then receive accelerations towards the position of equilibrium, which are proportional to their distance from it, with the same factor of the proportion for all elements. Thus all elements perform pendulum-like and synchronous oscillations, simultaneously pass the position of equilibrium, and simultaneously reach the maximum of their displacements. If the acceleration belonging to a definite displacement is determined for *one* element, then the time of vibration of the string can be found.

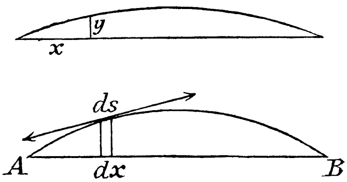


Fig. 38.

In order to make the problem definite, we will consider an element of the string, *ds*, which may be assumed equal to *dx*. If p (in absolute measure) is the tension of the string, then, from the left, the element is subject to the pull p, whose vertical component downward, since u is diminished thereby, is $-p(\partial u/\partial s)$ or $-p(\partial u/\partial x)$. On the right, the pull p likewise operates, but its vertical component is

$$+p\left(\frac{\partial u}{\partial x} + \frac{\partial^2 u}{\partial x^2}\,dx\right).$$

Therefore the vertical component affecting the element ds (or dx) is $p(\partial^2u/\partial x^2)\,dx$ or, since $u = a\sin(\pi x/l)$ and

$$\frac{\partial^2 u}{\partial x^2} = -\frac{\pi^2 a}{l^2}\sin\frac{\pi x}{l} = -\frac{\pi^2}{l^2}\,u,$$

the force becomes $-dx\,p(\pi^2/l^2)u$, and is, accordingly, proportional to the displacement. If m is the mass of the whole string, and, consequently, $m\,dx/l$ that of the element, then for the unit displacement, the acceleration (force divided by mass) of any element is $p\pi^2/ml = f$.[24] The time of a complete oscillation is[25]

$$T = 2\pi\sqrt{\frac{1}{f}} \quad \text{or} \quad T = 2\sqrt{\frac{ml}{p}}.$$

Taylor regarded the motion of the string described above as the only one. Were the initial form of the string different, Taylor erroneously believed — he even produced a proof for it — that the sine-form would immediately establish itself and the form of oscillation described above would be set up.[26] D'Alembert[27] was not under this delusion: he knew that, on the contrary, the motion of a string can be just as infinitely diversified as the initial form given to it. Since, from what precedes, the force affecting an element of the string is $p\,dx(\partial^2u/\partial x^2)$, and, as it can also be represented by $(m\,dx/l)(\partial^2u/\partial t^2)$, where $\partial^2u/\partial t^2$ denotes the acceleration, d'Alembert found the equations

$$\frac{\partial^2 u}{\partial t^2} = \frac{pl}{m}\frac{\partial^2 u}{\partial x^2}$$

in which Euler[28] wrote, more briefly, $pl/m = c^2$.

It is even possible, by suitable choice of the units of measurement, to put $c = 1$. This latter case is the one which d'Alembert actually investigated. The displacement u of a point of the string depends both upon the distance x of the point from the end of the string and upon the time t: it is a function of both variables. By particular considerations d'Alembert gained the insight[29] that

$$u = \phi(x + t) + \psi(x - t)$$

represents the general integral of the equation

$$\frac{\partial^2 u}{\partial t^2} = \frac{\partial^2 u}{\partial x^2}$$

where ϕ and ψ are undetermined functions of $x + t$ and $x - t$. Euler afterwards gave for the more general equation the integral $u = \phi(x + ct) + \psi(x - ct)$, and deduced it by his method.[30] Thus there are therefore infinitely many states of motion of the string conceivable.

21. Daniel Bernoulli believed that he could harmonize the conceptions of Taylor and d'Alembert in a simple way. Sauveur[31] had already shown experimentally that a string can move not only as a whole, vibrating its fundamental tone, but also when divided into 2, 3, 4, ... equal parts, vibrating with a 2, 3, 4, ... fold number of oscillations, and that, furthermore, all these motions may take place simultaneously.

Theoretical difficulties did not stand in the way of the elucidation of Sauveur's phenomena. It was seen that the nodes (k), if the string

Fig. 39.

receives sine-shaped bendings, were continually acted upon by equal and opposite tensions, and so behaved as fixed points. If a very feeble and sine-shaped bending of the fundamental tone is imagined, scarcely anything is changed by it in the relations of the tensions of the string. The sine-shaped bending of the octave appears as a deviation from that of the fundamental tone and it might be conceived to carry out its motion about this as about a (variable) form of equilibrium. In this way, Bernoulli imagined to be situated in the string a whole series of sine-bendings of which 1, 2, 3, 4, ... half-periods left no remainder in the length of the string, so that the initial bending u was represented by

$$u = a_1 \sin \frac{\pi x}{l} + a_2 \sin \frac{2\pi x}{l} + a_3 \sin \frac{3\pi x}{l} + \ldots,$$

and he thought any initial bending whatever of the string could be represented in this way. Thus, in his opinion, Taylor had the correct solution, and the infinite multiplicity of the solution of d'Alembert was explained mathematically and physically by the simultaneous occurrence of such motions as Taylor described.[32] Euler[33] admitted the value of Bernoulli's view, but denied the possibility of representing by periodic series every initial form of the string, for example one composed of broken straight lines. Accordingly d'Alembert's solution, which admitted such initial forms, still seemed to him the more general. In the discussion alluded to there lay, as we shall see, all the germs of Fourier's developments.

22. Having now discussed the circumstances under which such questions arose, we propose to examine these questions more closely; and in the first place to inquire into the essential difference between the integrals of an *ordinary* and a *partial* differential equation.

An ordinary differential equation $dy/dx = f(x)$, in which we imagine the variables separated, gives the law of growth of y for variations of x. The integration consists in the reconstruction of the function from this law of growth. But the law of growth, by its very nature, contains nothing about the initial value of the function; and, for this reason, the "constants of integration" remain undetermined. For example, if the gradient of a railway is known from meter to meter of horizontal projection, the contour can be reconstructed from this, but not the absolute height of the initial point (or of any other point).

A *partial* differential equation gives, in the simplest case, the dependence of the two first partial differential quotients of a function of *two* variables upon one another. If, for example, $u = f(x, y)$, and we put

$$\frac{\partial u}{\partial x} = a\frac{\partial u}{\partial y},$$

then $\partial u/\partial x$ is determined by $\partial u/\partial y$ or *vice versa*, but the values of the one or the other remain wholly undetermined. And so the manner of dependence of u upon x or upon y, as the case may be, remains wholly undetermined. There is merely a relation between the law of dependence of u upon x and that of u upon y, and this relation is that expressed by the equation.

This will be made still clearer by examining particular examples which lead to partial differential equations. Referred to a system of rectangular coordinates, $y = b - ax$ is the equation of a straight line in the xy-plane, or, in three dimensions, the equation of a plane perpendicular to this xy-plane which passes through the above line (where $b = OM$), while $u = c$ (where $c = OR$) is the equation of a plane perpendicular to the u-axis. Both equations together represent a straight line ($M'N'$) parallel to the first (MN). If a remains constant, while b and c vary according to a certain law $c = \phi(b)$, the line moves parallel to itself and describes a cylindrical surface. If we regard $b = OM$ and $c = OR$ as coordinates of a directrix $c = \phi(b)$ lying in the yu-

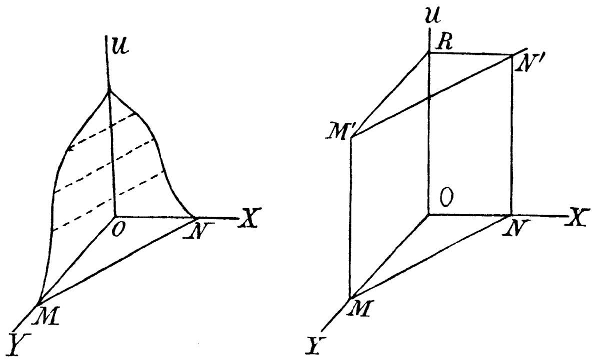


Fig. 40.

plane, we obtain, by substituting u for c and $y + ax$ for b in the above equation,

$$u = \phi(y + ax).$$

as the equation of the cylindrical surface (in the yu-plane) with the wholly arbitrary directrix $u = \phi(y)$. Everywhere that $y + ax$ has the same value, u has the same value; and in this lies the character of this cylindrical surface. If we form the expressions $\partial u/\partial x = a.\ \phi'$ and $\partial u/\partial y = \phi'$, it is evident that between the two the relation

$$\frac{\partial u}{\partial x} = a\frac{\partial u}{\partial y}$$

holds, and this represents the (partial) differential equation of the cylindrical surface, from which the function which determines the form of the directrix has entirely vanished, nor can it be derived from the differential equation.

The function ϕ of the integral equation $u = \phi(y + ax)$ is thus undetermined; nevertheless, if the differential equation is to be satisfied, it cannot involve x and y in any manner, but only in the combination $y + ax$. Thus the peculiarity of such integrals is that they present themselves in the form $u = \phi[f(x, y)]$ as undetermined functions ϕ of determined functions f of x and y. The two differential quotients are

$$\frac{\partial u}{\partial x} = \frac{\partial f}{\partial x}\phi \quad \text{and} \quad \frac{\partial u}{\partial y} = \frac{\partial f}{\partial y}\phi'$$

The fixed relation between these partial differential quotients is given by the determined function f and its partial differential quotients $\partial f/\partial x$,

$\partial f/\partial y$, which do not disappear. The differential equation of the foregoing example says that at every point $\partial u/\partial x$ is a times greater than $\partial u/\partial y$. With regard to the course of the surface in the section XU or YU, nothing is determined by this. Only if one is chosen, then the other is also subject to a condition.

For the total change of u, we find

$$du = \frac{\partial u}{\partial x}\, dx + \frac{\partial u}{\partial y}\, dy = \frac{\partial u}{\partial y}\,(a \cdot dx + dy).$$

Thus $du = 0$ if $a \cdot dx + dy = 0$ or $dy = -a\, dx$; that is, if dy always moves with a times greater steps than dx and in the opposite sense. In this consists the character of the cylindrical surface with this axial direction.

A surface of rotation which has the u-axis as axis will serve as a second example. Let the meridian section be $u = \phi(r^2)$; then the equation of the surface of rotation is

$$u = \phi(x^2 + y^2).$$

and, since

$$\frac{\partial u}{\partial x} = \phi' 2x \quad \text{and} \quad \frac{\partial u}{\partial y} = \phi' 2y,$$

we have

$$y \frac{\partial u}{\partial x} = x \frac{\partial u}{\partial y}$$

as the (partial) differential equation of the surface of rotation of which the above is the integral equation. Here ϕ is an undetermined function of the determined function $x^2 + y^2$ of x and y. The meridian section is wholly undetermined. But the character of the surface consists in that u remains unchanged so long as $x^2 + y^2$ is constant, or $x \cdot dx + y \cdot dy = 0$.

23. The general integral of the partial differential equation

$$\frac{\partial^2 u}{\partial t^2} = c^2 \frac{\partial^2 u}{\partial x^2}$$

is, as already stated,

$$u = \phi(x + ct) + \phi(x - ct).$$

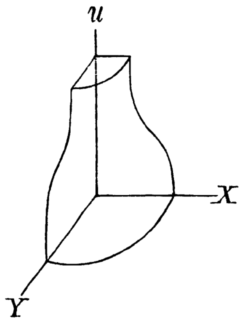


Fig. 41.

This contains two undetermined functions ϕ and ψ each of a determined function ($x + ct$ or $x - ct$) of the two variables x and t. Substitution or the working out of the differentiation shows at once that this integral satisfies the equation. If x, t and u are regarded as geometrical coordinates, then $u = \phi$ and $u = \psi$ are two cylindrical surfaces of different axial directions which are parallel to the xt-plane and symmetrical about the t-axis, but of undetermined directrices. In them, u remains unchanged as long as $x + ct$ and $x - ct$ respectively remain unchanged, or as long as $dx + c \cdot dt = 0$ and $dx - c \cdot dt = 0$, respectively. If, then dx and dt are put in the relation $dx/dt = -c$ or $dx/dt = c$ respectively; that is to say, if we move in the physical sense upon x with the velocity $-c$ (or $+c$ respectively), we keep the same values of u. Thus, physically speaking, ϕ, ψ are waves of any form whatever, which proceed along the x-axis with the velocities $-c$ (or $+c$). For a string with fixed points, ϕ and ψ satisfy special and easily assignable conditions which will not be further investigated here.

A more careful consideration of the foregoing differential equation explains why its integral contains two undetermined functions. Since $\partial^2 u/\partial t^2$ is determined by $\partial^2 u/\partial x^2$, the latter, and therefore also $\partial u/\partial x$ and $u = F(x)$ remains undetermined for all values of x. If $\partial^2 u/\partial t^2$ is indirectly determined (by $\partial^2 u/\partial x^2$), the general expression for $\partial u/\partial t$ can be derived, but not the initial value of $\partial u/\partial t = f(x)$ for the whole range of x. In order to be able to make provision both for F and f, the integral must contain *two* functions ϕ and ψ. Physically, the necessity of two undetermined functions results from the fact that we can give to the whole series of points of the string both any initial displacements and also initial velocities altogether independent of these displacements.

24. A special function which satisfies a differential equation — a so-called particular integral — is comparatively easy to find. The exponential has the known property of giving, by differentiation, the original function multiplied by a constant. Thus the idea of substituting $u = e^{\alpha t + \beta x}$ in the equation readily suggests itself. In fact, the equation is seen to be satisfied for $\alpha = \pm\beta c$. Thus it is satisfied by $u = e^{\beta(x+ct)}$ and $u = e^{\beta(x-ct)}$. If β is chosen imaginary, it is seen that both $u = \cos\beta(x \pm ct)$ and $u = \sin\beta(x \pm ct)$ and also the expressions $\sin\beta x \cdot \cos\beta ct$, $\cos\beta x \cdot \sin\beta ct$, $\cos\beta x \cdot \cos\beta ct$, and $\sin\beta x \cdot \sin\beta ct$, into which they decompose, satisfy the equation. As Euler[34] observed, the expression

$$a_1 u_1 + a_2 u_2 + a_3 u_3 + \ldots,$$

where $u_1, u_2, u_3, \ldots$ are particular integrals and $a_1, a_2, a_3, \ldots$ arbitrary constants, also satisfies the differential equation if it is linear. Because of this property, we can construct more general integrals, in numerous ways, from particular integrals. The consideration just mentioned leads also to the above *most general* form of the integral.

25. The general integral of the equation

$$\frac{\partial^2 u}{\partial x^2} + \frac{\partial^2 u}{\partial y^2} = 0,$$

which forms the basis of important investigations, is

$$u = \varphi(x + y\sqrt{-1}) + \phi(x - y\sqrt{-1}),$$

which is derived from the former one if we substitute y for t and -1 for c^2.

26. The ideas just explained were combined, developed, and turned to good account by Fourier. Fourier observed, in the first place, that simple relations such as those upon which Taylor had based his examination of the motion of strings are also imaginable in the domain of the conduction of heat. In an infinitely extended heat-conducting body, let the temperature u vary only in the one direction x, and according to the law

$$u = a \sin rx.$$

Then it is easily proved that the velocity of variation of temperatures is throughout proportional to the temperatures themselves and according

Fig. 42.

to the same coefficient of the proportion throughout. It is true that the temperatures will become equalized, yet the distribution will always remain sine-shaped and retain its period, just as the analogue holds for the displacements of the string in Taylor's investigations. But while the string, since accelerations are determined by the differences of the displacements of neighboring points, enters into vibrations, the temperatures — since velocities of equalization which diminish proportionally to the temperatures are determined by the differences — tend by the law of a geometrical progression to the mean final temperature which is reached only after an infinite time.

In fact, if

$$u = a \sin rx,$$

we have, by Fourier's equation

$$\frac{\partial u}{\partial t} = \kappa \frac{\partial^2 u}{\partial x^2},$$

where $\kappa = k/c\rho$,

$$\partial u/\partial t = -\kappa r^2 a \sin rx,$$

or

$$\partial u/\partial t = -r^2 \kappa u,$$

the velocity of change thus being proportional to the temperature. Integration with respect to t gives, for a definite point,

$$u = Ae^{-r^2 \kappa t},$$

where A denotes the initial value of u, therefore

$$u = e^{-r^2 \kappa t} a \sin rx$$

represents the entire course of the phenomenon as it was described above in words. We may verify that the last expression satisfies the differential equation, whatever values a and r may assume.

27. If we imagine an infinitely long thin bar protected from outward

conduction of heat, the temperature follows the same law if we assume the same variation in the direction of length. If the bar is taken of finite length and bent into a ring, the phenomenon still remains the same if only a number of periods of the sine — for example, one period — divides into the circumference of the ring without remainder. In the latter case, if the temperature ordinates are erected perpendicularly to the plane of the ring, their ends lie in a plane drawn through that diameter of the ring which contains the point of zero temperature; this plane, during the equalization of temperatures, gradually diminishes its angle with the plane of the ring and finally, after an infinite time, coincides with it (Fig. 43).

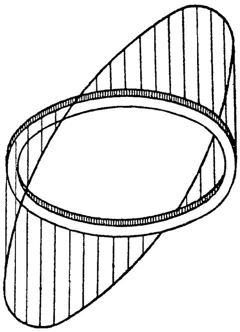


Fig. 43.

If the temperature of the surrounding medium is taken as zero, the interchange of heat with it cannot change the form of the foregoing process, since the velocities of equalization are proportional to the temperatures. The only difference is that the fall of the geometrical progression will be greater. The same is true for a bar which is not protected from external conduction of heat.

28. We will leave the consideration of the external loss of heat, and turn back to the variation of temperature in the x-direction in a conducting body of infinite extension. When Fourier took up the idea of putting together the solution of a differential equation from particular integrals, after the precedent of Daniel Bernoulli and Euler, he arrived at a very manifold distribution of temperature. That is, if we put

$$u = e^{-r_1^2 \kappa t} a_1 \sin r_1 x + e^{-r_2^2 \kappa t} a_2 \sin r_2 x + e^{-r_3^2 \kappa t} a_3 \sin r_3 x + \ldots,$$

where a_1, a_2, a_3, ... and r_1, r_2, r_3, ... have any values whatever, and the number of terms can be as large as we please, then this expression also satisfies the above differential equation, and represents the whole process which begins with the initial distribution

$$u = a_1 \sin r_1 x + a_2 \sin r_2 x + a_3 \sin r_3 x + \ldots$$

But Bernoulli had not yet succeeded in representing an *arbitrary* function; he had not yet been able to attain to the complete generality of d'Alembert's solution, which appeared to Euler also unattainable by this method. However Fourier accomplished this by using infinite periodic series. In order not to interrupt the discussion of the main subject, we will postpone the consideration of the method adopted by Fourier for this purpose. We will now illustrate Fourier's treatment of the subject by examples, the results of his work.

29. Fourier attempted so to determine the coefficients a, b, c, d, ... in the infinite series

$$1 = a \cos x + b \cos 3x + c \cos 5x + d \cos 7x + \ldots$$

that the foregoing equation is satisfied. Fourier[35] succeeded in doing this through the successive addition of terms by induction, and obtained the equation

$$1 = \frac{4}{\pi} (\cos x - \tfrac{1}{3} \cos 3x + \tfrac{1}{5} \cos 5x - \tfrac{1}{7} \cos 7x + \ldots)$$

which is correct for values of x between $\pi/2$ and $-\pi/2$.

By reason of the periodic nature of the terms the value of the right-hand side of the equation changes in the manner indicated in the figure between $+1$ and -1. If both sides of the equation are multiplied by u, an oscillation between the values u and $-u$ is represented. If the variation is to occur, not in periods of the length π but of the length l, then $\pi x/l$ is to be substituted in the place of x. Paying attention to the foregoing[36] consideration, we see that the equation

$$u = \frac{4u_1}{\pi} \left(e^{-\kappa\pi^2 t/l^2} \cos \frac{\pi x}{l} - \frac{1}{3} e^{-9\kappa\pi^2 t/l^2} \cos \frac{3\pi x}{l} + \ldots \right)$$

which, for $t = 0$, represents a variation of temperature (corresponding to Fig. 44) in the x-direction with jumps between u_1 and $-u_1$, and in

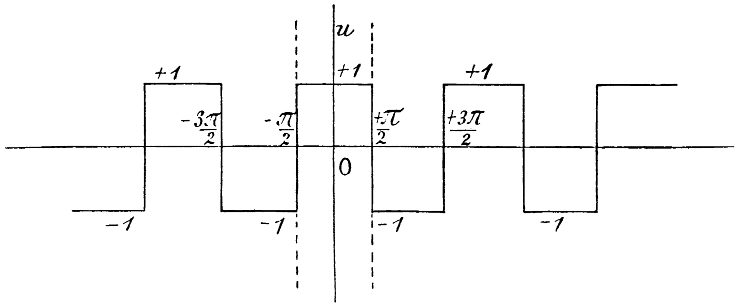


Fig. 44.

periods of length l, gives the entire course of the equalization of temperature for increasing t's. Nothing now interferes with our imagining an infinite plate of thickness l and included between parallel planes perpendicular to x, being cut out of the conducting body. If this plate is heated to the initial temperature u_1, and immersed in melting ice, the equation represents the whole process of the conduction of heat (or the process of cooling) in it. That the equation beyond $x = \pm l/2$ has an analytical significance need not perplex us. The process takes place in the same way in the plate, whether we regard this plate as part of an infinite body or as isolated and immersed in ice, just as an oscillating part of a string behaves as if its ends were fixed.

30. If the curve corresponding to the series is constructed by adding one term after another, (Fig. 45) the curves 1, 2, 3, . . . , in turn, are formed and they approach, as we see, the curve in Fig. 46, 1, about which they play, so to speak, in oscillations of diminishing amplitude and period. If now, the exponentials are added, it is seen that the terms of shorter period vanish much more rapidly than those of longer period, so that, as the time increases, the term of longest period takes on a predominating importance. This is shown in Figure 46, 2. This results in a rounding of the corners of the curve. This behavior corresponds exactly to that in the case of the vibrating strings whose higher partial tones have a shorter time of vibration than the lower. Fourier, following the example of Galileo, endeavored to separate the process into component processes which can immediately be grasped.

31. As a second example a stationary distribution of temperature may

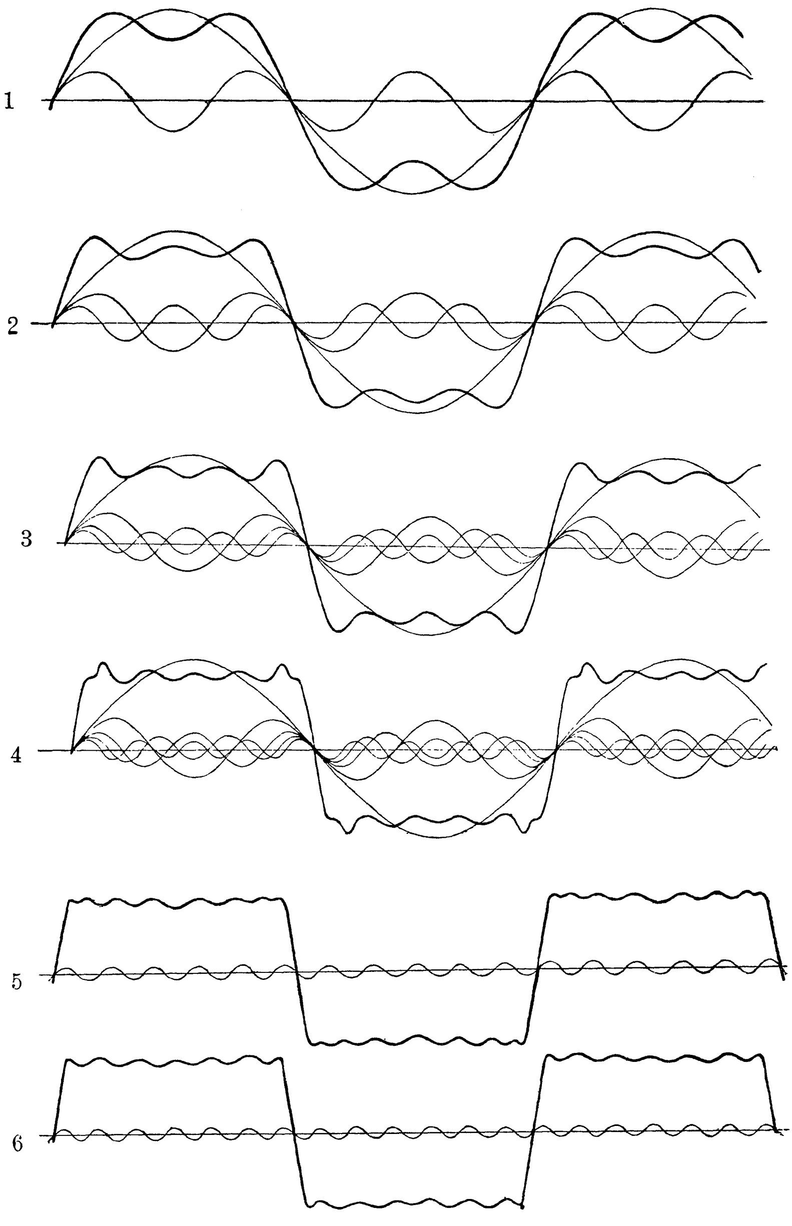


Fig. 45.

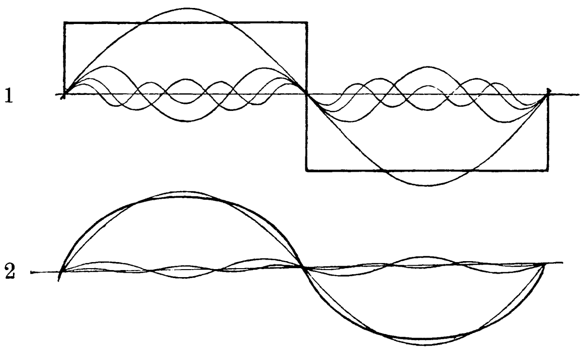


Fig. 46.

be discussed. We imagine a conducting body bounded by three planes perpendicular to the XY-plane. Of these, two are parallel to the X-axis (these represented in section by AC and BD) and unbounded in the X-direction; the third (AB) goes through the Y-axis. The whole plane AB is bathed by steam of boiling water, while AC and BD remain in contact with melting ice. The stationary state of temperatures must satisfy the equation

$$\frac{\partial^2 u}{\partial x^2} + \frac{\partial^2 u}{\partial y^2} = 0.$$

A particular integral is

$$u = e^{-\mu x} \cos \mu y,$$

and therefore a more general integral is

$$u = a_1 e^{-\mu_1 x} \cos \mu_1 y + a_2 e^{-\mu_2 x} \cos \mu_2 y + a_3 e^{-\mu_3 x} \cos \mu_3 y + \dots .$$

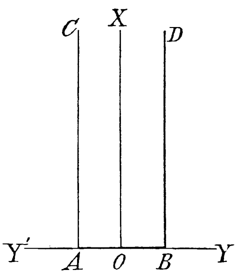


Fig. 47.

By referring to the previous example, we see that this integral may be adapted to the conditions of the problem by putting

$$u = \frac{4\mu_1}{\pi}\left(e^{-\pi x/l}\cos\frac{\pi y}{l} - \frac{1}{3}e^{-3\pi x/l}\cos\frac{3\pi y}{l} + \right.$$

$$\left. + \frac{1}{5}e^{-5\pi x/l}\cos\frac{5\pi y}{l} - \dots\right)$$

In this $AB = l$ and the temperature on AB is put equal to u_1. Here also the stationary state of flow can be resolved into several parts which allow of an easy survey; and observations which are similar to those in the previous case may be made.

32. It is not necessary here to go into the particulars of all the problems that Fourier solved. The examples given suffice to show the character of these researches. Throughout, it is Fourier's plainly expressed aim[37] not only to represent the phenomena in formulas, but in such formulas as permit an insight into, and numerical calculation of, the processes. Formulas which do not afford these advantages appeared to him to be idle transformations under which the processes remain no less hidden than under the differential equations from which we started.

33. But we must mention the propositions which make the convenient handling of periodic series possible. The ideas underlying them are as follows. It is required so to determine the coefficients in the series

$$a_1 \sin x + a_2 \sin 2x + a_3 \sin 3x + \dots$$

that its sum is equal to a given function $f(x)$. If we have n terms of the series, we can so choose the n coefficients a_1, a_2, a_3, ... that, for n values of x, the value of the series actually represents the respective values of $f(x)$. But it is clear that, for $x = 0$ and $x = \pi$, the value of the series is necessarily zero. If the series, for values of x which increase from 0 to π, assumes in succession the values $+p$, $+q$, $+r$, ..., then, from π to 2π, by virtue of the periodic nature of the terms, the values ..., $-r$, $-q$, $-p$, with contrary signs and in reverse order, must be taken; and this entire succession of values repeats itself as often as x

increases by 2π. Thus, by choice of the coefficients $a_1, a_2, a_3, \ldots$, we are able to assign values only between $x = 0$ and $x = \pi$.

If we divide the interval π into n parts, we have for the $n - 1$ points of division:

$$f\left(\frac{\pi}{n}\right) = a_1 \sin\frac{\pi}{n} + a_2 \sin\frac{2\pi}{n} + \cdots + a_{n-1} \sin(n-1)\frac{\pi}{n},$$

$$f\left(\frac{2\pi}{n}\right) = a_1 \sin\frac{2\pi}{n} + a_2 \sin 2\,\frac{2\pi}{n} + \cdots + a_{n-1} \sin(n-1)\frac{2\pi}{n},$$

$$f\left(\frac{(n-1)\pi}{n}\right) = a_1 \sin\frac{(n-1)\pi}{n} + a_2 \sin 2\,\frac{(n-1)\pi}{n} + \cdots + a_{n-1} \sin(n-1)\frac{(n-1)\pi}{n}.$$

From these $n - 1$ equations, $n - 1$ of the coefficients $a_1, a_2, a_3, \ldots$ can be determined. This is done most conveniently according to the method of Lagrange[38], that is, by multiplying each equation by one of the coefficients $\lambda_1, \lambda_2, \ldots, \lambda_{n-1}$, adding all the equations, and afterwards by so choosing the coefficients that all factors of $a_1, a_2, \ldots a^{n-1}$ with the single exception of a_m, for example, vanish. In this way a_m is determined. Lagrange found that a_m is determined by putting

$$\lambda_1 = 2 \sin\left(m\frac{\pi}{n}\right),$$

$$\lambda_2 = 2 \sin\left(2m\frac{\pi}{n}\right),$$

$$\lambda_3 = 2 \sin\left(3m\frac{\pi}{n}\right),$$

. . . .

$$\lambda_{n-1} = 2 \sin((n-1)m\pi/n);$$

so that every equation is multiplied by twice the coefficient which a_m already has in that equation. In fact, a computation which is rather lengthy but simple in principle[39] shows that all the coefficients of a_1, a_2, $\ldots$, a_{n-1} vanish with the exception of that of a_m. Then obviously

$$a_m = \frac{2}{n}\left[f\left(\frac{\pi}{n}\right)\sin\frac{m\pi}{n} + f\left(\frac{2\pi}{n}\right)\sin\frac{2m\pi}{n} + \right.$$

$$\left. + f\left(\frac{3\pi}{n}\right)\sin\frac{3m\pi}{n} + \cdots + f\left(\frac{(n-1)\pi}{n}\right)\sin\frac{(n-1)m\pi}{n}\right].$$

34. Fourier[40] was the first to think of carrying out this process for an *infinite* number of terms of a series; by this means an infinite number of values of $f(x)$ can be represented by the series, even though the curve representing $f(x)$ be composed by broken lines. In this case, if $2\pi/\pi n$ is written before the bracket instead of the factor $2/n$, and we put $\pi/n = dx$, $2\pi/n = 2\,dx$, and so on, the whole expression on the right becomes a definite integral and we have

$$a_m = \frac{2}{\pi}\int_0^\pi f(x)\sin mx\,dx.$$

If, in the interval from 0 to π_1, $f(x)$ is discontinuous, the integral must, of course, be separated into several parts.

In a similar way we find the development

$$f(x) = b_0/2 + b_1\cos x + b_2\cos 2x + b_3\cos 3x + \ldots,$$

where

$$b_m = \frac{2}{\pi}\int_0^\pi f(x)\cos mx\,dx.$$

A still more general expression is[41]

$$f(x) = b_0/2 + b_1\cos x + b_2\cos 2x + b_3\cos 3x + \cdots +$$

$$+ a_1\sin x + a_2\sin 2x + a_3\sin 3x + \ldots,$$

in which

$$b_m = \frac{1}{\pi}\int_{-\pi}^{+\pi} f(x)\cos mx \cdot dx$$

$$a_m = \frac{1}{\pi}\int_{-\pi}^{+\pi} f(x)\sin mx \cdot dx.$$

If the function $f(x)$ is such that $f(-x) = f(x)$, the coefficients a vanish; if, on the other hand, $f(-x) = -f(x)$, the coefficients b vanish; so that this last series contains the two previous ones as special cases.

The series can only, in the first instance, be used within the limits $x = -\pi$ to $x = +\pi$. If $f(x)$ is to be represented within a wider interval of values of x, a variable u, connected with x by the equation $x = cu/\pi$, is introduced: then u varies only from $-\pi$ to $+\pi$ while x varies from $-c$ to $+c$. For $u = -\pi$ to $u = +\pi$, the equations

$$f(cu/\pi) = b_0/2 + b_1 \cos u + b_2 \cos 2u + b_3 \cos 3u + \cdots + \\ + a_1 \sin u + a_2 \sin 2u + a_3 \sin 3u + \ldots,$$

$$b_m = \frac{1}{\pi}\int_{-\pi}^{+\pi} f(cu/\pi)\cos mu\, du,$$

$$a_m = \frac{1}{\pi}\int_{-\pi}^{+\pi} f(cu/\pi)\sin mu\, du,$$

hold. Hence, for $x = -c$ to $x = +c$, the equation

$$f(x) = b_0/2 + b_2 \cos(\pi x/c) + b_2 \cos(2\pi x/c) + \cdots + \\ + a_1 \sin(\pi x/c) + a_2 \sin(2\pi x/c) + \ldots$$

holds.

The names of the variables in the definite integrals a and b do not matter, but if $cu/\pi = \lambda$ is put in then as a new variable, they take the form

$$b_m = \frac{1}{c} \int_{-c}^{+c} f(\lambda) \cos \frac{m\pi\lambda}{c}\, d\lambda,$$

$$a_m = \frac{1}{c} \int_{-c}^{+c} f(\lambda) \sin \frac{m\pi\lambda}{c}\, d\lambda$$

35. Extension in this way of the limits of validity of the development easily gives rise to the idea of extending these limits to infinity. Fourier, indeed, succeeded in representing by periodic functions a function $f(x)$ whose values are arbitrarily given from $x = -\infty$ to $x = +\infty$. If the coefficients a and b are substituted in the above series, we see, when we consider the well-known development of cos $(\alpha - \beta)$, that the series can be written in the following way:

$$f(x) = \frac{1}{c}\left[\frac{1}{2}\int_{-c}^{+c} f(\lambda)\, \mathrm{d}\lambda + \sum_{m=1}^{m=\infty} \int_{-c}^{+c} f(\lambda) \cos \frac{m\pi}{c}(\lambda - x)\, d\lambda\right]$$

or, if we take m in the limits $m = 0$ to $m = \infty$,

$$f(x) = \frac{1}{c}\left[-\frac{1}{2}\int_{-c}^{+c} f(\lambda)\, d\lambda + \sum_{m=1}^{m=\infty} \int_{-c}^{+c} f(\lambda) \cos \frac{m\pi}{c}(\lambda - x)\, d\lambda\right]$$

If c becomes very large, π/c becomes very small. If, then, m increases by one unit, we can regard $m\pi/c = p$ as continuously increasing and can put $\pi/c = dp$. If the first integral on the right hand side is finite, it vanishes on account of being multiplied by $1/2c$. For $1/c$ we write $(1/\pi)\,(\pi/c) = (1/\pi)\, dp$, and get, in place of the sum the definite integral

$$f(x) = \frac{1}{\pi}\int_0^{\infty} dp \int_{-\infty}^{+\infty} f(\lambda) \cos p(\lambda - x)\, d\lambda$$

The more detailed *mathematical* investigation of Fourier's expressions and also further examples which would require somewhat extensive calculations must be examined elsewhere.[42] Here we have been mainly concerned with showing in what manner Fourier's works were joined to the works of his predecessors and what important points of view he gained from them for his own investigations.

CHAPTER VII

THE DEVELOPMENT OF THE THEORY OF CONDUCTION OF HEAT

1. Fourier's theory of the conduction of heat may be characterized as an ideal physical theory. It is founded, not upon a hypothesis but upon an observable fact according to which the velocity of equalization of small differences of temperature is proportional to these differences themselves. Such a fact can be more precisely established or corrected by finer observations; but it can, as such, enter neither directly nor in its correct mathematical deductions into conflict with other facts. This foundation of the theory, with the entire structure supported by it, remains secure — while a hypothesis like that of the kinetic theory of gases, for example, which assumes molecules with evanescent reciprocal action and moved with great velocities in all directions, must be prepared at any moment for contradiction by new facts, no matter how much it may have contributed to the survey of the properties of gases up to that time.

2. The entire theory of Fourier really consists only in a consistent, quantitatively exact, abstract conception of the facts of conduction of heat — in an easily surveyed and systematically arranged *inventory* of facts, or rather in an introduction to the developing of this inventory from the above fundamental property, and to the fitting into it of each fact.[1]

Galileo reduced the entire mechanics of heavy bodies to the fact of constant acceleration of falling, and Newton recognized this acceleration as dependent upon the mutual distances of the bodies. Analogously, Fourier's theory is based upon the Newtonian principle of proportionality between difference of temperature and velocity of equalization. The conducting powers and capacities for heat determined the factors in the proportions, just as the masses do in the mechanical case. Distances with bodies gravitating toward one another, and temperatures with bodies of unequal temperatures tend to become equalized; only, in the former case, *accelerations* of equalization are determined by the differences of distance, in the latter, *velocities* of equalization are determined by the differences of temperature.[2]

3. In saying that every material point tends to the mean temperature of the surrounding points, the result of Fourier's theory is so expressed that it appears almost self-evident, and very close to our instinctive perception. It lies as close as the observation that all heavy bodies left to themselves sink. Science confirms, in both cases, an obvious fact, only more exactly and completely in all respects than involuntary and undisciplined observation is able to do. In mechanics and in the theory of conduction of heat it is, really, only *one* great fact in each domain which is ascertained.

Two contiguous bodies of unequal temperatures tend to their mean temperature which is determined by their capacities for heat. The velocity of change, $\partial u/\partial t$, of the temperature u of the point of a body whose temperature varies only in the x direction, is determined by

$$\frac{\partial u}{\partial t} = \left(\frac{k}{c\rho}\right)\frac{\partial^2 u}{\partial x^2},$$

and thus by its deviation from the mean temperature of the surroundings.[3] According as the temperature u lies above or below this mean temperature, it sinks or rises proportionally to its deviation from this mean. For the case of temperature varying in any manner from point to point in space, we imagine three straight lines drawn parallel to the coordinate axes through the point (x, y, z), and erect the temperature-ordinates perpendicular to each. The values $\partial^2 u/\partial x^2$, $\partial^2 u/\partial y^2$, $\partial^2 u/\partial z^2$, correspond to the curvatures of the three curves of temperature, or to the deviation of the temperature u of the point (x, y, z) from the mean temperature, in the three directions. The equation

$$\frac{\partial u}{\partial t} = \frac{k}{c\rho}\left(\frac{\partial^2 u}{\partial x^2} + \frac{\partial^2 u}{\partial y^2} + \frac{\partial^2 u}{\partial z^2}\right)$$

thus only repeats that u tends to the mean temperature of the surroundings with a velocity which is proportional to the deviation from this mean. For the stationary state

$$\frac{\partial^2 u}{\partial x^2} + \frac{\partial^2 u}{\partial y^2} + \frac{\partial^2 u}{\partial z^2} = 0,$$

that is to say, this state occurs if the above deviation from the mean is zero, or if every point has attained the mean temperature of the surroundings. The stationary (dynamical) state passes into a complete (statical) state of equilibrium if the flow of heat vanishes, so that

$$\frac{\partial u}{\partial x} = \frac{\partial u}{\partial y} = \frac{\partial u}{\partial z} = 0,$$

or u is constant.

4. The last equation but one, which bears the name of 'Laplace's equation' is, as is well-known, of great importance not only in the domain of the conduction of heat but in almost all domains of physics. This is due to the following circumstance. If we conceive u as the characteristic of a physical state of a material point (such as temperature, potential, concentration of a solution, velocity-potential, etc.), then every change of state, the continuance of a stationary process, equilibrium, is determined by the differences of values of u at the point (x, y, z) and the neighboring points. In a physical continuum, the behavior of every point is determined by the deviation of the values of its physical characteristic from a certain mean value of the characteristic of the neighboring points.

5. Let, in general, $u = f(x, y, z)$. For a neighboring point of the point (x, y, z), u is given by $f(x + h, y + k, z + l)$. If $\phi(\sqrt{h^2 + k^2 + l^2})$ denotes a function of the distance, to be ascertained in any particular case, which determines the weight of the neighboring points in mean value, and which, in general, diminishes, very rapidly with increasing distance, then the determining mean value takes the form

$$\frac{\iiint_{-\infty}^{+\infty} f(x + h, y + k, z + l)\,\varphi(\sqrt{h^2 + k^2 + l^2})\, dh \cdot dk \cdot dl}{\iiint_{-\infty}^{+\infty} \varphi(\sqrt{h^2 + k^2 + l^2})\, dh \cdot dk \cdot dl}.$$

If we develop f by Taylor's series up to the second powers of h, k, l, and integrate through all eight octants about the point (x, y, z), then, on account of the alternate signs, all terms affected with odd powers of h, k, l drop out, and there remains as the expression of the mean value,

$$u + \frac{m}{2}\left(\frac{\partial^2 u}{\partial x^2} + \frac{\partial^2 u}{\partial y^2} + \frac{\partial^2 u}{\partial z^2}\right).$$

Here m has the value

$$m = \frac{\iiint_0^\infty \varphi(\sqrt{h^2 + k^2 + l^2})\, h^2 \cdot dh \cdot dk \cdot dl}{\iiint_0^\infty \varphi(\sqrt{h^2 + k^2 + l^2}) \cdot dh \cdot dk \cdot dl},$$

which depends solely upon the behavior of φ. For the case of the conduction of heat we have $m = 2k/c\rho$. The deviation of u at the point (x, y, z) from the mean value of the surrounding is, as we see, given by the second part of the last equation but one. We notice at the same time that the employment of the form just referred to depends upon an approximation. If the value of φ diminishes more slowly with increasing distance, then the development up to the second differential quotients is not sufficient; it must be continued further. Further complications arise if the values of u itself have an influence upon those of φ, as Fourier considered possible and Forbes experimentally proved.[4] This explains the general *phenomenological* signification of Laplace's equation. That this equation is not confined to the narrower domain of physics I have elsewhere briefly shown.[5]

6. A scientific theory like the theory of the conduction of heat just considered, results from a double process: from the receiving of *sense-perceptions* by observation and experiment and from the independent *reproduction* of the facts of perception in thought. This reproduction, if it is to have a scientific character, must be communicable. But thoughts are only transferable when they are expressed in speech as images of generally known facts. We have, then, always to reproduce the results of observation, generally known facts of perception, by means of generally known and readily performed activities. Only seldom can this process be enacted entirely in the imagination; it is, for example, when we imagine the cooling of a hot body in cold surroundings or the formation of red cinnabar from metallic mercury and yellow sulphur. In the determination of the refraction of light by a geometrical construction, we imitate the *physical* fact by geometrical ones, which emerge when a readily performed muscular activity is exercised on known geometrical objects. Likewise, the representation of the cooling process by a geometrical progression is based ultimately upon a readily performed reckoning or counting operation which is undertaken with the degrees of the thermometer; thus it also is based upon a muscular activity (a directing of the eyes, marking, naming of the degree, etc.).

7. Our behavior in the domain of science is merely a copy of our behavior in organic life generally. We react to qualitatively different, definite stimuli with qualitatively different sensations and movements; the latter are partly organically formed in advance (motions of tasting and swallowing), partly acquired by personal experience (and here memory comes in) — as when we shrink back from a red-hot body. In the association and conflict of such reactions, which in their *elements* are reflex actions organically formed beforehand, consist organic and intellectual life. The nature of the process is not changed if the images we form of the reactions are converted into movements; only the intensity and the scope of the process has been augmented.

In the simplest organisms, all reactions serve *directly* the preservation of favorable conditions of life; what excites the corresponding sensation of taste is swallowed. With fuller development, a reaction may serve as means to a further end. The sight of an object recalls its taste; the taste gives rise to the desire to seize the object. But this end is often only attainable by a series of intermediate reactions.

All processes by which scientific results are gained have the nature of such (intellectual) intermediate terms necessary for the attainment of an (intellectual) end in life. In the simplest cases, we have to do with this state of things: by the property A of a sensuous fact, the idea or expectation of another property B is aroused, which determines our further practical or intellectual behavior. Mental development consists in the progressive association in memory of such connected properties. In many cases this association, on account of the complication of circumstances, cannot take place of itself involuntarily, but the discovery of the (sensuous) properties that belong together is itself the result of a reaction which is discharged by interest in the end; the properties are *sought.*

Those sensuous properties which make their appearance through such an intellectual or practical reaction are the properties of a concept. The testing or constructive employment of the concept consists in the performance of that wholly concrete reaction by which the properties concerned become manifest in a given fact, or by which a fact with those properties is represented. The concept "statical moment" may serve as an example of this.[6]

8. In regard to isolated facts, there is nothing to do but to retain them simply in memory. If entire groups of interrelated facts are known such

that the two connected propeties A and B belonging to them each form a *series* whose terms differ only in the number of equal parts into which they may be resolved, then a more convenient survey and notional representation may be obtained. The angles of incidence (A) as well as the angles of refraction (B) of a series of incident rays; the temperature-excesses (A) as well as the temperature-losses per minute (B) of cooling bodies may be resolved into equal parts; and to every term of series A corresponds a term of series B. A systematically arranged table can now facilitate the survey by assisting or replacing the memory. *Quantitative* investigation begins here; and it is, as we see, a special case of *qualitative* research, applicable only to series of facts of a particular kind of relationship.

9. We gain a new facility if the entire table can be replaced by a compendious *rule for reconstruction*: if we can say, for instance: multiply the temperature excess u of the cooling body by the coefficient μ and you obtain the temperature loss per minute (μu). If such a rule or formula of reproduction is closely examined, it is seen to contain merely an impulse to a concrete reaction which, stimulated by A, produces B whose quality is always the same but whose extension is determined by A, so that the reactions themselves also form a series (well-known and practised) analogous to A and B. The formula $a + b$ produces the impulse to the concrete further counting from a on, and only the extent of this activity is determined by b. The case is analogous and not essentially different with complicated formulas.

10. After the above explanation, it cannot seem strange that apparently remote facts and thoughts which were familiar from use in other investigations were drawn upon for the representation in thought of the phenomena of the conduction of heat. Here the ideas gained by considerations of the vibrations of strings play the most important role. The observation of a rope swinging slowly in the simplest manner must have suggested to Taylor the idea of considering the separate points of the rope as *synchronous* pendulums and of determining a *feeble* sine-bending as the condition of this behavior. The accelerations and velocities of each of two points of the rope then stand in the same ratio as the displacements belonging to them; and all displacements change, therefore proportionally to one another. For a sine-form distribution of temperature an analogous simple relation holds: here all temperatures

also change proportionally to one another. But in this Fourier introduced a clear, and to him already thoroughly familiar state of affairs, into the theory of heat.

Sauveur examined the distribution of nodes on the string; Daniel Bernoulli represented the case analytically as a combination of Taylor's vibrations, and recognized the variety of form of the motions which are produced in this way. Fourier also utilised this knowledge and treated more complicated distributions of temperature as constructed – out of simple (Taylorian) components — and their behavior now becomes just as clear as in the more simple case.

Only the study of vibrating strings could suggest the idea of representing the form of the string between two nodes of distance l by a series of the form

$$a_1 \sin \frac{\pi x}{l} + a_2 \sin \frac{2\pi x}{l} + a_3 \sin \frac{3\pi x}{l} + \dots$$

where the same form must be exactly repeated between the 0th and 1st, 2nd and 3rd, 4th and 5th, . . . nodes, and between the 1st and 2nd, 3rd and 4th . . . , nodes in centrally symmetrical reversion. Fourier availed himself of such series, with an infinite number of terms, for the representation of any function whatever. Functions with the same value for equal positive and negative values of the argument are naturally represented by cosine series and functions with more general properties by the sum of sine series and cosine series. By conceiving the distance between two nodes as increasing up to infinity Fourier was able to represent any function whatever, throughout any range, by the double integrals into which his series are then converted.

11. By the conception of any of the more complicated distributions of temperature as the algebraic sum of more simple distributions, Fourier's representation gains an extraordinary clearness upon which Fourier himself laid the greatest value. With this is bound up his conviction that the method is generally applicable to the treatment of any possible case with sufficient accuracy. All this is attained when we allow the facts of conduction of heat to be represented in thought by a *function* better known to us than these facts, and exhibiting the essential properties of them.

Fourier followed the method which led Galileo to the understanding of the motion of projectiles. He attempted to understand a process which cannot be grasped at a glance by resolving it step by step into more easily surveyed component parts.

12. The favorable influence which investigations in different fields exert upon one another stands out with particular clearness in the theories considered here. Physical observations stimulate mathematical investigations, and these latter again react upon the former. The theory of heat is promoted by the theory of vibrating strings. The conceptions of electric current by Ohm and of diffusion of liquids by Fick are imitative of the theory of conduction of heat; so that we can, to-day, develop an entirely general theory of currents, in which hydrodynamical, thermal, electrical, diffusion and other processes, are included as special cases.

13. To any one who has come thoroughly to know Fourier's theory, it appears as a great achievement. But if we remember of what simple elements it is constructed, and how these elements have been accumulated, laboriously and with many errors by different distinguished men during an interval of more than a hundred years, we can well believe that this edifice, under more favorable outer and psychological circumstances, could certainly have been erected in a very short time. From this, we see that even the eminent intellect is more adapted to living conditions than to research.

CHAPTER VIII

HISTORICAL SURVEY OF THE THEORY OF RADIATION OF HEAT

1. The observation that there is a reciprocal action between the temperatures of neighboring bodies is so immediate and so evident that information as to when and where it was first made is scarcely conceivable. Warmer bodies cool by communicating "heat" to cooler surroundings; and Newton was the first to formulate a law to be discussed later on concerning this communication. It was only gradually discerned that, in it several very different kinds of process are combined. Contiguous bodies mutually change their temperatures; this process, in particular, we will call "communication". If differently heated parts of one and the same homogeneous body are involved, we will call this communication "conduction", and we may remark that an accurate investigation of this process took place comparatively late. If the warmer body is immersed in a liquid of which the parts in contact with the body are heated by communication, their density and specific gravity alter, and *currents* appear in the fluid, owing to the disturbance of the equilibrium of gravity, which promote the reciprocal action of the temperatures. This process is called propagation of heat by "convection". J. Black[1] treated convection in a perfectly clear manner.

2. But that mode of propagation of heat which must have struck people first of all is what we call "radiation". The instantaneous heating by the sun when it comes out from behind a cloud, as well as the equally rapid cooling when a cloud passes before its face, leaves no doubt as to the great velocity of heat propagation of this kind. In addition to this, the properties of burning mirror and burning glass, which undoubtedly were accidentally observed, show the inherent connection between heat and light so clearly that knowledge of it can only be obscured by later theoretical prejudices. Kircher[2] mentioned the ancient burning mirror and recounted the well-known tradition of the burning mirror of Archimedes.

3. Systematic experiments which are worthy of mention were made with large burning mirrors and burning glasses by Tschirnhausen.[3] The

lenses, which he made for these experiments by moulding them, were 100 to 130 cm in diameter. The concentration of the sun's rays was increased by the employment of two lenses, one behind the other. At the focus, wet wood was burnt up, water in a small vessel was brought to boiling point, lead and iron were melted, minerals were vitrified. Sulphur and pitch melted under water; wood under water became charred inside; and bodies inserted in coal were much more intensely affected, and metals were successfully volatilized in this way. This showed the greater absorption of heat by *black* bodies. Copper melted in this way and thrown into water burst the earthen vessel by the resulting explosion. Colored glass fluxes were made by means of the burning glass; and finally, proof was furnished that moonlight produces, in the focus, no perceptible heat.

The name "radiant heat" appears to be due to Carl Wilhelm Scheele.[4] He observed that smoke rises to a distance of ten feet from a fire; but that the radiation, felt at this distance from the open door of a stove, is not affected by a current of air passing between. A glass plate set up between keeps off the heat but not the light. The burning mirror burns without itself becoming heated; but if it is covered with soot, heating does occur. The heat rising through the chimney is to be distinguished from that issuing from the door of the stove, and the former is contained in the air quite differently from the latter. Air irradiated by heat shows no shadow-marks (*Schlieren*) even in the sun, as heated air does.[5]

5. Lambert[6] made many experiments on heating bodies at the fire, and the effect of "fire rays", and the sun's rays; and to his mathematical treatment of the process we will return. The laws of the propagation and reflexion of fire rays, are, in his view, the same as those for light rays.[7] Accordingly, he developed his propositions concerning the effect of the burning mirror from the fundamental principles of optics.[8] Lambert expressly remarked that "dark heat" also can be reflected.He employed two coaxial concave mirrors for experiments on radiation. The influence of black color upon radiation was known to him.[9]

6. Marc Auguste Pictet[10] placed two large concave tin mirrors coaxially opposite one another and introduced into the focus of the one a hot body and into the focus of the other a thermometer with the bulb sooted. Even at a distance of 23 m between the two mirrors, the

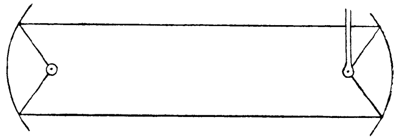


Fig. 48.

thermometer immediately began to rise without a time that would be necessary for propagation being perceptible. He therefore distinguished *radiant* (*rayonnante*) heat from *propagated* (*propagée*) heat, and was of the opinion that only the latter proceeds slowly from particle to particle, while the former, which traverses the space between the particles, travels in straight lines and in every case with considerable velocity, perhaps as quickly as sound or even as light.

But Pictet did not reach a clear idea on the distinction between radiation and conduction. Since he was not successful in collecting, with a glass lens, the heat of a vessel filled with boiling water, he hoped to succeed with a metal lens. Thus, he believed that good conductors were good transmitters of heat-rays.

7. Through a conversation with Bertrand, Pictet[11] was induced to undertake an experiment on the *radiation of cold.* The hot body of the above experiment was merely replaced by a vessel with snow or a freezing mixture of snow and saltpeter; whereupon, to the surprise of Pictet, the thermometer suddenly fell. Yet Pictet soon explained the occurrence and recognized that here the thermometer is the warmer body and loses its heat to the colder body — the freezing mixture. A similar experiment had been performed by the Accademia del Cimento, but the authors themselves regarded it as indecisive. The experiment is historically important, because it suggested to Prévost an entirely new conception of the equilibrium of heat which we shall have to discuss later on. By the experiments of James Hutton (1794) Rumford (1796), Leslie (1799), Herschel (1800), Nobili (1830), Melloni (1831), Forbes (1835), Knoblauch (1847), and others, the identity of the rays of light and heat and their agreement in all physical properties was gradually more clearly and more completely established.

8. Pictet was of the opinion that *fire* behaves very similarly to light, yet light may be present alone, as is the case with moonlight, and also heat alone, as is the case with the "dark heat" of Lambert. Bodies of higher

temperature contain the heat in a “higher state of tension”. The equalization by radiation is an equalization of the tensions of heat. According to Prévost’s hypothesis, of which we shall speak again, warm bodies throw off warm particles with great velocity in straight lines to one another. According to Hutton,[12] radiant heat does not differ from light. He knew that a red-hot body heats a thermometer more intensely than the white light of a candle flame; and according to him, a dark body continues to radiate even when the radiation is not perceptible to the eyes. The hot body transforms its heat into light; and the light, by absorption, can again become heat. Hutton’s investigation is very clearly expressed. For Rumford[13] heat consists in vibrations; he compared the radiating body with a bell, and — but in a way difficult to understand — the warmer body with a more quickly vibrating body the colder with a more slowly vibrating body. The temperature would, accordingly, be dependent upon the time of vibration. Sir John Leslie[14] reduced radiant heat to pulsations of the air. The layer of air in contact with the body take up the heat and give it by impacts to the successive layers. This idea is surprising because Boyle had already observed in 1680 that the burning glass acts in the vacuum produced by an air pump. Leslie was led to this view by the circumstance that he was able to keep off the radiant heat by a *thin* metal screen, and this seemed to him inconsistent with a more subtle nature of heat. Herschel[15] discovered thermal activity in the infrared part of the spectrum of the sun by allowing this spectrum to fall upon a lens, shutting off the visible part by a diaphragm, and introducing a thermometer into the focus. Since, therefore, the optical action and the thermal action do not run parallel, the thought occurred to Herschel that every ray is composed of a luminous ray and a calorific ray. Gradually Nobili, Melloni,[16] and their successors proved the complete agreement of the rays of heat and light with respect to reflexion, refraction, interference, and polarization. There are, accordingly, only rays of *one* kind, which may be of different wavelength and intensity, and merely on account of this appear more prominently sometimes in optical and sometimes in thermal or chemical action, and also have definite *physiological* properties.

9. The general views which the investigators mentioned reached have just been set forth. But now we must pass in review the more important particulars of the experience which they gained by means of their researches. Rumford[17] worked, as he stated, and as is indeed likely

from his achievements, at about the same time as Leslie and independently of him.[18] Rumford[19] [with the object of determining whether the invisible heating rays which a warmer body — such as a heated stove — gives out are not of the same character as those coming from the sun], procured boxes of the same size, open at the top; fitted thermometers in the boxes through cork stopppers in the centers of the bottoms; and filled the boxes in exactly the same way with silver wire, to secure uniformity of heating. The tops were formed of metal discs of brass, tinned iron, and ordinary sheet-iron respectively. Rumford was not at all surprised to find that the rays of the sun excited more heat in a given time on the black and unpolished iron disc than on the other two bright and polished discs, but was astonished to find that the box with the iron disc cooled the most quickly of all.[20] The experiments were repeated before a stove instead of the sun, and they gave the same results. After several modifications of the experiments with improved apparatus, among which was the differential air thermometer,[21] Rumford remained of the opinion that there are not only heating rays which accelerate the vibrations, but also cooling ones which retard the vibrations.[22] The results may be summarized in the following propositions.[23] (1) All bodies radiate at every temperature; (2) The intensity of radiation is different at the same temperature (for example, it varies as 1:4:5 for bright, oxidized, and sooted brass); (3) At the same temperature, bodies are not influenced by the mutual radiations.

10. Leslie performed a great number of good experiments. He constructed the *cube* named after him, a tin vessel covered on three vertical faces with soot, paper, and glass, respectively, while only one was left bright. He used large parabolic tin mirrors for the reflexion of heat, and different screens for its interception. With a differential air thermometer, one of whose bulbs was placed in the focus of the heat rays, he observed the rise of temperature. The irradiated bulb was sometimes wrapped in tin-foil or blackened with indian ink. Enclosed in a glass tube, this air thermometer served also as a photometer. The rays emanating from a surface of the cube fell upon the concave tin mirror and converged after reflexion upon the bulb of the thermometer. The effects of the heat emanating from the surfaces of the cube which were respectively covered with soot, paper, glass, and tin, when the cube was filled with hot water, were as 100:98:90:12. Analogous experiments succeeded with *cold*, and the radiations of cold were found

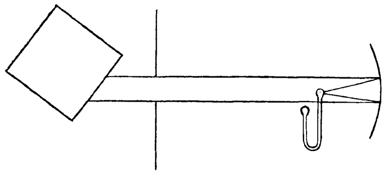


Fig. 49.

to stand in the same relation. From this Leslie[24] concluded that absorption and emission of heat increase and decrease together. If a surface of the cube was placed obliquely to the axis of the concave mirror and allowed to radiate through the openings of a screen of tin-plate, the action was found to be independent of the obliquity.[25] Since, now, with a given breadth of opening, the radiating surface coming into consideration is greater in the oblique position, the intensity of the rays leaving obliquely must be less. Later on, Leslie[26] mentioned that the luminosity of a shining surface is not altered by a position which is oblique to the line of vision, just as a red-hot ball does not appear brighter at the edge than at the middle; and he concluded from this that the intensity of the rays of light is proportional to the cosines of the angles the lines of departure make with the perpendicular to the radiating surface. These are the views set forth by Lambert in his *Photometria* a work known to Leslie.[27] Leslie further remarked that reflexion and emission of heat mutually supplement one another, as strongly reflecting surfaces showed a feeble emission of heat. Some interesting observations were concerned with the strong diminution of the mobility of the air in thin layers,[28] between cylinders placed in one another, and the resulting small permeability for heat; and with the special conductive power of hydrogen which is shown by the rapid cooling in this gas.[29] Prévost did not make many observations of his own; but, in the essay to be spoken of later, he utilised in an admirable theoretical manner the knowledge gained up to his time. A considerable part of his book consists of translations and summaries of the works of Rumford, Leslie, and others.

11. Newton was the first to express a theoretical view of the process of communication of heat, on the occasion of his attempt to compute high temperatures. He said: "For the heat which the heated iron communicates in a given time to the cold bodies in contact with it, that is, the

heat which the iron *loses* in the given time, behaves like the *entire* heat of the iron. Hence, if the periods of cooling are taken equal, the amounts of heat will stand in geometrical ratio and are, therefore, easy to find with the help of a table of logarithms.[30] This passage can only be understood from the whole context in the following way. Newton put the temperature *losses* in equal times proportional to temperature *excesses* of the hot body above the surroundings. No indication is yet to be found of a separation of the ideas temperature and quantity of heat, radiation and conduction. The correction which Dulong and Petit have applied to the law will be discussed later.

12. Lambert[31] attempted to solve various problems according to the Newtonian principle. If u is the temperature excess of a body above the surroundings, t the time, then he put

$$du = -a \cdot u \cdot dt,$$

from which follows by integration

$$u = Ue^{-at},$$

where U is the (initial) temperature-excess corresponding to the limit $t = 0$. The quantity $1/a$ was called by Lambert, from its geometrical significance, the "subtangent of cooling". It is inversely proportional to the velocity of cooling and represents the time in which the body would lose its entire temperature excess, if the velocity of cooling of the first diminutive portion of time were retained throughout. Lambert knew that a depends upon the capacity for heat, the surrounding medium, and the nature of the surface of the body.[32]

For a body which is exposed to heating by an invariable source of heat and, at the same time, to cooling by the surrounding medium, there obtains according to Lambert the easily comprehensible equation

$$du = k \cdot dt - a \cdot u \cdot dt,$$

which, by integration, gives

$$u = \frac{k}{a} - \left(\frac{k}{a} - U\right) e^{-at},$$

in which U again denotes the initial difference of temperature of the body considered, as compared with the surroundings.

By means of this equation, the maximum value of u may also be found. In an analogous way, the course of the variations of temperature arising from the reciprocal action of several bodies was examined. Such an example will be mentioned later. From the formula here considered it results that an equalization of temperature is, strictly speaking, only attained after an infinitely long time.

13. Lambert had a very lively constructive imagination, and gave a stimulus to all domains by his ingenious treatment of the subject-matter. He always endeavored to reconstruct all phenomena by *mathematical* conceptions. As an example it may be mentioned that he tried to determine the resistance of strings to rupture from the sound which they gave immediately before the breaking. In this, he calculated from the formula $p = qln^2/2g$, in which p denotes the tension, q the weight of the string, l the length of the string, n the number of oscillations, and g the acceleration of falling.[33]

In another place,[34] he compared melting with breaking, and, from the loading necessary for breaking, the extension corresponding to it, and the known lengthening from known increase of temperature, he inferred the melting temperature — a temperature which would produce the extension of breaking. Lambert's inclination to schematize sometimes led him astray. Thus he assumed, for example, that sound behaves exactly like light with regard to refraction and reflexion, based upon it a false theory of the speaking tube, and in so doing effaced a distinction already clearly recognized by Newton. While Leslie recognized Lambert's happy gift, yet he[35] regretted that Lambert so often built far-reaching conclusions upon inadequate observations. Lambert's universality led him also into the field of philosophical investigations,[36] where his wish to solve everything by mere reflection operated still more detrimentally. He believed it possible, for instance, to deduce the impenetrability of matter from the principle of contradiction alone, whereupon Kant[37] remarked: "But the principle of contradiction does not preclude any matter from advancing in order to penetrate into a space in which another body exists". In fact, we can drive ideas from the head with this principle but not bodies from space. This will serve for the purpose of a characterization of Lambert, and we may add that we shall meet him yet again in the following pages as a contributor to the theory of heat.

14. Pierre Prévost clearly distinguished radiation from conduction of heat. After a short discussion of the material and kinetic theories of heat in general, and of the emission and wave theories of radiant heat in particular, he declared[38] that he did not wish to occupy himself with the discussion of these systems; what concerned him was the elucidation of the facts, in so far as this was possible. He preferred for his own use the manner of expression of the emission theory; and formed his ideas of particles of material of heat thrown off from hot bodies in imitation of the kinetic theory of gases of Daniel Bernoulli and G. L. le Sage. Led by Végobre's remark that Pictet's experiment on the radiation of cold was not sufficiently explained, he attempted to apply his mode of conception to this case, and thus arrived at his idea of *mobile equilibrium of heat* which he expounded in three different publications.[39]

He imagined that heat is composed of discrete particles, which are very small compared with the distance between them, moving with great velocity in different directions and very seldom colliding. Every point of space or of the surface of a hot body may be regarded as a center from which particles of heat proceed in all directions, and to which they come from all directions. Thus threads (*filets*) or rays of particles of the material of heat cross at every point.[40] Two portions of space are in thermal equilibrium if they send each other an equal number of particles of heat in equal times. If the state with respect to heat of a body does not change, this is due, according to Prévost, to the fact that it receives just as many particles of heat as it gives out in the same time. "It is like a lake into which rain falls while, at the same time, an equal quantity of water evaporates.[41]

The double mirror experiment of Pictet was explained by Prévost both for the case of heat rays and for that of cold rays in an equally simple manner. Two equally hot bodies in the two foci exchange equal quantities of heat. If one of the two is warmer than the other, the one sends a greater quantity of heat to the other than it receives from it, and the latter continues to radiate its previously emitted heat.[42]

Thus, it is not necessary to imagine now one and now the other body as radiating, but both may be conceived as radiating continually whether they are equally or unequally hot. Moreover, on this point, Hutton[43] had directed attention to the fact that the assumption of a single radiation is not sufficient, as it would have to be thought of as depending on the state of the irradiated body.

Prévost attempted to apply this view to all the facts ascertained by Pictet, Rumford, Leslie, and others. He brought the parallelism between emission and absorption into connection with reflexion; and he regarded all unabsorbed heat as reflected. Good reflectors, that is to say bodies which absorb little, also retain their own heat well by reflexion on their surface, and thus are the kind of bodies that emit heat feebly.[44]

As glass keeps off dark heat but allows light to pass through, Prévost supposed that there are two or more kinds of heat particles, and thus suspected the facts ascertained later by Melloni and others.[45]

15. The principles discovered may be summarized in the following way.[46]

(1) Every point of the surface of a body is a center of rays emanating from it and meeting in it;

(2) Thermal equilibrium consists in equality of the exchanges of heat;

(3) If the time increases in arithmetical progression, the differences of temperature vary in geometrical progression;

(4) In a portion of space of uniform temperature, a reflecting surface — since it reflects only surface elements of the same temperature — has no influence in changing the temperature;

(5) But if a warmer or colder body is introduced, the temperature of those bodies upon which the rays emanating from the first body are directed by the reflecting surface is changed;

(6) A body which reflects well assumes more slowly the temperature of the surroundings;

(7) A warm or cold body which reflects well influences less another neighboring body.

A part of Prévost's book is devoted to meteorological and climatological investigations which we shall not consider here.

16. Fourier, the founder of the theory of conduction of heat, seems to have been the first to give the different special experiments on radiant heat into a stronger theoretical connection, by recognizing them as necessary conditions of the equilibrium of radiation.[47] Without going into all the particulars of Fourier's extensive investigations, this connection may be explained in the following way.

The equilibrium of radiation of neighboring bodies of equal temperature is a fact abundantly verified. If the temperature of one of the bodies is raised in any way, the temperatures of the other bodies also gradually rise. The radiation thus increases with the temperature of the radiating bodies. This is a result of the work of Pictet and Prévost.

Since the unit of surface of different bodies of the same temperature has a very different intensity of radiation (Lambert, Leslie, Rumford), actual equality of temperature between two different bodies — for instance, of two with parallel plane surfaces — could not subsist unless the body with half the intensity of radiation were to absorb only half the heat falling upon it in the same time and at the same temperature. The proportionality of emission and absorption is thus a necessary condition of equilibrium of radiation with equality of temperature.

17. This relation was demonstrated by an experiment of Ritchie.[48] Between two equal vessels A, B, which are connected with one and the same differential air thermometer, stands a third vessel C filled with hot water. The surfaces turned towards one another are, as is indicated in Fig. 50, of bright metal (_______) or covered with soot (– – – – –). The thermometer shows no difference, from which it follows that the stronger radiation from C to B is compensated by a more feeble absorption of B, the more feeble radiation from C to A by a stronger absorption of A.

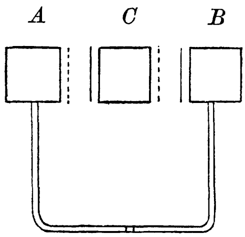


Fig. 50.

18. The law (Lambert, Leslie) according to which the intensity of radiation of a surface is proportional to the cosine of the angle that the direction of departure of the rays makes with the normal to the surface or to the sine of the angle of inclination of the ray towards the surface — of the angle of emission —, likewise appears as a necessary condition of the equilibrium of radiation. Imagine that two homogeneous bodies

Fig. 51.

of the same temperature irradiate one another solely through the two small portions of the surfaces, f, f', from a great distance, where the whole section of the bundle of rays proceeding normally from f must be exactly filled up by f'. If the law mentioned did not hold, then, if the intensity of the radiation in all directions were equal, f must receive more heat than it gives up to f' in the same time, and the thermal equilibrium would instantly be disturbed. But this equilibrium continues in force if f' radiates just as much in the oblique direction as its projection upon the plane perpendicular to the direction of the rays — that is f' sin — does in the normal direction. The intensity of the bundle emanating from a surface-element of a definite body of given temperature in any direction is then determined solely by the cross-section of this bundle. It is then clear that the irradiation of a small sphere K, which is contained in an enclosure H of given temperature and given material, may be replaced by the radiation of a hollow sphere SS concentric with K and of the same temperature and the same material as H. The sphere K, accordingly, is irradiated in the same way at every

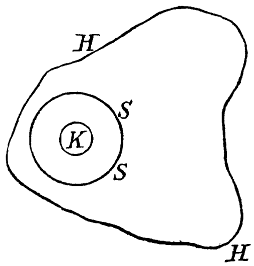


Fig. 52.

place in the hollow space. If, on the contrary, we assume that the intensity of radiation of the points of the surface of H is independent of the direction, then, as we easily find with Fourier, the intensity of the irradiation which K experiences, and therefore its temperature of equilibrium, is dependent upon the position of K inside the space H.

19. Fourier also attempted to explain *physically* why the intensity of radiation is proportional to the sine of the angle of radiation. He assumed that the rays from a certain depth penetrate the surface. But, at a given depth of the radiating particle, its rays have to penetrate a thicker absorbing layer the more obliquely toward the normal they are emitted. This point will not be discussed further here. As Zöllner[49] has remarked, the milk-glass (porcelain) globe surrounding a gas flame, which appears equally bright over its entire expanse, is a good illustration of Fourier's view. The equally irradiated particles at the same depth under the surface here also radiate through an absorbing medium.

20. A more pronounced development of the ideas concerning the equilibrium of radiation was brought about by a series of peculiar observations. Fraunhofer[50] discovered the lines in the solar spectrum named after him. Brewster[51] discovered the monochromatic nature of the light of the flame in which common salt is placed and the absorption bands of vapor of nitrous acid gas, and, briefly, the selective emission and absorption with respect to light of different colors. By the investigations of Angström, Plücker, and others, observations relating to this subject were greatly multiplied. To the older observations concerning the impermeability of glass to "dark" heat were added the later experiments of Melloni[52] on the perviousness of bodies for different "heat colors." It could no longer be doubted that every body behaves individually with regard to every wavelength of radiation.

21. Foucault had observed that the electric arc light sends out light corresponding to the Fraunhofer D-line and also absorbs chiefly the *same* light. Kirchhoff,[53] as he was examining more closely the coincidence of the dark D-line of the solar spectrum with the clear line of the sodium flame by pushing the latter flame before the slit of the spectroscope, noticed a marked strengthening and darkening of the D-line of the solar spectrum. Thus the fact again emerged that a body absorbs chiefly the same light that it emits in radiation. But while different investigators connected their researches with some of Euler's, and endeavored to explain this and similar facts according to the principle of resonance (Stokes, Angström), Kirchhoff divined in it the trace of a general and important law of the theory of heat. This is, apart from the application of the principle recognized to the analysis of the

light of the stars, the essential distinction between his intellectual attitude and that of his predecessors. Kirchhoff in fact ascertained that the proportionality between absorption and emission must hold with respect to *each* particular wavelength if the equilibrium of radiation of bodies of equal temperature is to subsist.

22. Without entering into too many details, we can acquaint ourselves with Kirchhoff's manner of thinking by the following considerations.[54] A body M is supposed to stand opposite to a body N of the same temperature, so that the two infinite, parallel boundary planes are turned towards one another. Let the surfaces of the bodies which are turned away from one another be covered with reflectors S and S'

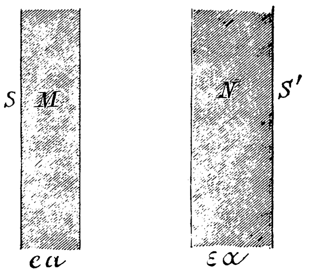


Fig. 53.

which throw back all rays. The total amount of heat which the unit of surface of M radiates in the unit of time is called the "emissive power" of M and denoted by e. That fraction of the radiant heat falling upon M which is absorbed is called the "absorption power" of M and denoted by a. The analogous quantities for N may be called ε and α.

The body M emits e from the unit of surface, and of it the quantity $e\alpha$ is absorbed by N, and $e(1-\alpha)$ sent back to m. Of this M absorbs $e(1-\alpha)a$ and sends back $e(1-\alpha)(1-a)$ to N. From $Ne(1-\alpha)(1-a)(1-\alpha)$ is returned to m, and of it M absorbs $e(1-\alpha)a(1-a)(1-\alpha)$. If we continue the process and denote the factor $(1-\alpha)(1-a)$ by k, then it appears, that M keeps back of its own radiation the amount

$$e(1-\alpha)a(1+k+k^2+k^3+\ldots)=\frac{e(1-\alpha)a}{1-k}.$$

The emission of N is ε, of which m takes up the amount εa and returns to N the amount $\varepsilon(1 - a)$; of this N absorbs $\varepsilon(1 - a)\alpha$, and sends $\varepsilon(1 - a)(1 - \alpha)$ back to m, and M keeps $\varepsilon a(1 - a)(1 - \alpha)$ of it. The continuation of the process shows that M receives, in all, from N

$$\varepsilon a(1 + k + k^2 + k^3 + \ldots) = \frac{\varepsilon a}{1 - k}.$$

If the temperature of M is to remain unchanged, the total amount received must be equal to its own radiation, that is

$$\frac{e(1 - \alpha)a + \varepsilon a}{1 - K} = e.$$

If we substitute the above value of k, we get

$$\varepsilon a = e\alpha, \text{ or } e/\varepsilon = a/\alpha, \text{ or } e/a = \varepsilon/\alpha.$$

The same condition follows obviously if we start from the assumption of the invariability of the temperature of n. If the radiant heat is considered as a whole, then, for the preservation of the equilibrium of radiation, it follows that the absorption power must be proportional to the emission power.

We will now suppose that the body M is perfectly transparent for all wavelengths with the exception of λ'. On the other hand, λ' is supposed to be absorbed and radiated by it. Experience teaches that bodies with such properties exists. In this case, N, on account of the reflectors S and S' will receive back entirely its own radiation with the exception of that of wavelength λ'. But for M only the wavelength λ' comes into consideration. Thus, if the equilibrium of temperature between M and N is to continue to subsist, the above developed condition must hold for the radiation of wavelength λ' in particular. We see that each particular kind of radiation could destroy the temperature equilibrium if the proportionality between absorptive power and emissive power for all bodies (of the same temperature) did not subsist for every simple kind of radiation. Thus, if a series of bodies with the emissive powers $e, e', e'', \ldots$ and the absorptive powers $a, a', a'', \ldots$ is given, then, for the same wavelength and temperature,

$$e/a = e'/a' = e''/a'' = \ldots.$$

The theorem of Kirchhoff was derived under the supposition of complete equilibrium of temperature, and is valid only under this condition. E. Wiedemann[55] has investigated the deviations which occur when this condition does not hold.

23. Kirchhoff specialized his observations still further. Since the absorptive power for *polarized* rays depends, in many bodies, upon the position of the plane of polarization, a disturbance of the equilibrium of temperatures by polarized rays could occur if the emissive power were not dependent in the same way upon the azimuth of polarization. Kirchhoff and Stewart[56] have independently demonstrated by experiment that a tourmaline plate which absorbs rays polarized perpendicularly to the axis also emits them in the same plane when it is in a red-hot state.

24. If the temperature of a body K, which up to this time was in equilibrium of radiation with other bodies, is increased, the temperatures of the neighboring bodies also rise. According to the theory of mobile equilibrium, this is comprehensible by the assumption that the emissive power (and therefore also the absorptive power) of K increases with the temperature.

If, with Kirchhoff, we imagine a "perfectly black body", that is, one that absorbs *all* light falling upon it, as soot nearly does, and call the emissive power and absorptive power for it e and a respectively, and for any other body (K) respectively E and A, then, for the same wavelength and temperature the equation

$$E/A = e/a = e,$$

holds, since, for the black body, a is to be put equal to unity. We will write this in the form

$$E/e = A.$$

If we take e as unit of measure and call E/e the "relative emissive power" of the body K, referred to that of a black body for the same wavelength and temperature, then this power is always equal to the absorptive power of the body K. Since $e = F(u, \lambda)$, the emission of the black body depending upon the temperature u and the wavelength λ, we have, for any other body,

$$E = F(u, \lambda) \cdot A.$$

As observation of the absorption spectra shows, A depends upon the wavelength. On the other hand, the temperature seems to have only a slight influence upon A. Transparent colorless bodies retain this property, as a rule, even at high temperatures, colored bodies remain colored, opaque bodies remain opaque. In general, therefore,

$$A = \varphi(u, \lambda),$$

where changes of u involve only slight changes of A, which we will for the present neglect. If we heat a piece of platinum gradually, it first sends out dark and then red rays. With further increase in the temperature, the spectrum of the emitted light increases towards the violet side; shorter and shorter wavelengths become noticeable in the radiation. Since platinum, like soot, is opaque for all wavelengths at every temperature, that is to say, since its A is throughout different from zero and comparatively large, the values of E and e must both, under the same circumstances, be different from zero. If the heated soot begins to send out a wavelength, then platinum must also do the same, and just so all other equally heated opaque bodies.

25. This conclusion is also confirmed by an observation of Draper.[57] The most diverse bodies, enclosed in the barrel of a gun and gradually heated, send out at first only dark heat. By sufficiently raising the temperature, all simultaneously begin to shine (to glow). Under continuously increasing temperature, the spectrum of their light extends, for all the bodies, towards the violet side.

For transparent bodies, A is either zero or very small. Hence they glow at the same temperature more feebly than opaque bodies. Glass and iron come to red-heat at the same temperature, yet glass shines much more feebly.

A black body has, for visible light, a much higher absorptive power than a white body. If this property continues at higher temperatures, the black body must glow more intensely than the white. An ink-spot upon a sheet of platinum glows more brightly than the platinum, a chalk-spot upon a black poker glows less brightly than the poker. If a common earthenware plate with a black and white pattern on it is made white-hot (Figs. 54 and 55), we see, instead of a dark pattern on a white ground, a white pattern on a dark ground; the negative of the pattern appears.[58]

26. Should the emissive power rise proportionally to the temperature, Newton's law of cooling mentioned above[59] would follow from it. But according to the experiments of Dulong and Petit,[60] it is only for *small*

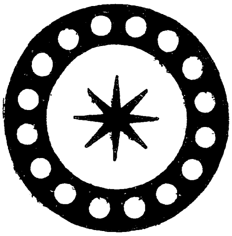


Fig. 54.

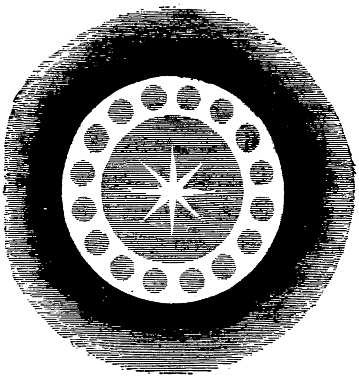


Fig. 55.

temperature excesses that the velocity of cooling is proportional to the excesses, while for greater temperature excesses it increases more rapidly than they. From this, Dulong and Petit concluded that the intensity of radiation is another function $F(u)$ of the temperature u. If the temperature of an airless, hollow spherical enclosure is θ, and t is the temperature excess of an enclosed thermometer above θ, the velocity V of cooling of the thermometer is given by

$$V = F(\theta + t) - F(\theta).$$

There was to be expected, therefore, a dependence of the velocity of cooling upon θ and t, and this indeed proved to be the case, as the following table shows

t	$\theta = 0$ °C,	$\theta = 20$ °C,	$\theta = 40$ °C,	$\theta = 60$ °C,	$\theta = 80$ °C,
240	10.69	12.40	14.35	—	—
220	8.81	10.41	11.93	—	—
200	7.40	8.58	10.01	11.64	13.45
180	6.10	7.04	8.20	9.55	11.05
160	4.89	5.67	6.61	7.68	8.95
140	3.88	4.57	5.32	6.14	7.19
120	3.02	3.56	4.15	4.84	5.64
100	2.30	2.74	3.16	3.68	4.29
80	1.74	1.99	2.30	2.73	3.18
60	—	1.40	1.62	1.88	2.17

This table exhibits the property that, from the velocity of cooling corresponding to a definite t and θ, the velocity of cooling corresponding to the same t but to a θ higher by 20° can be derived by multiplication by 1.165. If θ increases in arithmetical progression, t remaining the same, V increases in geometrical progression. This property is represented by putting

$$f(u) = ma^{u.}$$

Thus

$$V = F(\theta + t) - F(\theta) = ma^{\theta}(a^{t} - 1),$$

where m and a are constant coefficients.

If we consider not only the fall of temperature but also the amount of heat lost by the cooling body, it is possible not only to compare the radiations, but also to determine them in absolute measure, as Hopkins[61] attempted to do.

27. Clausius[62] discovered a peculiar dependence of radiation of heat upon the medium in which the radiation takes place. This dependence results if we assume that two bodies of the same temperature, each being in a different medium which transmits rays of heat, do not change their temperature through mutual irradiation. Apart from the fact that this is in itself probable, since the disturbance of the equilibrium of temperature in such cases would certainly have been noticed, the assumption of the contrary would contradict a well-tested fundamental principle of thermodynamics.

In a simple case, the considerations which lead to Clausius's theorem are easily shown. Two hemispheres A and B which are perfectly reflecting on the inside and are filled with different media touch so that the line joining the centers is perpendicular to the section planes of the hemispheres. At the point of contact, there are small parts cut away so that the two media are contiguous to one another in a small plane surface-element S perpendicular to the above line. Near the center of A is a small portion f of a perfectly black body, from which rays which form at most a small angle α with the normal radiate nearly perpendicularly towards S and in a pencil whose aperture is of angle β arrive at the portion f', of a perfectly black body. Rays of other directions are thrown back upon for f', and again absorbed by them. Thus, only the mutual radiation of f and f' remains to be considered.

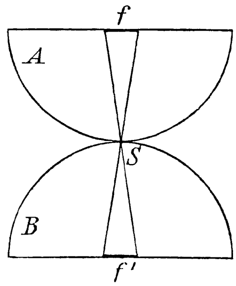


Fig. 56.

Since, for a small angle of incidence, the index of refraction can be represented by $n = \alpha/\beta$, the ratio of the areas of the surfaces is $f/f' = n^2$. If e denotes the quantity of heat radiated perpendicularly from the unit of surface into the medium of A, and e_1 has the same significance for the medium of B, then, taking into account the fact that, of the radiation falling upon S in one or the other sense, the fraction μ is let through but $(1 - \mu)$ is reflected back, for the maintenance of the equilibrium of radiation between f and f', the quantities of heat interchanged must be equal, that is to say,

$$ef\mu = e_1 f_1 \mu, \text{ or } ef/f' = en^2 = e_1, \text{ or } ev^2 = e_1 v_1^2,$$

where v and v_1 denote the velocities of propagation in the media of A and B respectively. In this consists the theorem of Clausius which G. von Quintus-Icilius has verified by direct experiment.[63]

28. Moreover, the concentration of rays by reflecting or refracting surfaces changes nothing in this behavior, as Clausius showed. We will limit ourselves here to proving that two surface elements f and f', of which the one is the optical image of the other, at equal temperature mutually radiate equally much heat. The surface element f of a perfectly black body in a medium A sends its rays upon its image f' in a medium B. The two media A and B are supposed to bound one another in a

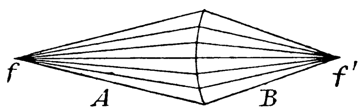


Fig. 57.

small circular portion of a spherical surface, which is pierced by the rays in almost a normal direction. The aperture of the bundle of rays is but small. If a and α are the distances of f and f' from the boundary surface, r is the radius of the spherical surface, m is the radius of the circle bounding the surface, and n is the index of refraction for A into B, then the apertures of the outgoing bundles from f and f' are as

$$(m/a)^2 : (m/\alpha)^2,$$

and the radiating surfaces as

$$(a+r)^2 : (\alpha - r)^2$$

Since an equal amount is lost in both directions by reflexion, and therefore the radiation which penetrates is diminished to the fractionary part μ, the equation

$$e\left(\frac{a+r}{a}\right)^2 \mu = e_1 \left(\frac{\alpha - r}{\alpha}\right)^2 \mu$$

holds for the equilibrium of radiation.

If in this equation the value of α is substituted from the well-known dioptric equation

$$\frac{1}{a} + \frac{n}{\alpha} = \frac{n-1}{r},$$

we get

$$en^2 = e_1, \text{ or } ev^2 = e_1 v_1^2,$$

where the above significance of the letters is retained. The theorem of Clausius is also in harmony with the results of the electro-magnetic theory of light.

CHAPTER IX

REVIEW OF THE DEVELOPMENT OF THE THEORY OF RADIATION OF HEAT

1. Researches concerning the radiation of heat began with the observation that there is an action at a distance with states of heat. Many investigators attributed such weight to the physiological distinction of quality of the sensation of heat and that of cold that they regarded heat and cold not as different degrees of homogeneous states but as opposite states of different kinds. Thus, in addition to rays which transfer heat were assumed rays which transfer cold.

2. Even one who does not yield to the physiological impression discovers at once a simple physical contrast in which, at least in many cases, it is entirely arbitrary which side he regards as positive and which as negative. It is indeed true, as Black remarked, that the sun impresses us as that from which all heat and, with it, all motion and all life proceeds, so that it seems natural to consider cold as the absence of heat. But if we imagine ourselves on a celestial body with a luminous atmosphere a dark body which traverses this atmosphere might be regarded as the surprising source of cold and of all variation connected with it.

3. In fact, in all events in which only the *differences* of temperature are the deciding factors, it is indifferent whether we say that heat is transferred from A to B or inversely that cold is transferred from B to A. But, with the more exact knowledge of facts, the conclusion emerges more and more clearly that the contrast between heat and cold is not a symmetrical one. Neither, indeed, does a complete symmetry, in which specific differences, like the Lichtenberg figures and so on, do not appear, correspond to the contrast of positive and negative electricity. Imagine two equal bodies A_1 and A_2 of the same temperature. Equalization of radiation takes place, according to Dulong and Petit, with greater velocity if the temperature of the one is increased by a number of degrees τ, than if it is reduced by the same number of degrees. However the subject may be viewed, there always results an asymmetry of the contrast of heat and cold.

4. This becomes still clearer by the proof, which gradually advanced towards completion, of the identity of light and radiant heat. Light is manifestly a process which *emanates* from the luminous body A. If an opaque body C is brought between the luminous body A and the illuminated one B, the latter is darkened. Another body K can still be illuminated between A and C, but not in the line AC on the side of C remote from A. The luminous process at places nearer to A is the condition for the luminous process at more distant places. Interference phenomena cause the spatial and temporal periodicity of the process to be recognized.

Any luminous process proceeding from A to B may be proved to be a heat-transferring process from A to B. An analogous cold-transferring process proceeding from A to B is not to be found. By this the asymmetry of the contrast between heat and cold is put beyond doubt.

5. A fact which naturally presents itself to the open-minded observer is the equilibrium of radiation of any system of bodies of the same temperature throughout. This equilibrium is disturbed by the changing of the temperature of any one body of the system. Upon the basis of some few observation with small differences of temperature, Newton laid down the hypothesis that the velocity of equalization is universally proportional to the difference of temperatures. But Dulong and Petit were the first to prove experimentally the dependence of this velocity on the temperatures of both of the bodies taking part in the equalization and to determine the mode of this dependence more accurately.

6. Before Prévost, of two bodies mutually reacting upon one another, the warmer was imagined to give up heat and the colder to receive heat. If the bodies exchange the parts that they play, the observer too must alter his view. Prévost put an end to this intellectual clumsiness when he succeeded in making the same general view do for all cases. The generalization of the idea is accomplished by seeking to retain, conformably to the principle of continuity,[1] the once conceived idea that the warmer body A gives up heat to the colder B, even when the temperature of the two bodies are equalized, and then beyond this point, up to the reversal of the differences of temperature; and by applying this view to other bodies as well. Prévost imagined the different processes of radiation to be simultaneous and independent of one another, just as Galileo[2] imagined several motions to be simul-

taneous and independent of one another. Prévost's idea also plays a part quite analogous to that of Galileo as a means of facilitating the survey and resolving complicated processes into simpler parts.

7. The perception of the equilibrium of the radiations of a system of bodies of equal temperature forces itself upon us unsought and instinctively, somewhat like the conviction of the equilibrium of Stevinus's chain.[3] Just as, from the chain, far-reaching conclusions can be drawn which reveal themselves as conditions of this equilibrium, the same thing may happen in regard to the equilibrium of temperatures. In both cases, the conclusions drawn have been verified by special observations before and afterwards.

Thus, an attempt was already made by Prévost to conceive the observed connection of more feeble radiation with more powerful reflexion in the same body as the condition of the equilibrium of temperatures. Fourier was perfectly clear about the fact that both (1) the proportionality between emission and absorption, and (2) the intensity of radiation being proportional to the sine of the angle of radiation, are such conditions for the equilibrium of temperature. Kirchhoff, added, as further conditions, the proportionality of the power of emission and the power of absorption for every particular wavelength and kind of polarization. Finally, Clausius recognized the dependence of the emissive power upon the velocity of propagation of the medium in which the radiation takes place as such a postulate of the equilibrium of temperature.

8. It is certainly surprising that such a multiplicity of conclusions can be drawn from the subsistence of equilibrium between temperatures, while the analogous case of Stevinus's chain yields only a single result. But the former fact, as we easily discern, is much more comprehensive. The intensity of the radiation of different bodies of the same temperature may be very different without the equilibrium being disturbed. The surface elements may have the most various orientation. Selective absorption is different for different bodies and different wavelengths. It is likewise different in regard to the kinds of polarization. It does not matter if the bodies taking part in the equilibrium of temperatures are immersed in different media. From each of these facts, discovered by a particular observation, together with the continuance of the equilibrium of temperatures, arises a particular inference, which appears as a

postulate of the supposed equilibrium and renders this equilibrium intelligible.

Perhaps in no other so small a domain may the adaptation of ideas to the facts which they represent[4], and the adaptation of ideas to one another, be so beautifully observed as in the one just considered[5].

CHAPTER X

HISTORICAL SURVEY OF THE DEVELOPMENT OF CALORIMETRY

1. Investigations concerning the reciprocal action of states of heat led quite gradually to a series of new conceptions whose employment brought the domain just mentioned into a clear order. We will here consider the development of these conceptions.

2. According to the view which Newton[1] had put forward as a hypothesis, the velocity of cooling of a body is proportional to the excess of its temperature above the surrounding medium, and, under circumstances otherwise the same, is proportional to the surface of the body. Later physicists, like Boerhaave,[2] were of the opinion that the velocity of cooling depends also upon the material and is diminished by the density of the body. Richmann's experiments[3] refuted this view and proved that mercury, under otherwise similar circumstances, cools more quickly and heats more quickly than lighter fluids. Moreover, balls of the same size of copper, brass, tin and lead cool, according to Richmann,[4] under circumstances otherwise the same, unequally quickly, but there is in this no appreciable influence of density or hardness. It first became evident later on that the undoubted influence of the material could only be correctly expressed by new conceptions. Different paths led to this: we shall examine one of these first.

3. Krafft[5] tried to represent the temperature U which results from mixing two masses of water m and m' of temperatures u and u' by the empirical formula

$$U = \frac{11mu + 8m'u'}{11m + 8m'} .$$

The asymmetry of the formula with respect to the two terms sufficiently shows that it can have only chance, and no general, validity. On the other hand, Richmann,[6] on the basis of theoretical considerations, produced a correct formula which gave the results of his mixture experiment. He supposed that the "heat" (*calor*) u of a mass m, by distribution upon the mass $m + m'$, yields the "heat" $mu/(m + m')$. If two masses m and m' with the heats u and u' are mixed, by uniform distribution the heat $(mu + m'u')/(m + m')$ is obtained. This formula may be easily extended to any number of mixed components, and we have then, for the temperature of the mixture,

$$U = \frac{mu + m'u' + m''u'' + \ldots}{m + m' + m'' + \ldots} = \frac{\Sigma mu}{\Sigma m}$$

It is worth noting that Richmann, in the article referred to, did not distinctly separate the two ideas which we to-day distinguish as "quantity of heat" and "temperature" but designated both by the same name "calor". He took account, in the *experiments* of the influence of the vessel and the thermometer, but in his consideration of them he treated both as though they could be replaced by an equal volume of water. From this it obviously follows that the view based solely upon mixture experiment with water was regarded as generally valid even for mixtures of heterogeneous bodies. At that time, people liked to imagine a distribution of heat according to volume.

On the other hand, Richmann was perfectly clear about the fact that in his calculations not the absolute heats but only the excess above the zero-point of his thermometer come into consideration. Richmann was led, as we see, by a material conception, though it was obscure. His experiments were an approach to showing the great significance of the product mu which we to-day call "quantity of heat."

4. Boerhaave[7] had a report concerning mixture-experiments. He stated that two equal volumes of water of different temperatures give, by quick mixing, the arithmetical mean of both temperatures. But, if the water is mixed with mercury in equal volumes, the temperature of the mixture is higher or lower than the arithmetical mean according as the water is the warmer or colder component. If we take two volumes of water and three volumes of mercury, the temperature of the mixture lies, according to Boerhaave, in the mean between the temperatures of the two mixed components. From these experiments, carried out by Fahrenheit for Boerhaave, Boerhaave concluded that twenty times the weight of mercury acts in the same way as one of water. Yet Boerhaave[8] considered a distribution of heat according to volume to be possible, in which opinion he was obviously strengthened by the fact that the most dissimilar bodies assume the same temperature when in contact with one another. Boerhaave was hindered by preconceived

ideas from finding the correct expression — to which he was so near — of the facts. From Fahrenheit's experiments, the capacity for heat of mercury would be 0.66 of that of an equal volume of water, while, according to more careful experiments, this fraction is 0.45.

5. Black brought clearness into the conception of these processes. After discussing the equalization of temperature between different contiguous bodies, he[9] called this the *equilibrium of heat.* The nature of this equilibrium was not, according to Black, understood rightly until he suggested a method for investigating it. Boerhaave was of the opinion that, wherever it took place an equal quantity of heat was found in an equal space, however different in kind the bodies may be which fill it, and Muschenbroek expressed his opinion in a similar manner: "For fire is distributed equally through all bodies that are of a fair size, in such a way that an equal amount of fire is contained in a cubic foot of gold, air, and feathers".[10] The grounds which they gave for this opinion are that, to whichever of these bodies they applied a thermometer, the thermometer always showed the same degree.

But this is, as Black remarked, to take a too cursory view of the subject: it is to confuse the *quantity of heat* in different bodies with their general *intensity* or *inner force,* although it is clear that these are two different things which should always be distinguished when we wish to speak of the distribution of heat. If, for example, we have one pound of water in one vessel and two in another, and both these masses are equally warm, as the thermometer indicates; then it is clear that the two pounds will have double the quantity of heat which is contained in the one pound.[11]

Before Black, it was supposed that the quantity of heat which is required to raise the heat of different bodies by the same number of degrees was in direct proportion to be quantity of matter, and that, therefore, if the bodies had an equal size the quantities of heat were in the ratio of their densities. However, soon afterwards — in the year 1760 — Black began to reflect upon this subject, and became aware that this opinion is an error.[12] He was led to this view by an experiment described by Boerhaave; — the Boerhaave-Fahrenheit experiment, which he said is made clearer by a numerical example. Assume that the water has 100° of heat, and an equal mass of mercury of 150° is instantaneously mixed with it and shaken. We know that the mean temperature between 100° and 150° is 125°, and that this mean temperature would be brought about if we mixed water at 100° with an equal mass of water of 150°, since the heat of the hot water is diminished by 25° while the cold water is warmed by exactly as much. If hot mercury had been taken instead of hot water, the temperature of the mixture would fall to only 120° instead of 125°. The mercury has, therefore, become 30° less hot and the water only 20° hotter: *and still the quantity of heat which the water has gained is exactly the same as that which the mercury has lost.* This shows that the same quantity of the substance of heat exhibits a greater power to heat mercury than an equal mass of water. Mercury has, therefore, less capacity for the substance of heat than water; a smaller quantity of heat is required in order to raise its temperature by the same number of degrees.

Black found the conclusion which Boerhaave drew from this experiment surprising. From the observation that heat is not distributed among different bodies in proportion to the quantity of matter in each, Boerhaave concluded that it is distributed in proportion to the space which each body occupies — a conclusion, which is refuted by this very experiment. Nevertheless, Musschenbroek still followed him in maintaining this. As soon as Black considered this experiment in the light just referred to, he found a singular agreement between it and some experiments carried out by "Dr. Martin".[13] He found, by repeated experiments, that mercury was heated much more quickly by fire than water — almost twice as quickly —, and that mercury always cooled much more quickly than water. Before these experiments were carried out, mercury was believed to require a longer time to heat or to cool than an equal mass of water, in the ratio of 13 or 14 to 1. These experiments of Martin's which agree so well with those of Fahrenheit, plainly show that mercury, notwithstanding its great density and weight, requires less heat to heat it than is necessary to raise an equal mass of equally cold water by the same number of degrees. We may therefore, said Black, fitly say that mercury has less capacity for heat.

6. Both Black's criticism of the works of his predecessors and his own achievements stamp him as one of the most eminent of natural scientists. This appears not only in the certainty and clearness with which he discriminated between and set up the ideas of the temperature (intensity of heat), quantity of heat, and capacity for heat, and with correct instinct for what was lacking in the survey of facts and necessary to it, but also in all of his general considerations upon the subject. In all

his work, he was at pains to dismiss arbitrary fancies, whether they originated from the heads of others or from his own head; to explain facts by facts; to adjust his own conceptual constructions to the facts; and to limit himself to the narrow and indispensable expression of what is actual. In this he was a worthy successor of Newton.

The assumption of a special substance "cold" he showed to be unnecessary. The sun is the ostensible source of all heat upon the earth and may, therefore, be regarded as the positive thing. He discussed without prejudice the kinetic theory and material theory of heat, and, through the difficulty of explaining many facts involved in the kinetic theory, was compelled to give the other the preference. Cold and heat were to him merely relative qualities, steps in the same series of states. Bodies like iron, water, and mercury — are not in themselves solid or liquid but the fluidity is determined by their state of heat. The freezing of cold bodies and the melting of hot ones was to him the same phenomenon. The chief advantage of the thermometer he considered to be the great extension of our observation of the series of degrees of heat. The assumption of absolute terminal points of this series he dismissed as groundless. The degrees of the thermometer appeared to him as numbered links in a chain whose ends are unknown to us.

The wind is not cold "in itself" but only on account of the rapid conduction of heat because of change of air. Ice melts in a current of air over 0° more quickly than in still air. Porous bodies and fur are not warm in themselves; they protect from cold *and* heat. Heat has not a tendency to go upwards, as we can prove under the receiver of an air pump by exclusion of currents of air. He discussed currents of air in mines and currents in deep seas. The air, simple because it is transparent, is not heated by the sun's rays nor in the focus of a concave mirror. Heating of air at such a focus only occurs when a non-transparent body is brought into the focus and heats the air in contact with it, as we then perceive by the ascending streams of air.[14] This observation was applied to the elucidation of cold in heights of the atmosphere. These are specimens of the quality of Black's mind, and we find them on every page of his book — a book which may be read with pleasure at the present day.

7. Black himself attempted to carry out determinations of the capacity for heat of some bodies. But most of the determinations of this kind date from W. Irvine[15] (1763?) who determined the capacities for heat

of some standard bodies like mercury, river-sand, glass, and iron filings, in order to ascertain the capacities of other bodies by mixing them with these bodies.

The Swede Johann Karl Wilcke[16] was also led to the conception of capacity for heat and showed that, for any body, a quantity of water which is equivalent with respect to the increasing of the temperature of the same quantity of heat can be given. Wilcke's[17] experiments began with the method of ice-melting, like those of Lavoisier and Laplace, of which process we will speak later. Mention must also be made of the treatise of Adair Crawford,[18] which contains determinations of capacities for heat.

8. In an independent and individual fashion, Lambert arrived at the conceptions above discussed. Lambert had a logical, deductive mind, and was skilled as a mathematician in quantitative distinctions. Such obscure interpretations of facts as are met with in the predecessors of Black were simply impossible to him. But Lambert was no born investigator of nature who, like Black, set out upon the discovery of new facts; he was above all a mathematician. He reconstructed the facts by setting out from certain suitable hypotheses. These hypotheses, it is true, contain gratuitous trimmings which Black would have discarded. A preponderance of spontaneous construction is characteristic of Lambert: it constitutes his merit where he is clear and fortunate, and his fault where he is prejudiced.

Lambert laid down his views concerning heat in two works of which the second was published twentyfour years after the first.[19] In the first he spoke of a repellent force of "fire particles", while, in the second, he ascribed a velocity to these fire particles — much as Daniel Bernoulli did to his gas particles — Lambert distinguished the "quantity of heat" from the "force" or "intensity" of heat. The quantity increases when, in the same matter, the degree of heat is kept the same, with the volume of the body; and, in the same body, with the degree of heat. But the same quantity of heat has, in different bodies of the same volume, an unequal force.[20]

The unequal velocity of cooling and heating of alcohol thermometers, on the one hand, and mercury thermometers, on the other, probably suggested to Lambert the idea that the same fire particles in mercury have a greater force than they do in an equal volume of water.[21] He interpreted the Boerhaave-Fahrenheit experiment by saying: "Hence it follows that, in water, *three* fire particles have not more force of heat than *two* fire particles in mercury, provided that the water and the mercury take up equal space".[22] From his own accurate experiments, where the temperature equalization between the fluid of the thermometer and another fluid in which it was immersed was observed, he concluded: "From these experiments it follows, in general, after compensation for the errors unavoidable in such experiments, that four fire particles in mercury, six in alcohol, and seven in water produce equal heat, provided that an equal volume of these substances is taken".[23]

9. The nomenclature of the conceptions just discussed varies with different writers and is sometimes not quite distinct in the same writer. In order to come to an agreement, we will introduce the names at present in use and define them as follows.

"Quantity of heat" is the product of the numerical measure of the mass of water (in kilograms) and the numerical measure of the change of temperature (expressed in degrees centigrade). The "kilogram-calorie" serves as unit, that is, the quantity of heat required to raise the temperature of 1 kg of water through 1 °C. The "specific heat" of a body is the quantity of heat which is required to raise the temperature of 1 kg of the body through 1 °C. "Relative heat" is the number of kilogram-calories required to raise the temperature of 1 l of the body in question by 1 °C. "Capacity for heat" of a body of any mass or volume, finally, is the quantity of heat (in kilogram-calories) which that body requires for the raising of its temperature through 1 °C. When a smaller unit is desirable, obviously the gram-calorie with the corresponding measure are to be employed. By these names every uncertainity is henceforth removed.

10. To the most noteworthy and enlightening of Black's works belong his investigations concerning the melting of ice.[24] Fluidity used to be generally regarded as a result following upon a *small* addition to the quantity of heat which has brought the body to almost the melting point; and the return of such a body to its solid state was supposed to depend on a very slight reduction in the quantity of its heat, when it is again cooled to the same point. This seemed to Black to be the general opinion when he began to give lectures at the University of Glasgow in the year 1757.

If we pay attention, remarked Black, to the manner in which ice and snow melt when they are exposed to the air of a warm room or when a thaw sets in after a frost, we can easily see that, however cold they were at first, they are *soon* heated to the melting point and soon begin to turn to water on their surfaces. If, now, the usual opinion were well founded, and if, for the complete changing of ice into water, merely a further addition of a very small quantity of heat were necessary, then the entire mass, even though of considerable dimensions, could be completely melted some few minutes or seconds later, since the heat is continually communicated from the surrounding air. Were this actually the case, the consequences arising from it would, under many circumstances, be terrible. For, even as things actually are, the melting of great quantities of snow and ice causes torrential streams and great floods in cold countries; or rivers are caused by it to overflow.

It is scarely possible to gain deeper insight than Black did here, by simple attention to unremarkable experiences which are accessible to everybody. To a glance so susceptible to the events in our daily surroundings was added, in Black's case, a clear-sighted analysis of particular experiments and skill in the effective employment of slender means of experiment.

11. A piece of ice in a considerably warmer space shows a rapid increase of temperature up to 0 °C. Then, however, a submerged thermometer remains stationary, and only begins to rise again when all the ice is converted into water. Figure 58 schematically indicates this; here the abscissae laid off towards the right represent the time and the ordinates represent the temperatures. If, now, some seconds before the moment *a*, the temperature of the ice is still one-hundredth of a degree below 0°, we would expect, according to Black, that in just as many seconds after *a*, the temperature would have risen above the zero point by one-hundredth of a degree, and that then the whole of the ice would

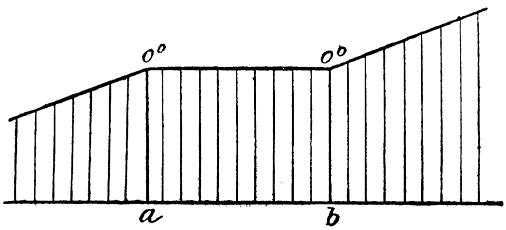


Fig. 58.

have suddenly melted. For all the circumstances with respect to the conduction of heat before and after the passing over of the zero-point remain almost unaltered. But melting ice, although it retains the temperature 0°, actually withdraws heat continuously from a hand which grasps it. A freely suspended piece of melting ice generates a cold descending current of air which we can perceive by the condensing water vapor. The slow melting shows that great quantities of heat are necessary for melting, and these quantities can be supplied only gradually from the warmer surroundings. Upon this fact the practicability of the ice house is based. Just so, freezing, concerning which analogous reflections may be made, does not take place suddenly, as the great quantities of heat given off by the freezing water can be removed only gradually by the surroundings. Freezing water in colder surroundings generates a warmer ascending air current which, according to Black, is perceptible on a thermometer placed above the water.

12. In order to obtain a measure of the quantity of heat expended in the melting of ice, Black proceeded as follows. We employ only the more familiar units. Imagine two equal flasks, the one filled with water at 0° and the other with ice at 0°, both provided with thermometers, and contained in a space of temperature 20 °C. If the water-flask were to assume a temperature of 4 °C in a quarter of an hour, then the contents

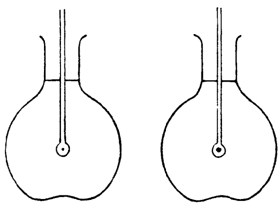


Fig. 59.

of the flask charged with an equal mass of ice, for which the circumstances with respect to the conduction of heat *remain* almost the same, would be completely melted in five hours. Therefore, for the melting of ice, a quantity of heat is supplied which would be capable of heating a mass of water equal to the mass of ice through 80 °C, but which produces no change of temperature. The experiment is not only wonderfully simply contrived, but also the number which results from Black's data (77—78) is remarkably accurate.

Black also put a weighed quantity of ice into a known quantity of warmer water in which the ice melted. From the cooling of the water, the quantity of heat consumed in the melting of the ice may be determined. The experiment may be imagined to be carried out according to the following plan.

Upon a scale (Fig. 60) are balanced 80 g of water at 20 °C together with an immersed thermometer, and the other scale pan received afterwards an overweight of 5 g. If, now, 5 g of snow at 0 °C are quickly put into the water, so that the index of the balance begins to move about, the

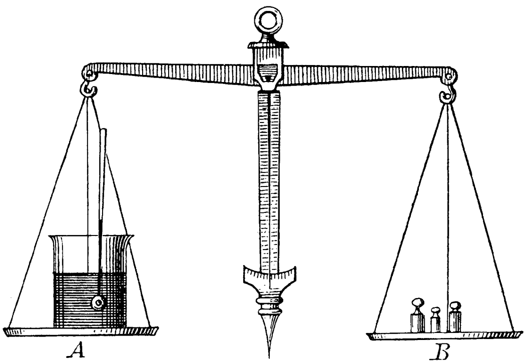


Fig. 60.

snow is melted. Since the 5 g of snow absorb 5 × 80 gram-calories, the 80 g of water are cooled on account of the giving up of 80 × 5 calories by 5 °C. The temperature sinks, then according to Richmann's law, to

$$\frac{80 \times 15°}{85} = \frac{16}{17} \times 15° = 14.1°,$$

since the 5 g of melted snow must be heated by the 80 g at 15 °C.

13. Black[25] attached special weight to Fahrenheit's experiment on cooling below zero, because in this the "latent" or hidden heat of the fluid appears suddenly and consequently very noticeably. Fahrenheit was able to cool boiled (free from air) water, standing undisturbed in a covered vessel, to about 4 centigrade degrees below the freezing point

without its turning to ice. But, by shaking, sudden and partial freezing set in, the water being interspersed with needles of ice, and an immersed thermometer immediately rose to 0 °. This was, for Black, the clearest proof that not the mere cooling below 0 °C but the releasing of a definite amount of heat is the condition of solidification.

14. Fahrenheit's experiment is very instructive and deserves a closer analysis. Black carried this analysis as far as was possible in his time. If the freezing of under-cooled water has begun, so much of it freezes that the heat liberated raises the temperature to 0 °C. Beyond this the temperature cannot rise, since with this rise the condition of further freezing would vanish. On the other hand, the liberated heat might be insufficient, under certain circumstances, for the raising of the temperature to the zero-point. Since, however, the latent heat of fusion suffices for the heating of the same liquid by 80 centigrade degrees, we see that the under-cooling must be very marked for this case to present itself.

For further analysis, we must take account of the following. Irvine and Crawford construed the consumption of heat in melting differently from Black. They assumed that the specific heat of the fluid is greater than that of the solid body, and that latent heat is simply the excess of the aggregate heat of the fluid at melting temperature above that of the solid at the same temperature reckoned from the absolute zero-point — the state of absolute lack of heat. This excess, according to the opinion referred to, must be supplied for the purpose of liquefaction. Upon this, these writers built a conception of the position of the point of absolute cold. They explained also the development of heat in chemical processes by such changes of the specific heats. From this, it is true, a different position of the absolute zero-point resulted from every example, and, in many cases, a meaningless result also followed. Black did not combat this view, but combated the assumption of an absolute point of cold and maintained that the supplying of latent heat is to be regarded above all as the cause of liquefaction.

15. If we take into account the fact that, according to more modern determinations, the specific heat of ice (between 0° and −20 °C) is very nearly half of that of the fluid, then water at 0 °C, to which 80 calories have been supplied by melting, contains in all, according to the Irvine-Crawford view, the total heat of 160 calories. If the water is deprived of

this, it contains no heat whatever. If the water could be cooled as *such,* then the absolute point of cold would be reached at −160 °C. At this point, the conversion of ice into water would claim no heat; this fluid heat would, however, become greater as the temperature above the absolute point of cold at which the conversion was begun was higher. If we reckon the temperature of this absolute zero-point upwards in centigrade degrees and denote it by τ, and the ordinary centigrade temperature by t, the heat of liquefaction is expressed by

$$\lambda = \tau/2 = (160 + t)/2,$$

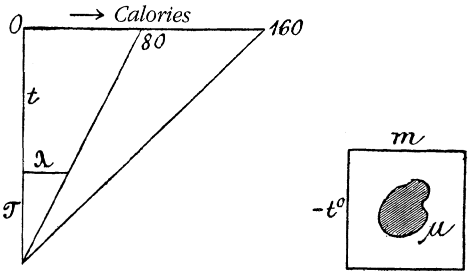


Fig. 60a.

which relation is represented by Figure 60a. Now, at $-t$ °C, let a part μ of the water mass m solidify into ice. The released heat is then $\mu(160 - t)/2$, which heats μ and $m - \mu$ by θ according to the equation

$$\mu(160 - t)/2 = \mu\theta/2 + (m - \mu)\theta,$$

from which we get

$$\theta = \frac{\mu(160 - t)}{2m - \mu}$$

or

$$\theta - t = \frac{160\mu - 2mt}{2m - \mu}$$

From what precedes, we necessarily have $\theta - t \leqq 0$, and, since obviously $\mu \leqq m$, we also have $2mt \leqq 160\mu$. From this are derived, by way of example, for the undercooling t, the highest possible values of μ and $\theta - t$:

t°	μ	$(\theta - t)^\circ$
−10	$m/8$	0
−80	m	0
−90	m	−20
−100	m	−40
−160	m	−160

By this the course of the undercooling experiment is cleared up. The calculation rests on the unproved assumption that the specific heat of water and ice below 0 °C remains constant. The proportionality between λ and τ is thus likewise based upon this assumption. It is, as we see, not necessary to conceive λ as the difference of the capacities for heat of water and ice from the absolute point of cold onward. We might just as well say that for −160 °C the value of λ is zero, and conjecture that, for lower temperatures, λ is negative. The fact must be completely separated from the associated theoretical idea, and the idea must never be regarded as decisive and infallible in a domain to which experiment has not yet penetrated.

16. Black was able to maintain the notion of the latent heat of fusion not only for water but also for all other bodies; indeed, he was able, without encountering any obstacles in the facts of observation, to speak of a latent heat of liquefaction in the formation of solutions, and by this view first to make intelligible the phenomena of freezing mixtures. According to this view, the mixed components of such a mixture, when they form a solution, take the necessary latent heat of liquefaction from their own supply of sensible heat.

17. The investigations on the melting of ice gave Black a convenient means for determining quantities of heat and especially specific heats. Wilcke had already employed, with small success, the method of melting ice for the determination of specific heat. This method was improved by Lavoisier and Laplace.[26] They used a tin vessel with double walls, delineated in Figure 61b and 61c. In the inner compartment the ice to be melted by the heated body was enclosed. The hollow wall and the lid were likewise filled with ice to order to prevent heat from penetrating from outside. If a body heated to t °C and of mass m, whose specific heat s is to be determined, was introduced, it melted a

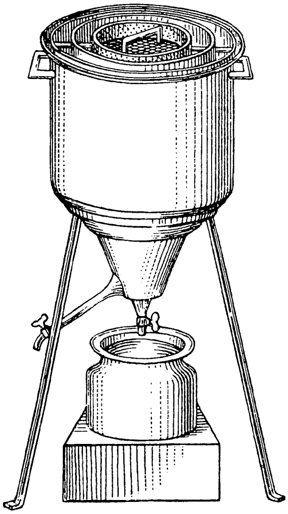


Fig. 61b.

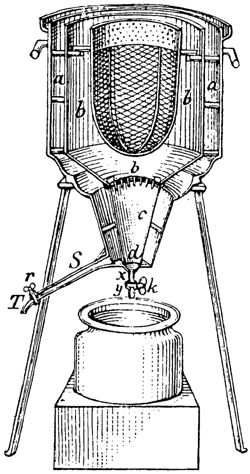


Fig. 61c.

mass μ as it cooled to 0 °C, where the equation $mst = 80\,\mu$ held. By weighing the water flowing away from the inner compartment, μ was determined. In the work mentioned, the authors also reviewed the kinetic theory of heat and explained the conservation of the quantity of heat by the conservation of *vis viva.* They were opposed to the assumption of an absolute zero-point, and declared the theory of the chemical development of the specific heats to be incorrect. In a second memoir,[27] determinations of specific heats were given. The method suffers from the drawback that it is difficult to obtain for weighing all of the melted water adhering to the pieces of ice. Bunsen[28] avoided this by using in his ice calorimeter the diminution of volume on the melting of ice for the determination of the quantity of ice melted. By this the method becomes very sensitive.

18. The method for the determination of specific heat which corresponds to the plan of the Boerhaave-Fahrenheit mixture experiment, and is usually called the method of mixtures, is really very simple in principle but requires great care when it is carried out. If m and m' are the masses, u and u' $(u > u')$ their initial temperatures, s and s' the specific heats, and U the temperature of equalization, then the method

is based upon the assumption that the loss of heat on one side is equal to the gain of heat on the other side, that is to say, that the equation

$$ms(u - U) = m's'(U - u')$$

holds.

If m' were the mass of the water of the calorimeter in which m is immersed, and consequently $s' = 1$, then we would have

$$s = \frac{m'(U - u')}{m(u - U)}$$

Only, in addition to m, the material of the vessel, the thermometer, and so on, must be heated. By computation or experiment the *water value* of these parts of the apparatus, that is, the quantity of water which has the same capacity of heat as they, is determined, and this water value is added to m' in the formula.

The method presupposes that an interchange of heat takes place *only* between the mixed bodies. But as a rule an interchange also takes place between the calorimeter and the surroundings. Rumford thought to eliminate this when he took the initial temperatures of the calorimeter approximately as far below the temperature of the surroundings as the final temperature, determined by a rough preliminary experiment, lay above the temperature of the surroundings. The method, however, is not sufficient, for the temperature of the calorimeter first rises rapidly after the entering of the hotter body and quickly passes the temperature of the surroundings, and then *slowly* approaches the equalization temperature.

19. In order to get a glimpse into the process in the calorimeter, let us imagine a body of temperature u_1 in calorimeter-water of temperature u_2, the temperature of the surroundings being τ. We assume that all parts of the body have the same temperature, and likewise all parts of the water. Then, if t denotes the time and a and b denote coefficients

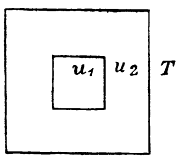


Fig. 62.

depending upon the masses, surfaces and relations of conduction, there subsists the differential equation

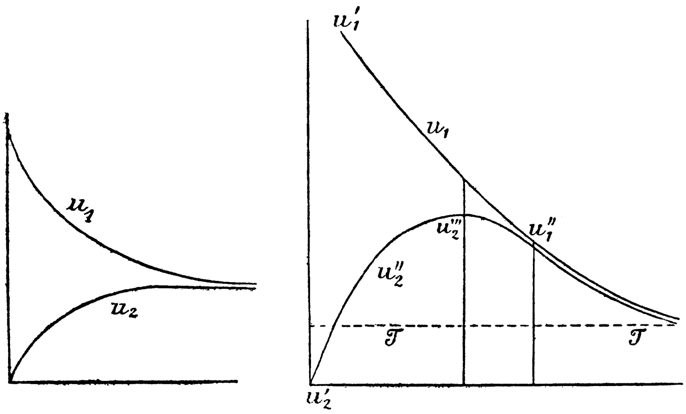
$$du_2/dt = a(u_1 - u_2) - b(u_2 - \tau).$$

For the maximum temperature of u_2 we have $du_2/dt = 0$, from which we have

$$u_1 = u_2 + b(u_2 - \tau)/a.$$

Provided $b = 0$, and only then, the maximum temperature is also the temperature of equalization, but otherwise $u_1 \gtrless u_2$ according as $u_2 \lessgtr \tau$.

If the times are laid off as abscissae and the temperatures as ordinates, Figure 63a represents schematically the course of the temperatures without disturbance of the surroundings and Figure 63b represents the same course in the case just discussed. We see that in the



a. b.

Fig. 63.

latter case u_2 (max.) is smaller than the corresponding u_1, and thus we may not regard the u_2 as temperature of equalization. But if the body in the calorimeter is allowed to cool from the initial temperature u_1' to u_1''' to which latter temperature the calorimeter-water is already uniformly cooled, then u_1'' is also very nearly the temperature of the calorimeter and $ms(u_1' - u_1'')$ is the quantity of heat given off by the body under examination. But this quantity of heat is no longer entirely in the calorimeter. In order to determine the loss of calorimeter up to the assumption of the temperature u_1'', Regnault proceeded wholly empirically, according to Pfaundler's account. The temperature of the calorimeter before the experiment and from minute to minute during the experiment was observed. The loss or gain of temperature corresponding to each temperature of the calorimeter and therefore also the loss or gain of heat during the time of the experiment, can be empirically determined. For this purpose, the temperatures of the calorimeter were laid off as abscissae and the gains and losses of temperature as ordinates, upward and downward respectively; through the end-points a straight line was drawn by means of which the loss for temperatures like u_2''', for example, which could not be observed directly, can be extrapolated. The algebraic sum of the ordinates of loss,

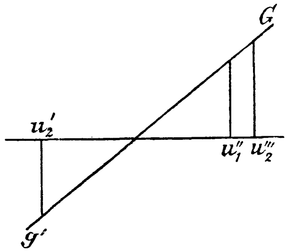


Fig. 64.

multiplied by the water-value of the calorimeter and added to the quantity of heat still present in the calorimeter, represents, at any time, the quantity of heat given up by the body under examination. In this way, the determination of specific heat is made possible.

20. The first more accurate determinations of specific heats were undertaken by Dulong and Petit, as already mentioned.[29] They established the fact that the specific heats depend upon the temperature — which Lavoisier and Laplace had already observed. According to the latter writers, the specific heat at a definite temperature, U is to be defined by the differential-quotient $\partial Q/\partial u$, where dQ is the elements of quantity of heat supplied to the unit of mass of the body for raising its temperature by du.

Dulong and Petit[30] found that the product of the atomic weights of the solid chemical elements with their respective specific heats yields a constant number (6.36), but to this, however, some elements (boron, carbon, silicon) form an exception.[31] Chemically equivalent quantities of these elements thus have equal capacities for heat. F. Neumann[32] entended the law to compound bodies of similar constitution.

The methods for determining specific heats were improved by Regnault. Pfaundler has described very convenient and simple methods, which, however, are outside the range of this exposition.

21. Of the greatest importance were Black's[33] investigations on evaporation. In the introductory remarks, Black mentioned the great increase of volume on evaporation, which, according to Watt's experiments with water, amounts to 1800 times that of the fluid. He illustrated this by the candle-bombs then in use — small glass balls containing a drop of water, which, when put into the wick of a candle, explode when the wick is lighted, extinguish the flame, and flatten the wick. He mentioned, further, the aeolipile for blowing on coal-fires, the danger of boiling linseed-oil containing drops of water, of using wet metal casting-moulds, of spitting into melted copper, and so on. He was aware of the difference of the boiling points of different liquids and cited Hooke as the first who had demonstrated the invariability of the boiling point of water.[34] The knowledge of the dependence of the boiling temperature upon pressure he traced back to Boyle's air pump experiments, Fahrenheit's observations of the barometer in determinations of boiling points, and Papin's experiments. The phenomenon now called by the name "Leidenfrost's drops" was known to Black, and he gave the explanation of it which is now accepted; he also mentioned that a piece of red-hot iron put into a tin vessel filled with water can melt a hole out of the bottom.

22. The invariability of the temperature of water during boiling, in spite of the continual supplying of heat, prompted Black to accurate investigation of the process, and he formed a view of it which was quite similar to the one on the process of the melting of ice. The opinions about boiling current at that time did not satisfy Black. Water, when the temperature of boiling is reached, was supposed to act as a sieve towards the heat. The heat penetrating through was supposed to cause the bubbling up, although, as Black remarked, it does not otherwise behave like a gas. The bubbling was sometimes looked upon as exit of the air from the water; though then the water, since it bubbles until the

last drop boils away, would have to be composed entirely of air. The most generally accepted view at that time seems to have been that the parts of the liquid in contact with the bottom receive a somewhat higher temperature from the flame, and, bubbling up as steam, rise and disappear, so that the liquid remaining behind and not in contact with the bottom retains only the boiling temperature.

From this explanation and the idea that had been conceived of the formation of steam, it was, writes Black,[35] regarded as certain that, after a body is heated to the point of vaporisation, nothing further is necessary than that a little more heat be added in order to convert it into vapor. But he could show that, just as in the case of liquids, a very great quantity of heat is necessary for the generation of vapor, although the body may be already heated to the temperature which it cannot overstep in the smallest degree without being changed in state. The necessary consequence of the formerly received view would be the explosion of the whole of the water with a vehemence like that of gunpowder. But he could show, said he, that this great quantity of heat goes into the steam gradually, although this steam is formed without becoming perceptibly hotter to the thermometer. If a kettle of water is put upon the fire, the heat passes into it very quickly from the beginning of the experiment until the water is heated to its boiling point. Possibly we find that in the last five minutes the heat of the water is increased by 20 degrees. It has been generally observed that the transference of heat from one body to another is nearly proportional to the difference of their temperatures, if other conditions remain exactly the same. From this, we may reasonably conclude in the present case that, since the water does not perceptibly change its temperature during the boiling, the heat continues to overflow in nearly the same proportion, and that four degrees of heat are received every minute by the water. This assumption leads to no sensible error; for Black frequently found that, if water in the last five minutes rises 20 degrees, it requires forty minutes to reach 162 degrees (Fahrenheit). If, now, the common opinion were the correct one, it is evident that in a few minutes more, the whole of the water would turn into steam and a violent explosion would take place, which would be enough alone to blow up the house.

Black could scarcely remember, he said, the time when he did not have a confused notion of this incompatibility of the facts with the current opinion; and suspected that it had, at some time, crossed the mind of almost everyone who has given attention to the boiling of a pot or a sauce-pan. But the importance of this surmise never affected him with full force until after he had made his experiments on the melting of ice. It seemed to him so difficult, if not impossible, to contrive an inflow of heat which would be in some degree uniform and to ascertain its irregularities that he had no disposition to make the experiment. Still, he once heard of a practical distiller who, if his furnace were in good order, could tell within a pint the quantity of liquid which he would obtain in an hour.

23. Black, then, immediately began his experiments. This was in 1762. He first satisfied himself that, as soon as water once boils, the quantity boiled is very nearly proportional to the time of boiling. A series of experiments was carried out according to the following plan, in which the temperatures are reckoned in centigrade degrees to make it easier to follow. Supposing that water at 10 °C. is brought to boiling upon a constant fire in a quarter of an hour and it is completely boiled away after 6 quarters more, it could be assumed that in this time, a quantity of heat is transferred to the water which would have heated it by $6 \times 90 = 540$ degrees. The exact determination of the heat of steam, for water, which Regnault performed, gives the number 536. Black obtained from his first experiments, it must be admitted, too small numbers (445,456). For, in these experiments, the determination of the instants of time of the beginning and the end of the boiling is somewhat arbitrary, as a repetition of the experiment readily shows. And we must not forget that as the experiment goes on the water presents a relatively greater surface for heating and consequently boils somewhat more rapidly than proportionally to the time. In fact, all of Black's numbers in these experiments turn out too small. Yet, considering the simplicity of the means, the results, as first approximations, are very remarkable. The experiment is greatly improved if we boil only a part of the liquid, then, after arbitrarity breaking off the experiment, determine this part by weighing, and make allowance for the time lost.

24. Another of his methods is no less simple and ingenious in arrangement. Water is heated in a closed vessel, and thus the temperature rises considerably above the boiling point. If, now, the vessel is opened and removed from the fire, the steam streams out for a long time and the temperature of the water falls rapidly to 100 °C. According to the view of his contemporaries, as Black remarked, the whole of the water —

heated above 100 °C as it was — would have suddenly to evaporate on the opening of the vessel. According to his view, the excess of the quantity of heat (above 100 °C) would be consumed in converting a small portion into steam. The experiment forms a beautiful counterpart to Fahrenheit's undercooling experiment. Unfortunately, a measurement of the heat of steam cannot be affected in this way, as a good deal of water accompanies the steam as it vehemently pours out, so that the mass of water vanishing must seem too great an l the heat of steam too small.

Watt, a friend of Black's, modified the experiment in the following manner. In an open Papin's digester an inch of water boils away in half an hour, for instance. If the digester is again replenished, the water is again brought to boiling point, and the digester is closed at the beginning of the boiling, and after half an hour again opened, then steam flows out for two minutes, during which time an inch of water disappears. The quantity of heat which the water, already heated to 100 °C, absorbs still further in half an hour is thus sufficient to convert an inch of water into steam, gradually during the absorption, or quickly afterwards.

The ideal of an experiment of this kind would be the following. If 540 g of water are heated in a closed Papin's digester to 105 °C, then, on the opening of the digester, if the water is prevented from being carried away, 5 g of steam would disappear, and the temperature of the water would sink to 100 °C.

25. The observations of others also accorded with Black's view. Boyle had already noticed that hot water, which is brought to boiling under the receiver of an air pump, cools considerably and very quickly. This observation was confirmed by Robinson, a pupil of Black's. Cullen, who carried out experiments on heating and freezing mixtures, had noticed that thermometers taken out of volatile liquids invariably showed a much lower temperature than that of the surroundings, and he had recognized the evaporation on the wetted thermometer bulb to be the cause of this phenomenon. He repeated the experiment with evaporating ether under the receiver of an air pump. In this, the ether became so cold by the rapid evaporation that water which was in contact with the ether vessel, froze. Here the heat of vaporization is taken from the liquid's own supply of sensible heat, as in freezing mixtures.

J. Black.

26. The question now to be investigated was whether the latent heat of steam can be again recovered when the steam is converted into liquid. If a liter of water was distilled and, after passing through the pipe of the cooler, was collected in the condenser, the 100 l of cooling water became warmer by 5.25 °C than they would have been by merely taking up the sensible heat of that distilled liter. Therefore, the heat of steam of a liter of water is able to heat it by 5.25 °C.

Black made several more experiments in connection with those already performed on the turning of water into steam; they completely convinced him that his opinion of the nature of elastic vapor was the correct one. In fact, as his mind was occupied with this thought, there flowed in to him from all sides the conviction that the quantity of heat in every vapor was immeasurably greater than that which was indicated merely by its sensible heat of temperature. Everyone knows the scalding power of steam. For a momentary blast of it from the spout of a teakettle, which will scarcely make the hand damp and does not contain a quarter of a drop, in one moment covers the whole hand with blisters which a thousand drops of boiling water could not produce. Scarcely anyone, said Black, will ever be found whom the great sensible heat in the cooler of an ordinary boiler does not surprise; and those who distil alcohol as an article of commerce have often had just as much difficulty and expense in supplying their cooler continually with a flow of cold water as in providing their furnace with fuel.

Watt found by measurements according to Black's principle, in which he also took account of the losses of heat, that the heat of steam lies between 495 and 525. Black had previously attempted to determine the heat of vapor from the method of ice-melting; these experiments failed however, and it was Lavoisier who carried them out and found for the vapor heat of water 550 or somewhat above it. Worthy of note is an experiment in which Black, by quick compression of steam, caused a marked increase of temperature. The self-evident transference of Black's theory of evaporation to all vapors need not be further mentioned here.

27. Of the more important of the general remarks in Black's work we will mention the following. He agrees with the idea of Amontons's that the air is only a body of higher degree of volatility, and, by a sufficient diminution of heat, it might become liquid and even solid.[36] Though, said Black, this opinion seems at the first glance, an extravagant flight

of the imagination, yet it is supported both by analogy and, in some respects, by immediate experience. We know that water is easily transformed into steam by heat, and steam, as long as it is kept sufficiently hot, has many of the properties of air. At another place, the method in use in India for the manufacture of ice by the aiding of evaporation was described and explained by the new theory of vapors.[37] The curious speculations made by Boerhaave in order to explain the cold of moonlight were annihilated by Black by the simple and natural observation that moonlight is only sunlight weakened in a very high degree.[38]

28. Black's general views on physical research are just as sane and forcible as his practical use of them in special investigations. The questionings and views of acute physicists concerning this combination of bodies with heat are, said he, very numerous and different from one another. But, since they are all hypothetical and the hypothesis is of a very complicated nature, — being in fact, a hypothetical application of another hypothesis, — he could not anticipate much profit from a more minute consideration of them. A skiful adaptation of certain conditions will make almost any hypothesis tally with the phenomena: this is gratifying to the imagination but does not enlarge our knowledge.[39] When we give an explanation of some extraordinary phenomenon or property of bodies, we always do it by showing that, in reality, it is neither so extraordinary nor connected so little with anything else already known, but that a connection exists between it and other things with which under more familiar circumstances we are very well acquainted, either on account of the resemblance which it has with them in certain particulars or on account of its origin from the same cause. But those who directed their attention only to chemistry were, for the most part, wholly unacquainted with the rest of the world. They could not, therefore, explain chemical facts by showing the resemblance between them and other better known things.[40] Through these passages there moves an unmistakeable trace of the Newtonian spirit.

29. Black's chief works were first published after his death by his pupil Robinson. Chemistry was by no means his particular profession; he was a professor of medicine and a physician with much a large practice. He attended his patients so strictly and assiduously that one would have thought that he would have had no time for his other profession.[41] This, moreover, seems partly to explain Black's attitude as an investi-

gator. Not without cause does it happen so very frequently that physicians and engineers contribute so abundantly to the furtherance of science as scientifically highly cultured men who are not estranged from life and are not hidebound in one narrow professional sphere. This circumstance explains, moreover, the circulation, without mention of Black's name, of discoveries made known through his lectures. These incidents must here be passed over, as this treatise has not a polemic aim.[42]

Black was one of those rare beings who, in everything that we know of him and in every page of his writings, wins our affection. The plain, straightforward, and unassuming simplicity with which he expounded his weighty ideas is attained only by few. In what he undertook, he succeeded, apparently without effort. We might say of him, in the phrase often used of poets: He is a thinker by the grace of God.

CHAPTER XI

CRITICISM OF CALORIMETRIC CONCEPTIONS

1. The unit of quantity of heat is usually defined as the quantity of heat which is necessary to heat 1 kg of water by 1 °C, or, if greater accuracy is required, from 0° to 1 °C. The quantity n units of heat, is, then, the quantity which contains the said unit n times. It is usually added by way of explanation that, in order to heat n kg of water from 0° to 1 °C, "obviously" n times the quantity of heat necessary to heat 1 kg through the same change of temperature is necessary, since, in the former case, "the same process" takes place n times. If we show that, by the cooling of 10 kg of water by 1 °C, 1 kg can be heated by 10 °C or 2 kg by 5 °C or 10 kg by 1 °C, then it follows, so far as this is exact, that the different single centigrade degrees are equivalent. It is then permissible to measure the quantity of heat "necessary" for the heating of m kg of water by u °C, by the product mu. It would be strange if anyone accustomed to more precise analysis of his conceptions who read this customary explanation as a student, or advanced it as a teacher, did not experience an intense logical discomfort.

2. If we look for the source of this discomfort, we find that, firstly, this definition assumes the defined concept as already known and given; and that, secondly, it tacitly regards a definite intuitive idea of the process of heating as self-evident and familiar. We have thus to remove the formal error mentioned, and, further, to inquire whence that intuitive idea comes and how it originated.

The first point might easily be disposed of by the following formulation: We say that the mass m (kg) of water *receives* in the raising of its temperature by u° (centigrade) the quantity mu of heat (in kilogram-calories), and that the same mass of water loses, in the reduction of its temperature by u °C the quantity mu of heat. This amounts to our *arbitrarily* giving to the product mu a definite name. If it can be shown, however, that a good scientific and practical use may be made of this designation, then the definition is justified by this. Such a procedure would be advantageously distinguished from the aforesaid method in that it does not attempt to conceal an arbitrariness. The second point would not, however, be disposed of by this procedure.

What causes us to call *mu* a *quantity*? I have a row of equal cylinders with vertical axes before me on the table. I turn one through 10° clockwise on its axis, and, after this, five others in the same manner. Here I have, "obviously", performed the same process six times. Should I call what the cylinders have here received a *quantity*? Should I say that the six cylinders have received six times the quantity of the first cylinder? The example may, for the present, make the necessity for an elucidation palpable. We shall find the same need in considering the progress of the mixture experiment repeatedly mentioned.

3. Experiments concerning the mixture of two equal masses of water of different temperature are very old. Besides the experiments of Renaldini already mentioned, Boyle, Wolf, Halley, Newton, Brook Taylor, Deluc, Crawford, and Black himself have, according to Black,[1] made some. The object of these experiments was to provide a basis for the graduation of the thermometer. The temperature of equalization was considered as the mean of the two temperatures of the components, that is to say, there was a *wish* to look upon the two steps from the temperature of equalization to the higher and lower temperatures respectively as equivalent. That this procedure could have no important practical results in the then state of experimental science and of the calorimetric conceptions, and could give only a very rough idea of the equivalence of the temperature degrees, is quite evident. According to the above experiments of Dulong and Petit, the scale thus obtained would be, furthermore, only an individual one and dependent upon the choice of the liquid used for the mixture.

4. The mixture experiments of Krafft, Richmann and Boerhaave, and Fahrenheit have quite another aim. The temperature scale is here considered as *given* and the temperature of the mixture is *sought.* Krafft's wholly uncritical procedure we will disregard. Richmann, undoubtedly under the impression of a not quite clear material conception, found the formula cited above[2] for the representation of the mixture experiment. The formula seemed obvious to the mathematicians, since it must have been familiar to them from numerous applications — for example, it serves for the determination of the price of mixed wares of different prices. But, while the application of the formula is self-evident and clear in many cases, as in the one just mentioned, Richmann's application is by no means self-evident. The

validity of the formula for this case is an important scientific find.[3] If I mix two masses of goods m and m' with the unit prices u and u', then the mixture has the unit price $(mu + m'u')/(m + m')$, since the fiftieth shilling is worth exactly as much as the tenth shilling which I receive. For the different degrees of temperature this is so little self-evident that it is strictly speaking not even true. In Figure 55, let the expansions of one thermometric substance be laid off as abscissae and those of another at the same states of heat as ordinates. In the sketch, the deviations from proportionality are taken considerable; in principle, however, it makes no difference if they are but small. If we mix two equal homogeneous messes with temperatures u and u', from the

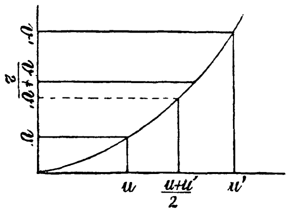


Fig. 65.

readings on the first thermometric substance, and if we assume the temperature of the mixture to be exactly $(u + u')/2$, then, as the dotted line shows, by reading on the other thermometric substance, the mean $(v + v')/2$ of these readings is not forthcoming. From this consideration alone, then, we must admit that Richmann's formula is certainly only an empirically approximate expression of the facts.

5. How, now, does the material conception come into the idea in question? We will reserve the full discussion of this question for a later place and limit ourselves here to the following observations. We must observe, even without purposely following up the facts, that one body is heated at the expense of another. One body is heated only by another's becoming cooled. A thermal property is transferred from one body to another just as a liquid can be poured partially from one vessel into another. This resemblance, which is forced upon us quite involuntarily, is the basis of the instinctively developing material conception. Add to this that the "matter of heat", or the fire, appears to become visible in the glowing or burning of a body, since we are usually accustomed to

perceive only what is material. If the material conception becomes clearer and more vivid, we at once think of an invariable *quantity* of substance and look for it in the process of the communication of heat. If we can find anything in the process which remains *constant*, — and we can do so comparatively easily by means of the material-conception, as we saw in the case of Richmann and still better in that of Black, — then this thing represents to us the substance or quantity. That the idea of a liquid is the moving force with Richmann, is disclosed especially in the otherwise unmotivated acceptance of the distribution of heat according to volume, with which we repeatedly meet in him and others.

If m and m' are the masses of water and u and u' the temperatures, and U the temperature of equalization, Richmann's law may be expressed in the form

$$mu + m'u' = (m + m')\,U,$$

which directly shows that the sum of the products of the masses and temperatures (above an arbitrarty zero-point, as Richmann and Black knew) remains constant on equalization. Thus, this product-sum represents the invariable *quantity of heat.* Richmann's equation in the form

$$m(u - U) = m'(U - u'),$$

teaches that the product of the numerical measure of the one mass of water by its loss of temperature is equal to the product of the numerical measure of the other mass by its gain of temperature. If we call, more briefly, the changes of temperature of the two masses of water m and m' entering into reciprocal action by the names θ and θ', we have the equation

$$m\theta + m'\theta' = 0,$$

where the sum is to be taken algebraically.

6. Since, accordingly, the products $m\theta$ (mass of water multiplied by change of temperature) have a decisive significance in the judgment on the heat processes; and since, by paying attention to them, the comprehension of the processes is made much easier, it is justifiable to give them a special name. There is no objection to calling these products "quantities of heat", and this use in no wise commits us to hold fast to a material conception, although this conception has played an important part in the introduction of the usage.

To take the material conception seriously is no longer permissible, on account of the many exceptional cases in which it presents no adequate expression of the facts. But where it is convenient, as in simple cases of communication of heat, it can be used as a means of illustration, and there it keeps its value for all the future. After all, only experience could show that all bodies of different temperatures enter into an equalization of temperatures, and so much the more can experience alone ascertain in what quantitative manner this equalization takes place. Thus, experience has acquainted us with the significance of the products $m\theta$.

7. On mixing equal volumes of water and mercury, it appears that the temperature of equalization remains far under or over the arithmetical mean of the temperatures of the components according as the mercury is the warmer or colder component. This is not favorable to the formation of a material conception. It appears as though in the first case heat were lost, and as though in the second it were gained; and this has occasionally been supposed to be the case. If we disregard the older mixture experiments with equal volumes and consider the mixture of different masses of non-homogeneous bodies — for example, the water mass m with the mercury-mass m', — it turns out that the equation

$$m\theta + m'\theta' = 0$$

is not satisfied. Instead of giving up the material conception and with it the importance of $m\theta$ assumed by it, we can proceed as Black did. He held to the material conception, which had become dear and familiar, and so modified it that it was adapted to the new case. In fact, the new case can be represented by the equation

$$m\theta + s'm'\theta' = 0,$$

where s' is a constant coefficient which stands for the specific heat (of the mercury). The coefficient s' is chosen so that the product $m\theta$ is compensated by $s'm'\theta'$. The still more general equation

$$sm\theta + s'm'\theta' + s''m''\,\theta'' + \ldots, = \Sigma sm\theta, = 0$$

represents a still greater number of processes which contain all those previously considered as special cases. It was reached by adhering to the existing ideas and conceptions conformably to the principle of continuity and by adapting them, for purposes of economy of thought, to a great multitude of cases.

8. Here also, although the material conception promoted this development, the development could have taken place without the help of any hypothetical assumption, in some such way, for instance, as the following. Two homogeneous bodies of different masses m and m' impart to one another, as experiment shows, temperature changes θ and θ' which are inversely in the ratio of the masses. We have, paying attention to the sign,

$$m'/m = -\theta/\theta'.$$

With two heterogeneous bodies, the ratio of the masses no longer by itself determines the ratio of the changes of temperature. However, there is no objection to defining quite arbitrarily: Bodies of "equal capacity for heat" we term those which impart to one another equal and opposite changes of temperature. Or, more generally: The ratio of the thermal capacities κ and κ' of two bodies is the negative inverse ratio of the respective temperature changes θ and θ', that is to say,

$$\kappa'/\kappa = -\theta/\theta'.$$

"Relative heat" would then be defined as the thermal capacity of the unit of volume and "specific heat" as the thermal capacity of unit mass of a body. In this, some arbitrarily chosen body, such as water, would be taken as basis for comparison, or standard. But this proceeding, which is the strictly scientific one, as it is confined to the conceptual expression of facts, makes a further explanation necessary. As long as I imagine thermal capacity measured by the quantity of material of heat which a body receives for the increase in temperature of one degree centigrade, it is self-evident that two bodies which have the same thermal capacity as a third are of the same thermal capacity as one another. But if the ratio of the thermal capacities is defined by the negative inverse ratio of the reciprocal changes of temperature, then the above proposition is no longer self-evident, since the equality of capacity of A and B and that of C and B are each based upon an experience which does not logically necessitate the third experience upon the basis of which the equality of the capacities of A and C could be affirmed. The latter is an independent and new experience. Still, its occurrence is to be expected on physical grounds, since the contrary would be most decidedly contradictory to the picture of the thermal processes which has been produced from daily experience without our conscious aid.[4]

9. In order to make this contradiction clear, let us assume that the bodies A and B behave to one another as if they had equal capacities for heat, and similarly B and C; whereas A and C have capacities in the ratio κ: 1, where, by way of example, we take κ greater than 1. Initially let A have had the temperature u, but B and C the temperature 0. Equalization between A and B gives to each the temperature $u/2$. Following upon this, equalization between B and C gives to each of the two the temperature $u/4$. The temperatures in the three successive stages of this mental experiment are represented in the following table:

	A	B	C
1	u	0	0
2	$u/2$	$u/2$	0
3	$u/2$	$u/4$	$u/4$

If, now starting from the same initial state, we effect equalization between A and C by contact, and then between C and B, there results the table:

	A	C	B
1	u	0	0
2	$\kappa u/(\kappa+1)$	$\kappa u/(\kappa+1)$	0
3	$\kappa u/(\kappa+1)$	$\kappa u/2(\kappa+1)$	$\kappa u/2(\kappa+1)$

But $\kappa/(\kappa + 1) > 1/2$ and $\kappa/2(\kappa + 1) > 1/4$. Consequently, starting from the same initial state and proceeding according to the second table, we could arrive at higher temperatures of the bodies A, B, C. We could withdraw from these bodies the excesses above the final temperatures of the first process by means of other bodies M, N and O, which would thus be heated without any expenditure, while daily experience teaches that one body is heated only at the expense of another. The assumption from which we started is, thus, irreconcilable with physical experience.

10. Generalized for indefinitely many bodies, the physical requirement just described may be expressed as follows. Let there be given a number of bodies

$$K\ L\ M\ N, \ldots, R,\ S,\ T;$$
$$\kappa,\ \lambda,\ \mu,\ \nu,\ \ldots,\ \rho,\ \sigma,\ \tau.$$

By the large Roman letters the bodies as well as their capacities are designated. Coordinated with these as indices on account of greater convenience of writing and of discrimination, are the small Greek letters. If L enters into reciprocal thermal action with K, we have the equation

$$K\theta_\lambda^\kappa + L\theta_\kappa^\lambda = 0,$$

which is immediately intelligible from what precedes, for the capacities and reciprocal changes of temperature. The capacity L is determined from K in the form

$$L = -K\theta_\lambda^\kappa / \theta_\kappa^\lambda$$

If M is determined in the same way by L, N by $M, \ldots, S$ by R, T by S, and finally K by T, then, for the last determination, if the above physical postulate is not fulfilled, a value K' different from K would result. For a series of m bodies, we would have

$$K' = (-1)^m \cdot K \frac{\theta_\lambda^\kappa \cdot \theta_\mu^\lambda \ldots \theta_\sigma^\rho \theta_\tau^\sigma \theta_\kappa^\tau}{\theta_\kappa^\lambda \cdot \theta_\lambda^\mu \ldots \theta_\rho^\sigma \theta_\sigma^\tau \theta_\tau^\kappa}.$$

The postulate is therefore expressed by the equation

$$\frac{\theta_\lambda^\kappa\, \theta_\mu^\lambda \ldots \theta_\sigma^\rho\, \theta_\tau^\sigma\, \theta_\kappa^\tau}{\theta_\kappa^\lambda\, \theta_\lambda^\mu \ldots \theta_\rho^\sigma\, \theta_\sigma^\tau\, \theta_\tau^\kappa} = (-1)^m.$$

If the θ's are conceived as reciprocal accelerations, this equation expresses a quite analogous postulate with respect to masses.[5]

11. The founders of the conception "specific heat" made the assumption, which was in keeping with the then state of observation, that the specific heat is constant and independent of the temperature. The experiments of Lavoisier and Laplace and still more those of Dulong and Petit showed the untenability of this assumption. By the cooling of a unit mass of water from 51° to 50 °C, the unit of mass of another colder body can be heated from u to $u + \theta$ degrees, and conversely the cooling of this body from $u + \theta$ to u, if it is warmer than the water, raises the temperature of unit mass of water from 50 to 51 degrees centigrade. The reciprocal value of θ measures the specific heat of the body under examination. Since θ depends upon u, the specific heat is also a function of the temperature. But, even if the unit mass of water may replace the body under examination, it appears that θ is not exactly equal to unity but varies somewhat with u. Therefore the specific heat of water is not for all temperatures exactly unity but changes with the temperature. The unit of quantity of heat must accordingly be more exactly defined by stating that it is given by the raising of the temperature of a kilogram of water from $u°$ to $u + 1°$ centigrade, where u is chosen by many physicists to be zero, and by others to be 15. The specific heat of water can then likewise be put equal to unity only for a definite temperature u. The difficulties arising from this can easily be overcome. The quantity of heat which corresponds to any change of temperature of any body can always be determined sufficiently accurately by ascertaining the very small compensating change of temperature of a correspondingly large mass of water at the normal temperature u.

12. The ratio of the specific heats s and s' of two bodies was defined, from the old standpoint, by the reciprocal temperature changes θ and θ' of the unit masses of the bodies compared, that is to say,

$$s'/s = -\theta/\theta',$$

where the initial temperatures u and u' of these bodies were indifferent. From the new standpoint we must, however, put

$$s'/s = -d\theta/d\theta',$$

where $s = f(u)$ and $s' = F(u')$.

If, in general, f were equal to F, a scale of temperatures could be found for which the specific heats would be constant. This is not possible, however, as Dulong and Petit were aware, since $f, F, \dots,$

are individual for every body. On the contrary, we would, starting with Renaldini, from two determined normal temperatures, arrive, by mixture experiments with different bodies, at temperatures which, indeed, according to this principle, are to be designated by the same numbers but which would not agree with the definition of equality of temperature laid down above.[6] But this inconvenience would be greater by far than the dependence of the specific heats upon the temperature.

Thus, although the original conceptions in their simplicity were no longer adequate, still it was found more advantageous to modify them suitably instead of to put entirely new ones in their place, for one thing because, for an approximate representation, the older and simple conceptions suffice.

13. The product $\kappa\theta$ of the theory of heat is analogous to the product mv of mechanics. The reciprocal changes of temperature are, like the reciprocal changes of velocity, of opposite sign. Neither negative masses nor negative capacities for heat have been found. However, masses have shown themselves to be independent of velocities,[7] while thermal capacities depend upon temperatures. Propositions may be constructed for $\kappa\theta$ in one dimension analogous to those for mv in three dimensions.

14. From our exposition it appears sufficiently clearly that, even without preconceived opinion and without any hypothetical or symbolical auxiliary conceptions and merely by the endeavor to express conceptually the facts of the communication of heat, approximately the same final results which we have attained in reality must have been reached though perhaps under other names. The temperatures of bodies mutually determine velocities of change of temperature which are influenced quantitatively by the positions, nature of the surfaces, masses, and material properties. The final temperatures of the system of bodies are more simply determined by the initial temperatures, masses, and those individual constants which are known as specific heats. This latter determination forms the proper subject-matter of calorimetry.

The essential character of Black's idea consists in that a positive product $m's'\theta'$ is regarded as compensation for an equally large negative product $ms\theta$, and that, therefore, such equal products of the same sign are regarded as equivalent. In this, the positive or negative products are regarded as measures of a material quantity of heat; which is not necessary though it helps imagination so to regard them. This view is, moreover, supported by a particular circumstance. If $-ms\theta$ appears in the warmer body A and $+m's'\theta'$ in the colder body B, this process cannot be reversed straightway, but, on the other hand, by the change $-m's'\theta'$ in B, there can be produced in the still colder body C the change $m''s''\theta''$ which could have been produced immediately by $-ms\theta$ in A. In this there are heatings and coolings, processes alike in kind and opposite in sense which mutually condition one another and are regarded as compensating; and heatings and heatings, processes alike in kind and sense, which are regarded as equivalent.

15. If the ideas of compensation and equivalence have once become familiar, they are only reluctantly again given up. If a quantity of heat disappears somewhere without an equivalent quantity of heat appearing elsewhere, the question arises: Where has the vanished quantity of heat gone, or by what is the cooling process compensated? In accordance with this manner of thinking, Black inquired into the compensation of the actually proved cooling of the source of heat on melting and boiling without a corresponding increase of temperature in the melting or boiling body. He found that a quantity of heat can be equivalent not only to a quantity of heat but also to the fusion or evaporation of a definite mass. Thus the equation of compensation may also appear in the form

$$sm\theta + \lambda m' = 0,$$

where λ denotes the latent heat of fusion or evaporation of the unit of mass. In this quantitatively exact and conceptual expression of the facts lies Black's great achievement. The idea that latent heat is still heat is really superfluous here and goes beyond the necessary expression of the actual facts. The constancy of the quantity of heat was just an idea grown precious and which, if it had been taken only figuratively and not seriously, would have been no hindrance to research later on. Actually, it did turn out to be a hindrance.

But with the thought that coolings can be compensated not necessarily by heatings but also by physical processes of quite another kind, Black approached by a great step to the manner of thinking that to-day characterizes thermodynamics — a branch of science which recognizes a relationship of thermal processes with physical processes of any kind.

CHAPTER XII

THE CALORIMETRIC PROPERTIES OF GASES

1. The methods for determining specific heats cannot be applied to gases without some difficulties. Crawford[1] attempted to determine the specific heats of gases by immersing large heated tin cylinders filled with gases in a calorimeter, but he could obtain only inaccurate results, on account of the small gaseous masses entering into the operation. Lavoisier and Laplace[2] let large heated gaseous masses, m, pass through the spiral tube of an ice calorimeter, determined the cooling, θ, of the gas resulting form this and the quantity, μ, of ice melted, from which the equation

$$ms\theta = 80\mu$$

for the determination of the specific heat s was obtained. Clément and Desormes[3] determined the specific heat of air by this method. If the same globular receptacle were filled with different gases of the same temperature and the same pressure and brought into a water calorimeter of known higher temperature, the capacities of these gaseous masses for heat could be put proportional to the times required for heating by the same number of degrees of temperature.

2. The first more exact determinations of the specific heats of gases are due to Delaroche and Bérard.[4] The principle of their method is as follows. A large gaseous mass m — in reality a small quantity is used over and over again — with temperature u_1 under constant pressure is led per minute through the spiral tube of a water calorimeter. By this means it is cooled to the temperature u_2, while the calorimeter finally, on the continuance of the operation, assumes the permanent temperature excess u above the surroundings. In this stationary state, therefore, the calorimeter loses as much heat to the surroundings as it obtained from the gas in the same time. If the calorimeter of water value w is observed without the gas being supplied, it is found to lose, v degrees per minute of temperature. Therefore we have the equation

$$ms(u_1 - u_2) = wv,$$

from which the specific heat *s* may be determined. The comparison of the quantities of different gases which impart to the calorimeter the same increase of temperature is an additional help.

Haycraft[5] sought to avoid small uncertainties in the determining of temperature and the influence of humidity of the gases. He thought that his experiments warranted the conclusion that equal volumes of the most different gases under the same pressure have the same capacity for heat; whereas Delaroche and Bérard, in this case, had found different numbers. Haycraft's result seemed to be substantiated by the experiments of Delarive and Marcet[6] who deduced the capacity for heat from the time of heating of the gas in a globular receptacle which served at the same time as an air thermometer.

3. Regnault[7] performed the most exact experiments essentially after the method of Delaroche and Bérard, with whose numbers his agree very well. He found that only those gases which approach most nearly to the ideal gaseous state (O, H, N) exhibit the same capacity for heat at the same volume and pressure. The specific heats of gases, equal weights being considered, is, according to Regnault, independent of the pressure. It is, for example, the same for air between 760 mm and 5674 mm of mercury pressure. Small differences are probably to be found only in easily compressible gases. Delaroche and Bérard still believed, from an experiment, that increase of specific heat must be inferred from decreasing pressure. Likewise, the specific heat of the permanent gases is independent of the temperature. This was proved for air between −30 °C and +200 °C. Carbonic acid gas, which shows considerable deviation from Mariotte's law, exhibits an appreciable increase of specific heat with rising temperature. After the experiments on the specific heats of solid and liquid bodies, a dependence of specific heat on temperature for gases as well could not be regarded as quite out of the question.

The following table serves for the comparison of the specific heats of gases.

The agreement between Delaroche and Bérard, on the one hand, and Regnault, on the other, is evident from the table; likewise the deviation of carbonic acid gas from the above-mentioned Haycraft's law, and the strikingly great specific heat of hydrogen (with reference to the weight) amounting to more than three times the specific heat of water which latter otherwise exceeds considerably those of all other bodies.

	Delaroche and Bérard			Regnault	
	equal volumes; air = 1.	equal weights; air = 1.	equal weights; water = 1.	equal weights; water = 1.	equal volumes; water = 1.
Air	1.0000	1.0000	0.2669	0.23751	0.23751
O	0.9765	0.8848	0.2361	0.21751	0.24049
H	0.9033	*12.3400*	*3.2936*	*3.40900*	0.23590
N	1.0000	1.0318	0.2754	0.24348	0.23651
CO_2	1.2583	0.8280	0.2210	0.21627	*0.33068*

It is important for what follows to consider how late a reliable judgment about the behavior of the specific heats of gases was arrived at.

4. Gradually there became known numerous observations which showed that changing the volume of a gas causes a change in its temperature. Erasmus Darwin[8] noticed the cooling of the blast of air discharged from an air gun and explained on the basis of this observation the cold of high mountains. Similar observations are due to Pictet[9] and systematic experiments to Dalton.

Dalton noticed the fall of a thermometer under the receiver of an air pump when the air was withdrawn, and its rise on the air being admitted. The employment of *open* thermometers proved to him that the movement is not due to changes of capacity of the thermometer bulb through pressure. The changes, small in amount (two to four degrees) but rapid, showed that the temperature alterations of the air are much greater than the indications, but only of short duration; and this was confirmed by the more abundant indications of thermometers with small bulbs. As Dalton's thermometers, at 50° excess of temperature above the surroundings, showed an equally rapid movement (1° in $3\frac{1}{2}$ seconds), he inferred changes of temperature up to 50° (Fahrenheit) in his experiments. He ascribed to denser air a smaller capacity for heat, and to a vacuum a greater capacity for heat than to an air space of equal size, and expected to ascertain, by experiments of the kind described, the capacity for heat of a vacuum.[10] Clément and Desormes took up these ideas.

In the year 1803, the pneumatic tinder-box invented by a workman in the weapon factory at Étienne en Forez, became known.[11] Somewhat later Gay-Lussac[12] made experiments which became very important in the further development of the theory of heat. Two equal globular receptacles *A* and *B*, dried with calcium chloride, and each of 12 liters capacity were connected by a tube with a cock. The one *A* is filled with gas and the other *B* is pumped empty. If the cock is opened, the gas expands to the double space. Since, from the cooling of gases by expan-

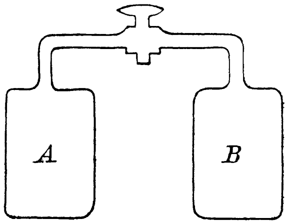


Fig. 66.

sion and the heating of them by compression, the increasing of the specific heat by rarefaction had been inferred, Gay-Lussac expected a cooling from the overflowing of *A* into *B* and hoped, from the extent of this in different and similarly treated gases, to be able to derive their specific heats. He observed, however, in company with Laplace and Berthollet, a rising of the indication of the thermometer in *B*. The mercury in a thermometer introduced into *A* fell, however, with the overflow of the gas into *B*. The temperature changes were, to the manifest astonishment of the observers, equal and opposite on the two sides, as the following table shows:

Air-pressure in *A*	Temperature-decrease in *A*	Temperature-increase in *B*
0.79 m	0.61°	0.58°
0.38 m	0.34°	0.34°
0.19 m	0.20°	0.20°

Thus, on the whole, no change of temperature appears. Therefore the specific heat is not changed by increasing the volume, but this fact, from the standpoint of that time, was difficult to reconcile with the current views. Gay-Lussac rejected the supposition that the temperature increase in *B* is to be ascribed to the compression of the residual gas in

B. If an increase of specific heat on rarefaction of the gas is assumed, then empty space must contain the most "heat substance", as was sometimes believed. Thus it might be imagined that heat is set free by the diminishing of empty space. On observing that volume changes of the Torricellian vacuum do not affect a thermometer enclosed in it, Gay-Lussac discarded this assumption as untenable. Under these circumstances, it was natural that Gay-Lussac observed the utmost caution in his conclusions. Likewise, the views of Dalton on the capacity for heat of gases, which were mentioned above[13] and which seem very strange to us, become in some measure comprehensible when we consider the historical circumstances.

5. The facts ascertained by Gay-Lussac, thus, were not in harmony with the current conception of known phenomena. That this conflict did not lead to a deeper investigation may be owing to the fact that Gay-Lussac's work was little known. A glance at Biot's text-book convinces us how little inclined people were to draw conclusions which are to-day recognized as correct.[14] There, as late as 1829, after an account of the experiments of Delaroche and Bérard, we read the following:[15]

The heating which the gases generate in the apparatus is therefore the combined effect of the heat which is liberated by the cooling and by the simultaneous contraction. In order to obtain simple results, we should have to be able to observe these operations separately. First of all the quantity of heat which any gas releases by cooling in a given space, and therefore under constant volume, would have to be determined, and afterwards the heat which it yields by change of its volume with constant external temperature. The separation of these two phenomena seems exceedingly difficult, but it is essential if we are to arrive at simple results and to bring to light the true laws upon which these effects depend. Certainly we are also subject to a disadvantage of the same kind in experiments on the specific heats of liquid and solid bodies, inasmuch as they likewise necessarily contract on cooling. But, since their change of dimensions is far less, we assume that the liberation of heat conditioned by this change is also very feeble in comparison with that due to reduction of temperature. However, there is nothing to prove that such is actually the case; and we might rather conjecture the contrary if we consider what enormous quantities of heat can be set free from bodies by mere separation of their parts from one another by means of rubbing, turning, boring, and filing — which is simply a friction of sufficient strength to tear the particles on the surface away from those lying underneath. For when Rumford[16] examined, from this point of view, the filaments issuing from the barrel of a bronze cannon when it is bored, they proved to have the very same specific heat as the bronze itself, although an enormous heat had been liberated during their production. From this it is to be inferred that this heat was present merely between the solid bronze particles, that is to say, between the small groups of these particles which the instrument had separated. If this is so, then this quantity of heat must likewise be altered by every expansion or contraction of the body; and this result, which is added to the absorption of heat dependent upon mere changes of temperature, need be by no means so weak as is commonly supposed.

6. People persisted, at this time, in imagining an absorption of heat connected with every geometrical increase of volume of the gas and an emission of heat with every decrease of volume. Accordingly, they could not but assume, with Laplace, that a unit mass of gas standing under constant pressure and expanding in consequence of the increasing of the temperature by 1 °C, consumes more heat than the same gaseous mass, confined to an unchangeable volume, absorbs on the same increase of temperature. Laplace was led to these questions by his investigations, soon to be spoken of, on the velocity of sound. The former quantity of heat, which Delaroche and Bérard had determined, was called the specific heat at constant pressure; and the latter quantity, which it is difficult to ascertain, for the above[17] reasons, was called the "specific heat at constant volume". Clément and Desormes[18] found, without intending it, a beautiful indirect method for the determination of the latter values.

The unit of mass of a gas is conceived at some definite temperature t and pressure p, for which it takes the volume v. If it is heated (I, Fig. 67) from t to $(t+1)$ °C, it expands by the fraction $\alpha/(1+\alpha t)$, determined by the coefficient of expansion α, of that volume which it had at t °C. It yields thus, simultaneously with the rise of temperature by 1 °C, the increase of volume $v\alpha/(1+\alpha t)$. The quantity of heat used for this, is the specific heat C at constant pressure.

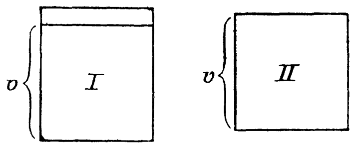


Fig. 67.

If, again, the temperature increase of 1 °C is imparted to the same gaseous mass (II) without permitting any expansion, a smaller quantity of heat c, the *specific heat at constant volume,* is expanded. If, now, in (I), the gaseous mass at $(t+1)$ °C is suddenly compressed by the fraction $\alpha/(1+\alpha t)$ of the whole volume, the difference $C-c$ must reappear and produce a heating of the gaseous mass by τ above

$(t + 1)$ °C. Since the volume is again the original one, we have $C = c(1 + \tau)$. Thus there only remains for the solution of the problem the determination of the temperature increase which corresponds to the compression $\alpha/(1 + \alpha t)$. But it is sufficient to determine some other small compression β and the corresponding temperature increase θ, as for small compressions the proportion β: $\alpha/(1 \div \alpha t) = \theta$: θ holds.

Clément and Desormes understood, from experiments which had an entirely different aim, how to utilize the compressed air itself as a thermometer. They employed a glass flask K (Fig. 68) with a cock H of wide bore. Attached to the flask was a glass tube rr which dipped into the mercury vessel Q. The air in the flask was somewhat rarefied by the

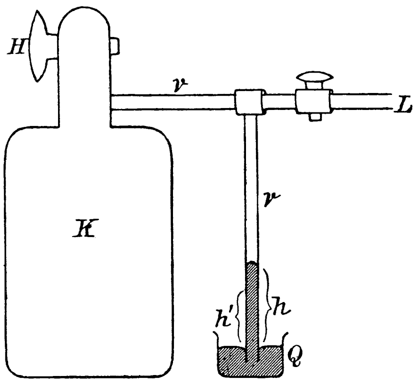


Fig. 68.

air pump L, and, by this, the mercury rose in the glass tube to h. Then the connection with the pump was removed. If the cock H with wide bore was opened, the mercury fell to its former level, but gradually rose again somewhat when the cock was again immediately closed. The air in K was quickly compressed and heated by the outer air without being able immediately to give up its heat, and only kept, in consequence of this heating, the equilibrium of the barometric height b. After dispersion of the heat, it showed only a smaller pressure.

The air, which at the beginning filled the flask space v and was under the pressure $b - h$, was compressed by the outer air to the volume v', and exerted, in consequence of the heating, the pressure b. After the escaping of the heat, however, this air showed merely the pressure $b-h'$. According to Mariotte's law we thus have $v'/v = (b-h)/(b-h')$, and the compression is

$$(v-v')/v = (h-h')/(b-h').$$

For the corresponding temperature increase θ, from Gay-Lussac's law follows

$$\frac{1+\alpha(t+\theta)}{1+\alpha t} = \frac{b}{b-h'}, \text{ or } \theta = \frac{h'(1+\alpha t)}{\alpha(b-h')}$$

From the above mentioned proportion follows, then,

$$\tau = h'/(h-h')$$

From this results from experiments with air

$$C/c = 1.357.$$

Observations with air pump experiments probably led to this form of experiment. If air is allowed to enter the exhausted receiver and the cock closed after the buzzing has ceased; this noise begins again, only more feebly, when the cock is reopened after some seconds. This may be repeated many times. Search for the explanation leads to the foregoing consideration.

The aim of the experiments of Clément and Desormes, was, as already noted, not the determination of the ratio C/c. Building on the ideas of Lambert[19] and Dalton[20] who supposed even the vacuum to be filled with heat substance, they sought to ascertain the "specific heat of the vacuum", and, by means of this, the absolute zero-point of temperature.[21] A work preliminary to this was formed by the determination of specific heats of gases by the ice calorimeter, and also by the method of determining the time of cooling or heating of receptacles filled with gases which themselves served as air thermometers. The authors imagined that the heating of a gas flowing into a vacuum was effected by the addition of the heat substance there contained to the heat of the gas itself, whereby the vacuum completely vanishes. Thus, the content for heat of the vacuum at 0 °C, for example, is found by an additional experiment, say at 100 °C. The difference suffices for the heating of the vacuum by 100°. The hundredth part of this divided into the content for heat at 0 °C leads, in round numbers, to the number 267 and thus determines the absolute zero-point to be 267° below the ice point. The authors laid down a principle on the basis of Gay-Lussac's overflow experiment in order to use this method when gas flows into slightly rarefied gaseous spaces instead of into empty ones. By other methods again — from the latent heat of water, from Gay-Lussac's expansion coefficients, and so on — the authors determined the absolute zero-point in round numbers as $-267°$; but it must be admitted that they rather forced the agreement between the results of different methods. The work of Amontons was not mentioned. The Paris Academy displayed its fine discernment in not crowning this very ingenious but too speculative work and giving the prize to the competitors Delaroche and Bérard.

7. The investigations on the ratio C/c alluded to received their chief impulse from the theory of the velocity of sound constructed by Laplace. Newton[22] was the first to discern that the velocity of sound can be computed. The essentials of his line of thought may be expressed in modern style as follows. A plane sound wave of wave-length λ advances towards the positive x-direction, for example, by the amount λ, while any particle of it executes a vibration of period T. If the T corresponding to a λ can be determined, then the velocity of sound is $\kappa = \lambda / T$.

If

$$u = a \sin 2\pi(t/T - x/\lambda)$$

is the displacement, supposed small, then $dx/(dx + du) = 1 - \partial u/\partial x$ is the density of the air, that of undisturbed air being taken as unity; or $-\partial u/\partial x$ is the small condensation. The force with which a layer of air of section q, of thickness dx, and of expansive force E (of the undisturbed air) is impelled is

$$qE \left[\left(1 - \frac{\partial u}{\partial x} \right) - \left(1 - \frac{\partial u}{\partial x} - \frac{\partial^2 u}{\partial x^2}\, dx \right) \right] = qE \frac{\partial^2 u}{\partial x^2}\, dx,$$

which will be referred to as (a). If ρ denotes the density of the air, the mass of the air-layer is $\rho\, q\, dx$, and consequently the acceleration is $(E/\rho)(\partial^2 u/\partial x^2)$.

But, since

$$\frac{\partial^2 u}{\partial x^2} = -\frac{4\pi^2 a}{\lambda^2} \sin 2\pi \left(\frac{t}{T} - \frac{x}{\lambda} \right) = -\frac{4\pi^2}{\lambda^2}\, u,$$

and is thus proportional to the excursion u, to the unit excursion corresponds the acceleration

$$\frac{E}{\rho} \cdot \frac{4\pi^2}{\lambda^2} = f,$$

which gives a period of vibration:

$$T = 2\pi\, 1/f = \lambda\sqrt{\rho/E}$$

and a velocity of sound:

$$\kappa = \sqrt{E/\rho}\,.$$

8. The Paris Academicians found in the year 1783, by experiments at 7.5 °C, a velocity for sound of 337.2 m/s, while calculation from the Newtonian formula gave, under the conditions of the experiment, only 283.4 m/s, so that the latter number is about a sixth too small. Lagrange was of the opinion that, in order to bring theory and observation into harmony, we must assume that the expansive forces increase more rapidly than the densities. Laplace, after various vain attempts to establish the harmony, came to the view that the changes of temperature caused by the sound waves themselves produce the increasing of the velocity of sound. At condensed places, rises of temperature are generated which are nearly proportional to these condensations and thus increases of pressure; while, at rarefied places, reductions of temperature and thus decreases of pressure are generated. In the same deformation, the differences of the expansive forces which represent the moving forces are greater than the Newtonian theory assumes them to be, and the actual velocity of sound must, accordingly, be greater than the velocity yielded by the Newtonian formula. According to Laplace, whose ingenious theory met, especially in Germany, with the most incredible misunderstanding,[23] all differences of force, for infinitely small oscillations, are greater in the ratio C/c and therefore the velocity of sound is greater, in the ratio $\sqrt{C/c}$, than Newton had supposed.

Laplace expounded these ideas clearly in a short communication of the year 1816. Upon the basis of experiments of Delaroche and Bérard, he assumed that, in round numbers, $C/c = 3/2$, and then, instead of the Newtonian velocity of sound (283), the value 345 results. Laplace ascribed the imperfect agreement of calculation with observation to errors of experiment, and finally gave expression to the important idea that the comparison of the Newtonian value of the velocity of sound with the observed value is the best means of determining the exact value of C/c, which he thus found[24] to be 1.4252. The very rapid sound vibrations represent an ideal carrying out of the experiment of Clément and Desormes, and in this idealized experiment no equalization of heat by conduction need be feared.

Laplace's result may be reached by the following simple consideration. The compression of a unit mass of gas by $\alpha/(1+\alpha t)$ releases the quantity of heat $C-c$, which heats this gaseous mass by $(C-c)/c$ degrees. The temperature increase θ, which corresponds to the condensation $-\partial u/\partial x$, is to the foregoing increase as $-\partial u/\partial x$ to $\alpha/(1+\alpha t)$. For the latter increase of temperature we thus obtain

$$-\frac{1+\alpha t}{\alpha}\,\frac{C-c}{c}\,\frac{\partial u}{\partial x}\,.$$

The pressure of an air layer of section q and condensation $-\partial u/\partial x$ is therefore increased in the ratio

$$\frac{1+\alpha(t+\theta)}{1+\alpha t}=1+\frac{\alpha\theta}{1+\alpha t}\,,$$

and is

$$qE\left(1-\frac{\partial u}{\partial x}\right)\left(1-\frac{C-c}{c}\,\frac{\partial u}{\partial x}\right).$$

By neglecting the higher powers of $\partial u/\partial x$, which we may do considering the smallness of $\partial u/\partial x$, we get

$$qE\left(1-\frac{C}{c}\,\frac{\partial u}{\partial x}\right).$$

If $\partial u/\partial x+(\partial^2 u/\partial x^2)\,dx$ is put for $\partial u/\partial x$, and the latter expression subtracted from the former, the difference is the force which affects the air layer between x and $x+dx$. It is

$$qE\,\frac{\partial^2 u}{\partial x^2}\,dx\,\frac{C}{c}\,.$$

The comparison of this expression with the one given in the formula (a) above results in Laplace's proposition.

9. Gay-Lussac and Welter[25] later made experiments for the purpose of determining C/c by the method of Clément and Desormes, but with condensed air, and found this value constant between the temperatures −20 °C and +40 °C and the barometric pressures 0.142 m and 2.300 m, and equal to 1.3748. But this value, determined in the interest of Laplace's theory, still did not give the full experimentally determined velocity of sound: it was somewhat too small.

Poisson[26] summarized and mathematically formulated the knowledge gained by the experiments of Clément and Desormes and of Gay-Lussac and Welter. The quantity of heat q contained in unit mass of gas depends upon the pressure p and the density ρ of the gas. We have

$$q = f(p, \rho) \tag{1}$$

But, by the laws of Mariotte and Gay-Lussac,

$$p = a\rho(1 + \alpha\theta) \tag{2}$$

If p in (2) is regarded as constant, then

$$\partial\rho/\partial\theta = -\alpha\rho/(1 + \alpha\theta)$$

If ρ is constant, then, on the other hand, we have

$$\partial p/\partial\theta = \alpha p/(1 + \alpha\theta)$$

For the specific heat with constant p we have, paying attention to (1),

$$C = \frac{\partial q}{\partial\rho}\,\frac{\partial\rho}{\partial\theta} = -\frac{\partial q}{\partial\rho}\,\frac{\alpha\rho}{1 + \alpha\theta}$$

and, on the other hand, for the specific heat at constant volume (ρ = const.),

$$C = \frac{\partial q}{\partial p}\,\frac{\partial p}{\partial\theta} = \frac{\partial q}{\partial p}\,\frac{\alpha p}{1 + \alpha\theta}$$

If, with Poisson, we make the assumption that $C/c = k$ is invariable, then follows

$$kp\,\frac{\partial q}{\partial p} + \rho\,\frac{\partial q}{\partial\rho} = 0$$

The integral of this partial differential equation is, as we can easily see by substitution,

$$q = \phi(p^{1/k}/\rho),$$

where ϕ denotes an undetermined function. Therefore we also have

$$p^{1/k}/\rho = \psi(q),$$

where ψ is the inverse function of ϕ.

If we assume that in any processes the quantity of heat contained in the gas remains unchanged, and consequently $\psi(q)$ remains constant, then we get

$$p^{1/k}/\rho = \text{const.},$$

or, by substitution of the volume v in place of $1/\rho$, the equation $v^k p =$ const, which represents the relation of pressure and volume with large variations of both, without taking up or giving off of heat.

10. Poisson preferred, after the precedent of Laplace, to derive the ratio $C/c = k$ from the observed velocity of sound, rather than to take, as the basis of the theory of the velocity of sound, values of k which were inexactly determined from complicated experiments.

Dulong[27] carried out these ideas in a comprehensive manner. The velocity of sound in different gases was determined by filling organ pipes with gases and blowing upon them until sounds were produced. From the number of oscillations n and the wavelength λ, derivable from the pipe length l, resulted the velocity of sound $\kappa = n\lambda$. Then it follows that $k = \kappa^2\rho/E$ for the gas in question, and, by help of this, the specific heat at constant volume may be immediately found if the specific heat at constant pressure is known.

Dulong found for:

	k		k
Air	1.421	CO	1.423—1.433
O	1.415—1.417	NO	1.343
H	1.405—1.409	C_2H_4	1.240
CO_2	1.337—1.340		

11. Summarizing, we may say that the changes of temperature of gases were imagined to have, in a way, a geometrical relation with changes of volume. Repeatedly the material theory of heat disturbed these ideas. The body changing in volume was imagined to be like a sponge which

by pressure yields the thermal substance and by dilation absorbs it again. For this reason, the Laplace-Poisson law, $v^k p =$ const, which, indeed, is still maintained to-day stood, at that period however, upon weak and, so to speak, arbitrary foundations. A clear and sure knowledge of the thermomechanical properties of gases had not yet been reached.

CHAPTER XIII

THE DEVELOPMENT OF THERMODYNAMICS. CARNOT'S PRINCIPLE

1. The view that thermodynamics begins with the conception of heat as "motion" is very widespread. But this idea was common among the philosophers of the middle ages, and therefore at a time when thermodynamics was unknown. Thus, it is found with Francis Bacon, who has been greatly praised without much justification. We must not be surprised at this, for the fire drill[1] of savages, the production of fire by striking steel on flint, the heating of pieces of metal which are being worked, and other technical experiences, must have been familiar to everybody long ago and approached closely to evidence of the connection between heat and motion.

Even neglecting the more ancient authors and their by no means precise statements, we may read the following in Huygens' *Traité de la Lumière*:[2]

> We cannot doubt but that light consists in the motion of a certain matter. For if we regard its production, we find that here on the earth it is chiefly fire and flame that generate it, and they contain without doubt bodies in rapid motion, since they dissolve and melt many other very solid bodies. If, on the other hand, we regard its effects, we see that, when light is collected, as it is by concave mirrors, it has the property of burning like fire, that is to say, it disunites the parts of bodies. And this certainly marks motion, at least in true philosophy, in which we conceive the cause of all natural effects as produced mechanically. This, in my opinion, must be done if we are not to give up every hope of understanding anything in physics.

The mechanical view of the whole of nature and in particular of the phenomena of heat can hardly be emphasized more forcibly and more clearly.

2. These ideas were never quite forgotten. Almost every writer on heat up to the end of the eighteenth century discussed such ideas, at least by the side of the material theory of heat, giving the preference to the one or the other or, again, without deciding between them at all. Eminent writers of this kind, whose views have been touched upon above are Pictet, Prévost, Black, and others. But we must particularly mention Lavoisier and Laplace.[3] We read in their memoir:

Physicists are not of one opinion about the nature of heat. Many among them consider it as a fluid .... Other physicists believe that heat is simply the result of unperceived motions of the molecules of matter .... In order to develop the latter hypothesis, we will remark that in all motions in which sudden variations do not occur a general law holds which mathematicians have called the 'law of the conservation of *vis viva*'. This law says that, in a system of bodies which act upon one another in any way, the *vis viva*, that is to say the sum of the products of the single masses, each into the square of its velocity, is constant .... The *vis viva* of the colder body will increase by the same quantity by which the *vis viva* of the other decreases .... We will not decide between the two above hypotheses. Many phenomena are in conformity with the second, for example, the phenomenon that heat rises by the friction of two bodies .... Now, in both hypotheses the free quantity of heat always remains the same when a simple mixture of bodies takes place.

It was then stated further that, according to both hypotheses, all the variations of heat which occur in a system whose state varies repeat themselves in a contrary sense when the system reverts to its original state.

Here, then, the kinetic theory of heat was retained together with the constancy of the free quantity of heat. The variation of the free quantity of heat (the binding or the freeing of heat) was attributed to molecular work. The auxiliary ideas of Lavoisier and Laplace were very nearly the same as the ones we now hold, but they did not lead to a thermodynamics. What happened to these ideas? Why did they not contain any constructive force? Were they perhaps, in the following stormy years, swept away like Lavoisier's head? But still Laplace was the chief supporter of these notions, and he studied the questions treated in the foregoing Chapter for a long time afterwards. Why was not his notion helpful and clarifying to him?

Benjamin Thompson, Count Rumford,[4] when engaged in superintending the boring of cannon in the workshops of the military arsenal at Munich, was struck with the great generation of heat in the boring of a brass gun. He found that the capacity for heat of the metal chips which were separated from the cannon by the borer was not less than the greater piece of metal from which the cannon was made. Thus the explanation, which was then favored, of the heat of friction by the diminution of the capacity for heat was not admissible. He put the tube that was to be bored into a water container and by the process of boring he brought the water to boiling point in two and a half hours. He calculated that the total heat developed could bring 26.58 pounds of ice cold water to boiling point, through 180 degrees Fahrenheit, and corresponded to the heat given out by the combustion of 2303.8 grams or four and four fifths ounces of wax. "One horse would have been equal to the work performed, though two were actually employed. Heat may thus be produced merely by the strength of a horse, and, in a case of necessity, this heat might be used in cooking victuals. But no circumstances could be imagined in which this method of procuring heat would be advantageous; for more heat might be obtained by using the fodder necessary for the support of a horse as fuel . . .".

In reasoning on this subject we must not forget *that most remarkable circumstance*, that the source of the heat generated by friction in these experiments appeared evidently to be *inexhaustible*. It is hardly necessary to add, that anything which any *isolated* body or system of bodies can continue to furnish *without limitation* cannot possibly be a *material substance*; and it appears to me to be extremely difficult, if not quite impossible, to form any distinct idea of anything capable of being excited and communicated in those experiments, except it be motion.

Humphry Davy[5] also combated the material theory of heat. He found that two pieces of ice at -1.7 °C melt owing to friction with one another. The supposition then current was that the increase of temperature produced by friction and percussion arises from a diminution of the capacities for heat of the acting bodies. "But", said Davy, "it is a well-known fact that the capacity of water for heat is much greater than that of ice; and ice must have an absolute quantity of heat added to it before it can be converted into water. Friction, consequently, does not diminish the capacities of bodies for heat". To show that no heat was communicated by surrounding bodies, Davy caused two pieces of metal to rub against each other by means of clockwork, the whole apparatus being placed on a block of ice which had some unfrozen water in a canal on its surface, enclosed in a nearly perfect vacuum produced by carbonic acid gas and caustic potash. Here again heat was developed by the friction, since wax on the metal was melted, but the heat did not come from the ice, for the water in contact with it was not frozen, nor from surrounding bodies, for in this case it would have to have passed through, and melted, the ice, but the ice remained unaltered. Davy then proceeded:

Heat, then, or that power which prevents the actual contact of the corpuscles of bodies, and which is the cause of our peculiar sensations of heat and cold, may be defined as a peculiar motion, probably a vibration, of the corpuscles of bodies, tending to separate them. It may with propriety be called the repulsive motion. Bodies exist in different states, and these states depend on the differences of the action of attraction, and of the repulsive power, on their corpuscles, or, in other words, on their different quantities of attraction and repulsion.

It does not, however, seem to be until 1812 that Davy[6] quite distinctly said: "The immediate cause of the phenomenon of heat, then, is motion, and the laws of its communication are precisely the same as the laws of the communication of motion".[7]

Thomas Young,[8] Ampère,[9] and other great investigators expressed themselves in the same sense as Davy. Thus the facts on which the science of thermodynamics is built were by no means unknown at the beginning of the eighteenth century. Nor were the intuitive ideas with which thermodynamics works then lacking. But these ideas had then an almost wholly contemplative, philosophical, and passive character; they did not — apart from Rumford's attempt — give the impulse to an accurate quantitative investigation of the connection between heat and work. At this time, it was only Black's material theory that had active and constructive force and was able quantitatively to represent the facts. It seems that, by the great results obtained by this theory in the period which immediately followed, attention was so turned away from the kinetic theory, and even from the facts favorable to it, that it was almost forgotten. Even the great founders of thermodynamics who will now appear on the scene had this prejudice — at least for a time.

4. Sadi Carnot, whose work we have first to consider, was very near to the kinetic theory of heat, but it did not come out in his memoir. "It will perhaps be objected", said he,[10]

that perpetual motion, which is proved to be impossible when mechanical actions alone are used, may possibly not be so when the influence of heat or of electricity is used. But can the phenomena of heat or electricity be conceived as due to anything else than to some motions of bodies, and must they not, then, be subject to the general laws of mechanics?

And yet, in this very treatise, Carnot maintained the constancy of the quantity of heat, and assumed that, when a body, after passing through a series of states, returns to its initial state, the quantities of heat which are absorbed and given out exactly compensate one another. "This fact has never been doubted; . . . . To deny it would be to overturn the whole theory of heat . . . . Be it said in passing that the main foundations of the theory of heat need the most attentive examination. Many facts of experience seem almost inexplicable in the actual state of this theory".[11] Indeed, later on, Carnot, as shown by the papers published after his death, gave up the supposition of the constancy of the quantity of heat, and even determined with fair accuracy the mechanical equivalent of the unit of heat.[12]

From the history of thermodynamics, we learn that the intuitive notions by means of which we obtain and facilitate our grasp of the facts are of far less importance than the accurate study of the facts themselves. By this study the notions spoken of adapt themselves and develop themselves to such an extent that they then attain a rich constructive power. Even the material theory of heat would not ultimately have hindered the full development of thermodynamics. One would have decided to assume a "latent heat of work" just as Black had assumed a latent heat of vapor; and, as has been already remarked, this step of Black's was quite in the direction of thermodynamics. The notions which serve for the representation of what is already known at one time favor the further progress of investigation and at another time stand in its way.

5. The path of thought which Carnot followed in his determination of the connection between heat processes and the performance of work ("motive power") is as follows:

Heat is capable of great performances of work. The best example of this is the steam engine. The great and striking motions on the earth also arise from heat. Now, is there no better medium than steam to bring about the performance of work by heat? Is this performance unlimited or has it a limit which is independent of the working material — such as steam or air? In order to recognize in its generality the principle of the performance of work by heat, our consideration must not be restricted to any special mechanism and agent — such as the steam engine — be must be applicable to every heat engine (*machine à feu*).

Every performance of work by heat is always connected with a reestablishment of disturbed thermal equilibrium, that is, the passage of caloric from a warmer to a cooler body. Not a consumption of caloric but the passage referred to determines the performance of work. Thus, in the steam engine, the caloric passes over with the steam from the warmer boiler to the cooler condenser. Not heat alone but also cold — in a word *temperature difference*, or disturbed heat equilibrium — is necessary for the performance of work. Everywhere where there is a difference of temperature, there can be a passage of caloric. With such passages are connected changes of volume of solid bodies such as metal bars, or of fluid or gaseous bodies, and thus performances of work; and the variations of volume are greatest with gases.

Now, is the performance of work of the unit quantity of heat constant for a given difference of temperature, or does it depend on the material which is used?

Wherever there is a difference of temperature, work can be obtained. Wherever work is at our disposal, we can produce — for example, by the compression of gases or vapors — a difference of temperature. If we imagine a boiler A of the temperature t_1 and another vessel B of the lower temperature t_2, we can take steam from A, let the steam expand in a cylinder, provided with a piston, and perform work till it sinks to the temperature t_2 and then condense it under pressure in B. By this, an excess of work (W) is obtained, because the compression has taken place at the lower temperature t_2 and the heat (Q) of the steam has passed over from t_1 to t_2. If, inversely, we take the same quantity of steam from B, compress it until its temperature rises to t_1, and then introduce it into A, we expend the work W and take the heat Q from B to A. Now, if there were a more advantageous working material — that is to say, if we could obtain, with the same quantity of heat (Q), a greater quantity of work (W'), — the heat Q could be returned to its source by means of the work W, and the work $W' - W$ would represent a net gain, and the *perpetuum mobile* would have been discovered.

Of course it may happen that different amounts of work may be obtained according to the different magnitudes of the accidental losses. But, if we suppose that all losses are avoided, the maximum of work theoretically obtainable when the heat Q is transferred, by means of steam, from t_1 to t_2 is the maximum for any working material whatever.

How is the maximum of work to be attained? Every reestablishment of the equilibrium of caloric *can* give rise to work. Every such equalization — that is to say, every variation of temperature — without work is accordingly a loss. The maximum is obtained if only such variations of temperature occur as are merely conditioned by variations of volume. On the other hand, every useless transference of heat, which occurs on contact of bodies of different temperature, must be avoided.

Carnot here remarked that the performance of work by heat is quite analogous to that by a waterfall. By the fall of heat (*chute du calorique*) the performance of work is determined in quite a similar manner to that performed by the fall of water (*chute d'eau*). But whilst for water the performance of work is simply proportional to the height of fall, we may not put this performance in the case of heat proportional to the difference of temperature without a closer investigation.

6. In order to determine the maximum of work mentioned, Carnot conceived a thought-experiment, the reversible cyclic process.

Imagine a body A of very great capacity for heat and at the temperature t_1, another body B of just the same nature and at the lower temperature t_2, and an absolutely non-conducting body C. Further, a cylinder M without ends, constructed of non-conducting material, is

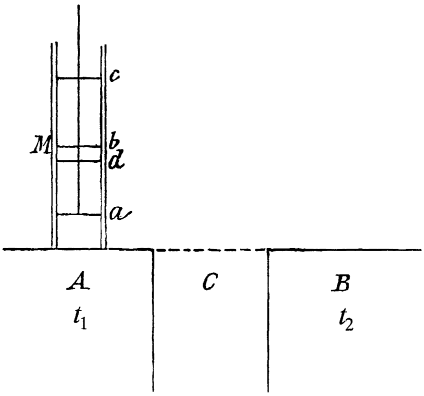


Fig. 69.

supposed to be capable of displacement along ACB. With this contrivance the following processes are imagined to be gone through:

(α) While the cylinder stands on A, the piston, which is always loaded with a weight equal to the pressure of the gas, rises from a to b; the gas being prevented from cooling by its taking heat from A so that it is kept at the temperature t_1.

(β) The cylinder is moved over C, so that heat cannot come to it from anywhere. The piston again rises under a pressure equal to that of the gas to c, which is so far that the temperature of the gas sinks to t_2.

(γ) The cylinder stands on B. The piston is pressed down by its return to d, and a rise of temperature is prevented by B, which absorbs the heat developed. In this operation, d is to be imagined to be so determined that if:

(δ) The cylinder is moved over C, and the compression to the original volume A takes place, the original temperature t_1 is also reached.

Now the series of processes may begin all over again.[13]

First of all we see that the process cannot actually be carried out. If the loading of the piston were *equal* to the pressure of the gas, no motion would take place. But we can imagine the loading to be as little different from the pressure of the gas as we wish, or even infinitely little different. Then the motion is very slow — infinitely slow in the later case. If the gas has the same temperature as A or B, *no* transference of heat at all takes place. But here again we may imagine an infinitely small difference of temperature in either sense. Carnot's process is therefore an ideal limiting case of all thinkable analogous actual processes. Carnot was quite clear about this point.

But this process has the following remarkable properties: (1) There nowhere occurs a contact of bodies of unequal temperature, and thus no useless diversion of heat without work. (2) All the variations of temperature that occur are consequences of variations of volume and therefore of work. By these two properties the attainment of the maximum of work is secured. (3) We can imagine the process proceeding in the opposite sense without altering its essential character. (4) After every cycle the working material — the gas — returns to its initial state and therefore contains exactly the same quantity of heat.

If the process proceeds in the sense described above, the gas performs an excess of work W, because the expansions of the gas take place at higher temperatures than the compressions; and consequently the elastic force of the air is greater during the times of expansion. On expansion a quantity of heat is taken from A, and on compression it is given to B; therefore a quantity of heat Q sinks from t_1 to t_2.

If the process is reversed, the compressions take place at higher temperatures and require an expenditure of work W. Heat will be taken from B and given to A. The heat Q rises from t_2 to t_1. The process is the exact inverse of the foregoing one.

Let us imagine two processes K_1 and K_2, which proceed with the quantity of heat Q between the same temperatures t_1 and t_2 and in the same sense, but with different materials, — for example, K_1 with air and K_2 with denser or more rarefied air, steam, or vapor of alcohol, — we must then suppose that both afford the *same maximum of work*.

If, for example, K_2 were to give a greater amount of work W', we could proceed with K_1 in the inverse sense with expenditure of the work W, which is less than W', and use $W' - W$ as net gain for a *perpetuum mobile.*

Thus, it results from Carnot's investigation that, *apart from all useless losses, W depends merely on the transferred heat Q and the temperatures,* t_1 *and* t_2 *between which the transference occurs, and not on the working material.* That is to say, we have

$$W = f(Q, t_1, t_2).$$

We may remark that the choice of a cyclic process for the derivation of this theorem was a particularly happy one. Indeed, nothing would stand in the way of determining the maximum of work spoken of by letting a body pass from a state a to a second state b, when all losses of work are avoided. Only then, in the second state, the body would, in general, contain a different quantity of heat from that which it did in the first. The thermal properties of bodies were very incompletely known in Carnot's time, and this lack of knowledge was, in a highly ingenious way, made of no moment by this very choice of a cyclic process.

7. The chief result of Carnot's investigation is expressed in the above theorem. Carnot then attempted to study more closely, under the guidance of the new principle, the properties of gases.

Imagine the temperatures of the two bodies A and B only infinitely little different, for example, let

$$t_2 = t_1 - dt.$$

The variations of volume β and δ, as well as the corresponding amounts of work performed, will then be infinitely small and may be left out of consideration. The cyclic process consists then in the expansion of the gas in contact with A at the temperature t_1 and in the compression of the gas to the original volume in contact with B at the temperature $t_1 - dt$. If we carry out the same process with two different gases M and N, which we expand with equal volumes v_0, equal pressures p_0, and the same temperature t_1, to the volume v_1, in contact with A, and then compress in contact with B to v_0, when cooled by dt, these gases develop, according to the law of Mariotte and Gay-Lussac, equal forces of expansion at homologous moments of the process, and provide the same amount of work. Accordingly, the heat transferred from A to B, that is to say the quantity of heat taken from A on expansion or that afterwards given up to B on compression must be the same. If, therefore, any gas at the constant temperature t passes from the volume v_0 and the pressure p_0 to the volume v and the pressure p, then the quantity of heat absorbed or given off in this process is independent of the nature of the gas.

8. Carnot determined in a very simple manner — relying on the inaccurate numbers of Poisson and Gay-Lussac — the ratio of the different specific heats of the gases. Imagine the unit mass of gas (I, Fig. 70) with the volume V at 0 °C. Compression by 1/116th heats this

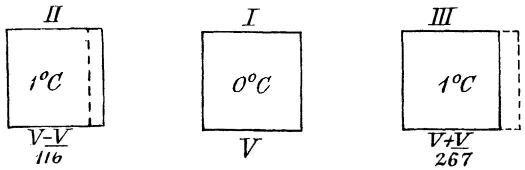


Fig. 70.

gaseous mass (II), according to Poisson, by 1 °C. Addition of the specific heat C at constant pressure (III) heats it by 1 °C and expands it, according to Gay-Lussac, by 1/267th of its volume. Now, II is only distinguished from III by volume and by the fact that III contains more heat by C. For expansion by (1/116 + 1/267)ths without variation of temperature the heat C will be expended. On the other hand, compression from I to II (1/116th) corresponds to a heating by 1 °C at constant volume, and consequently to the heat c. If we put the heatings proportional to the variations of volume, we get

$$C/c = (1/116 + 1/267)/1/116.$$

Imagine the gas, which has undergone the above cyclic process between t_1 and $t_1 - dt$, transferred from I to II (Fig. 71) in a cylindrical

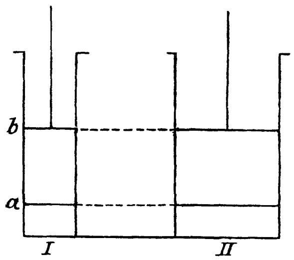


Fig. 71.

space of q times the cross-section but of the same height. All the gaseous densities and forces of expansion at homologous moments are now q times less; the pressures on a piston of q times the area, and consequently also the amounts of work performed with equal displacements (ab) of the piston, will remain the same. Accordingly the same quantities of heat will be absorbed on variations of volume. But the initial and final volume in II stand in the same ratio as they do in I. *When the arbitrary initial volumes of equal quantities of a gas at the same constant temperature vary in the same ratio, equal quantities of heat are absorbed or emitted.* This theorem may also be expressed as follows: *When a gas varies in volume at constant temperature, the quantities of heat absorbed or emitted form an arithmetical progression while the variations of volume are in geometrical progression.* Carnot used this law for the calculation of the pneumatic tinder-box.

Some of Carnot's discussions of gases rest on false but at that time accepted ideas about their properties. We may pass over these discussions here, the more so as they do not play an essential part with Carnot.

9. In a third part of his memoir, which is indeed not explicitly designated as such but is clearly recognizable, Carnot proceeded actually to determine the work of the transferred heat, and to determine by the comparison of different processes whether this work is independent of the body used.

Suppose that a kilogram of air which was initially under the pressure of one atmosphere, goes through a cyclic process between 0 °C and (0 − 1/1000) °C. The difference of the forces of expansion is then in homologous moments $(1/267) \times (1/1000)$ of an atmosphere or of the pressure of a column of water 10.4 m high. The air occupies a space of 0.77 m^3, and the whole expansion is, for convenience of calculation, to amount to $(1/110) + (1/267)$ of this. The excess of work performed in the cyclic process is then

$$(1/116 + 1/267) \times 0.77 \times 1/267000 \times 10.4.$$

where evidently the unit of work is the raising of one cubic meter of water to the height of one meter, and is therefore 1000 kilogram-meters. In terms of this unit the above calculation gives 0.000000372.

The quantity of heat consumed in the hypothetical expansion and given out in the compression, and therefore falling about one thousandth of a degree (C) in temperature, is therefore, in kilogram-calories, $C = 0.267$. If we suppose that the difference of temperature is 1000 times greater and therefore 1 °C, and that we have 1000 calories instead of 0.267 calories, the work is

$$1000 \times 1000/0.267$$

times greater, that is, in terms of the above unit, 1.395.

An analogous process was carried through with a kilogram of water which, when in contact with the boby A, at 100 °C, is transformed into steam of 10.4 m water pressure, and which occupies a space of 1.7 m^3; and is then compressed and liquefied in contact with a body B at a temperature of 99 °C under a force of tension of 0.36 m of water pressure. The work amounts to

$$1.7 \times 0.36 = 0.611$$

in the above units. In this process a latent heat of steam of 5.50 calories falls from 100 °C to 99 °C. For the fall of 1000 calories we thus obtain the work 1.112, which number as compared with 1.395 is considerably too small. However, we must reflect that the second calculation falls in quite a different region of the scale of temperatures, whereas processes with different bodies ought to be compared between the same limits of temperature. If we suppose that the heat of steam at 0° is 650, and carry out the calculation for this case, we get the number 1.290, which approaches 1.395 decidedly more closely.

An analogous calculation for the vapor of alcohol between the boiling point 78.7 °C and 77.7 °C gives the number 1.230. But steam gave 1.112 between 100° and 99°. If we calculate the effect for steam between 78° and 77°C, we find 1.212, which number lies much nearer to that for alcohol.

The agreement of the numbers found is only moderate. Carnot abstained from further comparisons in view of the inaccurate data which were accessible to him. At this place Carnot[14] again threw doubt on the foundations of the then accepted theory of heat.

10. The remainder of Carnot's treatise is devoted to a comparative critique of heat engines. Solid bodies were excluded, as means for obtaining work, since with them variations of volume are small and variations of temperature in consequence of variations of volume can hardly be shown. Great differences of temperature of parts in contact

would therefore be unavoidable with heat engines; but they are rejected by the theory as useless. Only vapors and gases are useful working materials. Since, in the case of steam, pressures above 6 atmospheres, which corresponds to 160 °C, are rarely used on account of the strength needed in the parts of the engine, only a small part of the height of temperature of the coal is made use of. The principle of high pressure engines was singled out as conforming to the theory and very advantageous. The advantages and disadvantages of hot air engines were critically discussed.

Finally it resulted that, according to a rough calculation, even the best steam-engines give hardly a twentieth of the theoretically possible useful effect of the heat of combustion of the coal.

11. The fundamental work of Carnot seems only to have become known to wider circles by means of the exposition of Clapeyron.[15] At the beginning of his memoir Clapeyron referred to the progress in the knowledge of the properties of gases, characterized as hypothetical the foundations of the works of Laplace and Poisson which have been dealt with above, and recapitulated the chief theorems of Carnot's treatise. Although Clapeyron worked entirely with Carnot's ideas, he yet performed a very important service by his convenient and perspicuous graphical and analytical representation of Carnot's theory. The essential part of his memoir is as follows.

Imagine the volumes of a gaseous mass represented as abscissae along the line *OV* in Figure 72, and the pressures as ordinates along the line *OP*.

(α) Let a gaseous mass expand in contact with a body *A* of very great capacity for heat and of temperature t_1 from the volume v_0 to the volume v_1 under a counter pressure always equal to its expansive force.

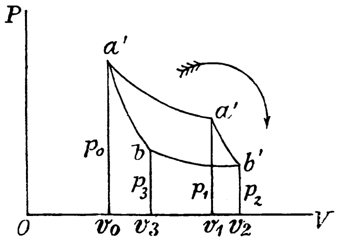
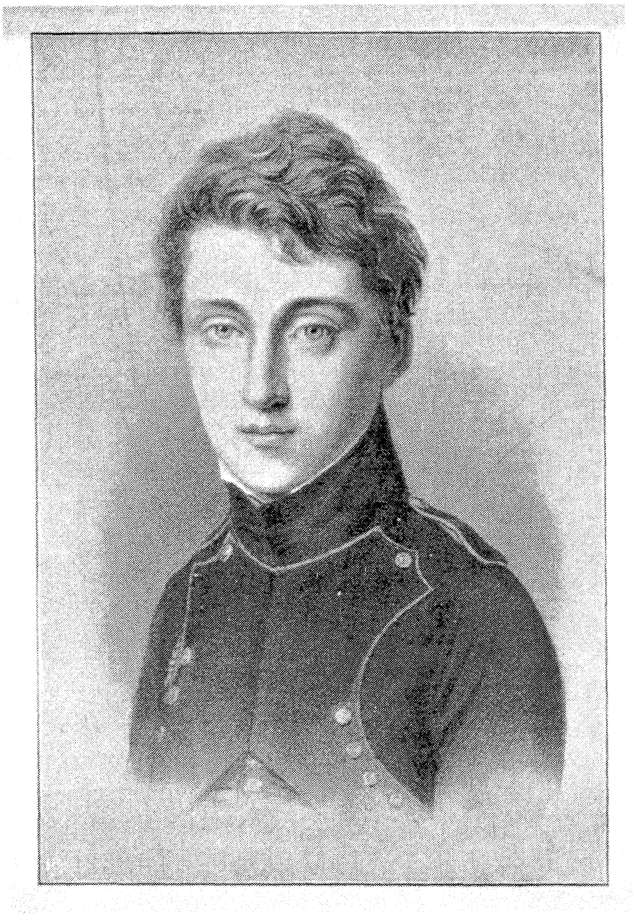


Fig. 72.



S. Carnot.

The pressure decreases from p to p_1 according to Mariotte's law, and the upper end of the ordinate of pressure describes the arc $a\,a'$ of an equilateral hyperbola.

(β) On a further expansion out of contact with A in an absolutely non-conducting envelope, up to the volume v_2, the pressure decreases more rapidly than would be the case according to Mariotte's law; it is not decided whether or no it is according to Poisson's law. The temperature also sinks, and v_2 is so chosen that the gas falls in temperature to t_2, the temperature of a body B of very great thermal capacity.

(γ) Now let a compression of the gas to v_3 take place when in contact with B. The pressure rises according to Mariotte's law and the temperature remains t_2.

(δ) On a further compression in the non-conducting envelope, the gas regains its original volume v_0, its original pressure p_0, and its original temperature t_1.

With regard to (γ) Clapeyron[16] said: "Suppose the compression (to v_3) be increased until the heat developed out of the gas and absorbed by the body B is precisely equal to the heat communicated to the gas by the source A during its dilatation in contact with it in the first part of the process". Clapeyron maintained that this was the condition that the original pressure and the original temperature should be reached with the volume v_0 in the process (δ). Here Carnot's idea that in the cyclic process the quantity of heat taken from A is *wholly* given to B, appears *explicitly*. On account of the connection with what follows it must here be remarked that this supposition is invalid. The volume v_3 is already determined by the condition that the gas, on reaching v_0, is again to have the initial pressure. Clapeyron's condition would contradict this determination.

The result of the whole cyclic process, in the sense given, is a performance of work represented by the surface $a\,a'\,b'\,b$[17] and a quantity of heat Q which has sunk from t_1 to t_2. On this quantity Clapeyron[18] said: "Still, the entire quantity of heat furnished by the body A to the gas during its dilatation by contact with it, passes into the body B during the condensation of the gas which takes place in contact with it". This again rests on Carnot's idea. Certainly a quantity of heat Q has sunk from A to B, yet, as will be seen, this is not the whole quantity of heat taken from the body A. Apart from this idea, which is harmless in many investigations owing to special circumstances, we can still, at the present time, maintain the correctness of Clapeyron's presentation.

If we carry out the cyclic process in the opposite sense to that of the arrow in Figure 72, we expend the same work W and raise the same quantity Q of heat, from t_2 to t_1.

An analogous process carried out with saturated vapor differs from the foregoing process in that aa' and $b'b$ in Figure 73 become straight lines parallel to the axis of abscissae, since when the temperature is constant, the pressure of the vapor remains the same. The considerations already brought forward can be repeated with slight alterations.

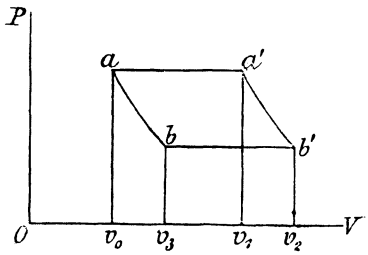


Fig. 73.

"From what precedes, it results that a quantity of mechanical action [work] and a quantity of heat passing from a hot to a cold body are quantities of the same nature, and that it is possible to substitute the one for the other reciprocally; just as in mechanics a body falling from a certain height and a mass endowed with a certain velocity are quantities of the same order, and can be transformed one into the other by physical agents.

Here also it follows that the work W developed by the passage of a certain quantity of heat Q, from a body A maintained at a temperature t_1, to a body B maintained at a temperature t_2, by one of the processes that we have just indicated, is the same, whatever be the gas or the liquid employed and is the greatest that it is possible to realize.[19]

Carnot's inference about the impermissibility of the *perpetuum mobile* was then repeated, and a reference was made to the similarity of Carnot's method to Lagrange's proof by pulleys of the principle of virtual displacements.

12. In order to determine the maximum amount of work which corresponds to the fall in temperature of a definite quantity of heat, Clapeyron chose a Carnot's cycle which proceeds between limits that are infinitely near to one another. This greatly simplifies the calculation.

Let a gaseous mass expand when in contact with A at the temperature t by the infinitely small volume $dv(\alpha\beta)$, and then let it expand in the non-conducting envelope, when it cools by dt; then let it be compressed when in contact with B at the temperature $t - dt$, and finally let it be brought back in the well-known way to the initial volume and temperature. The work performed is represented by the area $abcd$ in Figure 74, which, as we can easily show, is a parallelogram

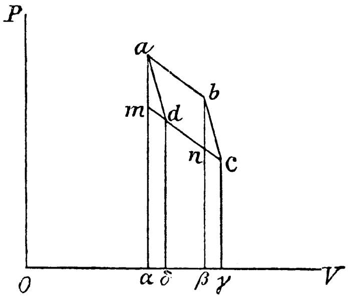


Fig. 74.

whose area is equal to $ab \cdot nm$ or $\alpha\beta \times bn$ or $dv\,dp$, where dp corresponds to the decrease in pressure from t to $t - dt$. By the law of Mariotte and Gay-Lussac we have

$$pv = R(a + t)$$

and thus

$$dp = R \cdot dt/v;$$

and consequently the work in question is $R \cdot dv \cdot dt/v$.

The heat transferred from t to $t - dt$ is at the same time[20] the heat taken from the body A on the expansion dv, and this heat is representable in the form

$$dQ = \frac{\partial Q}{\partial v}\,dv + \frac{\partial Q}{\partial p}\,dp,$$

since Clapeyron regarded p and v as the independent variables. Since, however, t is constant for the expansion, we have the equation

$$pv = \text{const. and therefore } p \cdot dv + v \cdot dp = 0,$$

or

$$dp = -p \cdot dv/v.$$

Consequently

$$dQ = \left(\frac{\partial Q}{\partial v} - \frac{p}{v} \frac{\partial Q}{\partial p} \right) dv.$$

If we divide the expression of the work performed by that of the quantity of heat transferred we get

$$\frac{R \cdot dt}{(v \cdot \partial Q/\partial v) - (p \cdot \partial Q/\partial p)}.$$

This value must be independent of the nature of the body used, and can only depend on the temperature t. Thus we can put, since R is a constant,

$$\frac{R \cdot dt}{(v \cdot \partial Q/\partial v) - (p \cdot \partial Q/\partial p)} = \frac{dt}{C}$$

where C (Carnot's function) is a function of the temperature which is the same for all bodies.

13. In contact with A, let the volume v of a saturated vapor, which undergoes the well-known cyclic process between t and $t - dt$, be generated at temperature t. If δ is the density of the vapor and ρ that of the fluid, then $v\delta/\rho$ is the volume of fluid from which the vapor has arisen and $(1 - \delta/\rho)v$ is the increase of volume on this generation of vapor. The area of the parallelogram *abcd*, in Figure 75, which represents the work is, by a consideration analogous to the foregoing one,

$$\left(1 - \frac{\delta}{\rho}\right) v \frac{\partial p}{\partial t} dt.$$

The transferred heat is at the same time the latent heat of steam of

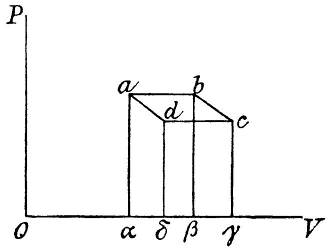


Fig. 75.

the volume v. If k is the latent heat for unit volume, the above expression is to be divided by kv. But, since the quotient must be equal to that of the gas for the same temperature, we have

$$\frac{(1-\delta/\rho)\cdot\partial p/\partial t}{k}=\frac{1}{C}$$

If we put approximately $\delta/\rho=0$, we have

$$k=C\cdot\partial p/\partial t.$$

Since C has the same value for all bodies at the same temperature, the heat of vaporization of equal volumes of different vapors at the same temperature is proportional to the coefficient $\partial p/\partial t$. More generally we have

$$k=(1-\delta/\rho)C\cdot\partial p/\partial t.$$

If the density of vapor gradually becomes equal to that of the fluid, it follows, since C and $\partial p/\partial t$ do not become infinite, that the heat of vaporization k sinks to zero.

14. Then followed the discussion of a still more general case. Let any body expand in contact with A at the temperature t by dv (represented by $\alpha\beta$ in Fig. 76), then let it be cooled by dt in contact with the body B of the temperature $t-dt$ and compressed by dv, and finally again heated by dt. In order that the cooling and the heating by dt may take place without a special expenditure of heat, we imagine very many bodies of great capacity for heat and graduated temperatures from t to $t-dt$ inserted between A and B. For cooling, the body to be investigated comes into contact with them all in order, and for heating this

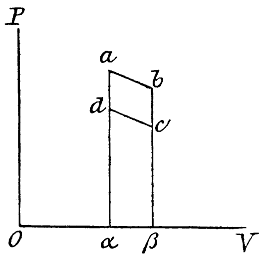


Fig. 76.

series of contacts takes place in the reverse order, so that we return to these sources the heat that has been borrowed from them in the first part of the operation. The work performed is again

$$dv\,\frac{\partial p}{\partial t}\,dt = \frac{dv \cdot dt}{\partial t/\partial p}.$$

The heat transferred or taken from A is

$$dQ = \frac{\partial Q}{\partial v}\,dv + \frac{\partial Q}{\partial p}\,dp.$$

But, since the temperature — which is independent of v and p — does not vary during expansion in contact with A, we have

$$dt = \frac{\partial t}{\partial v}\,dv + \frac{\partial t}{\partial p}\,dp = 0,$$

or

$$dp = -\frac{\partial t/\partial v}{\partial t/\partial p}\,dv,$$

and consequently

$$dQ = \left(\frac{\partial Q}{\partial v} - \frac{\partial Q}{\partial p}\,\frac{\partial t/\partial v}{\partial t/\partial p}\right) dv, \ldots \qquad (a)$$

and the work divided by the heat gives

$$\frac{dt}{\dfrac{\partial Q}{\partial v}\dfrac{\partial t}{\partial p} - \dfrac{\partial Q}{\partial p}\dfrac{\partial t}{\partial v}} = \frac{dt}{C},$$

or

$$\frac{\partial Q}{\partial v}\frac{\partial t}{\partial p} - \frac{\partial Q}{\partial p}\frac{\partial t}{\partial v} = C. \ldots \qquad (b)$$

The expression (a) for the heat dQ becomes, by means of this relation (b),

$$dQ = \frac{C \cdot dv}{\partial t/\partial p} = -\frac{C \cdot dp}{\partial t/\partial v} = -C \cdot dp \frac{\partial v}{\partial t}.$$

The last equation says that all bodies at the same temperature give, by the same increase of pressure, quantities of heat which are proportional to the coefficients of voluminal expansion of these bodies.

15. After these general discussions, Clapeyron sought a numerical determination of the function $1/C$. This was done most conveniently by using the properties of vapors. We have, namely,

$$\frac{1}{C} = \frac{\partial p/\partial t}{k},$$

and, from the observations of various physicists, we have

	$\partial p/\partial t$ in atmospheres at boiling point	Densities of vapors at boiling point; that of air being 1	Heat of vaporization in 1 kg of vapor	Boiling point in degrees centigrade	1/C
Sulphuric ether	1/28.12	2.280	90.8	35.5	1.365
Alcohol	1/25.19	1.258	207.7	78.8	1.208
Water	1/29.1	0.451	543.0	100	1.115
Essence of turpentine	1/30	3.207	76.8	156.8	1.076

According to Carnot, for air between 1° and 0 °C, we should have $1/C = 1.395$. Clapeyron, by using another mode of calculation, found the somewhat greater number 1.41.

From the supposition that saturated steam contains the same quantity of heat at every temperature, and that otherwise steam comes under the gaseous laws — which suppositions are certainly inaccurate —

Clapeyron derived a new series of values $1/C$, which deviate little from the preceding ones. For the temperatures, in degrees centigrade, 0, 35.5, 78.8, 100, 156.8 resulted the values: 1.586, 1.292, 1.142, 1.102, and 1.072, respectively.

We see that Clapeyron, although he contributed much to the more general and more distinct analytical formulation of Carnot's theory, did not — apart from some applications — go beyond Carnot's position. Also, later on Carnot's ideas acted in a stimulating way, but at long intervals. These thoughts first fell on fruitful soil in England.

16. William Thomson,[21] stimulated by Carnot's work, had the brilliant idea of founding on the basis of Carnot's law the definition of an absolute scale of temperature which should be universally comparable and independent of the choice of the thermometric substance. Thomson discussed the works of Regnault on gases, and remarked that different gas-thermometers agree with one another very well. But this is not an absolute scale, since we are still limited to the choice of certain thermometric susbtances.

We can only regard, in strictness, the scale actually adopted as *an arbitrary series of numbered points of reference sufficiently close for the requirements of practical thermometry.* In the present state of physical science, therefore, a question of extreme interest arises: *Is there any principle on which an absolute thermometric scale can be founded*? It appears to me that Carnot's theory of the motive power of heat enables us to give an affirmative answer.

According to Carnot, the performance of work of a unit of heat which sinks from one temperature to a lower one only depends on these temperatures. According to Clapeyron's investigation, the performance of a unit of heat which falls through one degree of the air thermometer is different in different parts of the scale and is less at higher temperature. Thomson suggested the choosing of degrees so that this effect in work is the same in all parts of the scale for the descent of the unit of heat through one degree. Such a scale then holds for all bodies.

The characteristic property of the scale which I now propose is, that all degrees have the same value; that is, that a unit of heat descending from a body A at the temperature T^0 of this scale, to a body B at the temperature $(T - 1)^0$, would give out the same mechanical effect, whatever be the number T. This may justly be termed an absolute scale, since its characteristic is quite independent of the physical properties of any specific substance.

The attempt to reduce the scale of the gas thermometer to this new scale was, of course, considering the incomplete knowledge of the properties of gases and vapors then possessed, incorrect, and on this account we will not go further into the question at present. But although the whole proposal has had to be modified by the development of the theory of heat, yet Thomson's extremely important idea was established for all time by these very modifications.

It is remarkable that Thomson, in the article of 1848 which we are now considering, still maintained the old assumption of Carnot's of the constancy of the quantity of heat. His opinion was that the transformation of heat into work was probably impossible. "This opinion", he said,

> seems to be nearly universally held by those who have written on the subject. A contrary opinion however has been advocated by Mr. Joule of Manchester; some very remarkable discoveries which he has made with reference to the *generation* of heat by the friction of fluids in motion, and some known experiments with magneto-electric machines, seeming to indicate an actual conversion of mechanical effect into caloric. No experiment however is adduced in which the converse operation is exhibited; but it must be confessed that as yet much is involved in mystery with reference to these fundamental questions of natural philosophy.[22]

These words spoken by a physicist of such a high standing at a time when everything in the question referred to had been clarified by Mayer, Joule, and Helmholtz, show how difficult it was for people to become accustomed to the new ideas. These words are very instructive for those people who consider that a thought expressed in 1842 was so generally grasped that no credit is to be allowed for the same discovery by another in 1843. There is not much historical understanding in this attitude, as we shall see in the following chapter. At the time spoken of these thoughts showed themselves strong enough to grow up only in those brains in which they had *spontaneously* arisen.

17. A year later — in 1849 — occupation with Carnot's theory led to the discovery of the lowering of the freezing point of water by pressure. Some time before this, William Thomson pointed out to his brother James Thomson that, by reasoning on Carnot's principles, it appeared that water at the freezing point may be converted into ice by a process solely mechanical, and yet without the expenditure of any mechanical work. Thereupon James Thomson[23] remarked that, since water expands while freezing mechanical work would be given out without any corresponding expenditure. He found the solution of the paradox in the

conclusion that the freezing point becomes lower as the pressure to which the water is subjected is increased. Carnot's theory then showed itself to be in complete harmony with the known facts.

The proof of William Thomson's conclusion was expressed as follows by James Thomson.[24]

Let there be supposed to be a cylinder, and a piston fitting water-tight to it and capable of moving without friction. Let these be supposed to be formed of a substance which is a perfect non-conductor of heat; also let the bottom of the cylinder be closed by a plate, supposed to be a perfect conductor and to possess no capacity for heat. Now, to convert a given mass of water into ice without the expenditure of mechanical work, let this imaginary vessel be partly filled with air 0 °C, and let the bottom of it be placed in contact with an indefinite mass of water, a lake for instance, at the same temperature. Now, let the piston be pushed towards the bottom of the cylinder by pressure from some external reservoir of mechanical work, which, for the sake of fixing our ideas, we may suppose to be the hand of an operator. During this process the air in the cylinder would tend to become heated on account of the compression, but it is constrained to remain at 0° by being in communication with the lake at that temperature. The change, then, which takes place is that a certain amount of heat is given from the hand to the air and a certain amount of heat is given from the air to the water of the lake. In the next place, let the bottom of the cylinder be placed in contact with the mass of water at 0° which is proposed to be converted into ice, and let the piston be allowed to move back to the position it had at the commencement of the first process. During this second process, the temperature of the air would tend to sink on account of the expansion, but it is constrained to remain constant at 0° by the air being in communication with the freezing water, which cannot change its temperature so long as any of it remains unfrozen. Hence, so far as the air and the hand are concerned, this process has been exactly the converse of the former one. Thus the air has expanded through the same distance through which it was formerly compressed; and, since it has been constantly at the same temperature during both processes, the law of the variation of its pressure with its volume must have been the same in both. From this it follows that the hand has received back exactly the same amount of mechanical work in the second process as it gave out in the first. By an analogous reason, it is easily shown that the air also has received again exactly the same amount of heat as it gave out during its compression; and, hence, it is now left in a condition the same as that in which it was at the commencement of the first process. *The only change which has been produced, then, is that a certain quantity of heat has been abstracted from a small mass of water at 0°, and dispersed through an indefinite mass at the same temperature, the small mass having thus been converted into ice.* This conclusion, it may be remarked, might be deduced at once by the application, to the freezing of water, of the general principle developed by Carnot, that no work is given out when heat passes from one body to another without a fall of temperature; or rather by the application of the converse of this, which of course equally holds good, namely that no work requires to be expended to make heat pass from one body to another at the same temperature.

Everything would be satisfactory if the freezing water could not perform work, that is to say, if the ice which performs work, and is therefore under pressure, has a lower melting point, and consequently the expanding air is colder than the compressed air.

James Thomson then determined the freezing point of water for *any* given pressure as follows:

Let us suppose that we have a cylinder of the imaginary construction described at the commencement of this paper; and let us use it as an ice engine analogous to the imaginary steam engine conceived by Carnot and employed in his investigations. For this purpose, let the entire space enclosed within the cylinder by the piston be filled at first with as much ice at 0° as would, if melted, form rather more than a cubic foot of water, and let the ice be subject merely to one atmosphere of pressure, no force being applied to the piston. Now, let the following four processes, forming one complete stroke of the ice engine, be performed.

Process 1. Place the bottom of the cylinder in contact with an indefinite lake of water at 0°, and push down the piston. The effect of the motion of the piston is to convert ice at 0° into water at 0°, and to abstract from the lake at 0° the heat which becomes latent during this change. Continue the compression till one cubic foot of water is melted from ice.

Process 2. Remove the cylinder from the lake, and place it with its bottom on a stand which is a perfect non-conductor of heat. Push the piston a very little farther down, till the pressure inside is increased by any desired quantity which may be denoted, in pounds on the square foot, by p. During this motion of the piston, since the cylinder contains ice and water, the temperature of the mixture must vary with the pressure, being at any instant the freezing point which corresponds to the pressure at that instant. Let the temperature at the end of this process be denoted by $-t$ °C.

Process 3. Place the bottom of the cylinder in contact with a second indefinitely large lake at $-t°$, and move the piston upwards. During this motion the pressure must remain constant at p above that of the atmosphere, the water in the cylinder increasing its volume by freezing, since, if it did not freeze, its pressure would diminish, and therefore its temperature would increase, which is impossible since the whole mass of water and ice is constrained by the lake to remain at $-t°$. Continue the motion till so much heat has been given out to the second lake at $-t°$ as that if the whole mass contained in the cylinder were allowed to return to its original volume without any introduction or abstraction of heat, it would assume its original temperature and pressure. This, if Carnot's principles be admitted, as they are supposed to be throughout the present investigation, is the same as to say, — Continue the motion till all the heat has been given out to the second lake at $-t°$ which was taken in during Process 2 from the first lake at 0°.

Process 4. Remove the cylinder from the lake at $-t°$, and place its bottom again on the non-conducting stand. Move the piston back to the position it occupied at the commencement of Process 1. At the end of this fourth process the mass contained in the cylinder must, according to the condition by which the termination of Process 3 was fixed, have its original temperature and pressure, and therefore it must be in every respect in its original physical state.

By representing graphically in a diagram the various volumes and corresponding pressures, at all the stages of the four processes which have just been described, we shall arrive, in a simple and easy manner, at the quantity of work which is developed in one complete stroke by the heat which is transferred during that stroke from the lake at 0° to the lake at $-t°$. For this purpose, let a be the position of the piston at the beginning of Process 1; and let some distance, such as ac, represent its stroke in feet, its area being

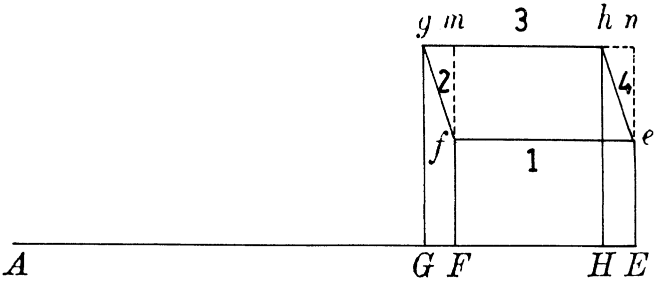


Fig. 77.

made a square foot, so that the numbers expressing, in feet, distances along ac may also express, in cubic feet, the changes in the contents of the cylinder produced by the motion of the piston. Now, when 1.087 cubic feet of ice are melted, one cubic foot of water is formed. Hence, if ab be taken equal to .087 feet, b will be the position of the piston when one cubic foot of water has been melted from ice, that is, the position at the end of Process 1, the bottom of the cylinder being at a point distant from b by rather more than a foot. Let bc be the compression during Process 2, and da the expansion during Process 4.

Let ef be parallel to ab, and let ae represent one atmosphere of pressure; that is, let the units of length for the vertical ordinates be taken such that the number of them in ae may be equal to the number which expresses an atmosphere of pressure. Also let gh be parallel to ab, and let fm represent the increase of pressure produced during Process 2. Then the straight lines of ef and gh will be the lines of pressure for Processes 1 and 3; and, for the other two processes, the lines of pressure will be some curves which would extremely nearly coincide with the straight lines fg and he. For want of experimental data, the natures of these two curves cannot be precisely determined; but, for our present purpose, it is not necessary that they should be so, as we merely require to find the area of the figure $efgh$, which represents the work developed by the engine during one complete stroke, and this can readily be obtained with sufficient accuracy. For, even though we should adopt a very large value for fm, the change of pressure during Process 2, still the changes of volume gm and hn in Processes 2 and 4 would be extremely small compared to the expansion during the freezing of the water; and from this it follows evidently that the area of the figure $efgh$ is extremely nearly equal to that of the rectangle $efmn$, but fe is equal to ba, which is .087 feet. Hence the work developed during an entire stroke is $.087 \times p$ foot-pounds. Now this is developed by the descent from 0° to $-t°$ of the quantity of heat necessary to melt a cubic foot of ice; that is, by 4925 thermic units, the unit being the quantity of heat required to raise a pound of water from 0° to 1° centigrade.

Next we can obtain another expression for the same quantity of work; for, by the tables deduced in the preceding paper[25] from the experiments of Regnault, we find that the quantity of work developed by one of the same thermic units descending through one degree about the freezing point, is 4.97 foot-pounds. Hence, the work due to 4925 thermic units descending from 0° to $t°$ is $4925 \times 4.97 \times t$ foot-pounds. Putting this equal to the expression which was formerly obtained for the work due to the same quantity of heat falling through the same number of degrees, we obtain

$$4925 \times 4.97 \times t = .087 \times p.$$

Hence

$$t = .00000355\,p. \tag{1}$$

This, then, is the desired formula for giving the freezing point $-t°$ centigrade which corresponds to a pressure exceeding that of the atmosphere by a quantity p, estimated in pounds on a square foot.

To put this result in another form, let us suppose water to be subjected to one additional atmosphere, and let it be required to find the freezing point. Here $p = 1$ atmosphere $= 2120$ pounds on a square foot; and therefore, by (1),

$$t = .00000355 \times 2120, \text{ or } t = .0075.$$

That is, the freezing point of water, under the pressure of one additional atmosphere, is $-.0075°$ centigrade; and hence, if the pressure above one atmosphere be now denoted in atmospheres, as units, by n, we obtain t, the lowering of the freezing point in degrees centigrade, by the following formula,

$$t = .0075n.$$

William Thomson[26] verified this conclusion by determining the melting point of ice in an Oersted's apparatus for the compression of water. He found that a pressure of 8.1 atmospheres produce a lowering of temperature of .106 °F, and a pressure of 16.8 atmospheres produce a lowering of temperature of .232 °F; and these numbers agree very closely with the numbers theoretically predicted.

18. These brilliant applications of Carnot's theory certainly contributed much to heighten interest in it and to make its fruitfulness evident.

Carnot, whose thoughts still dominate the whole of thermodynamics, and whom we get to know as a lovable and noble personality, both by the biography, written by his brother, Lazare Hippolyte Carnot, with such affectionate admiration, and by the notebook which he left behind him, had a rare nature. He affords us the very pleasant spectacle of a genius who, without a special effort or a laborious expenditure of scientific means, and merely by considering the simplest experiences, discovered the most important things almost, so to speak, without trouble.

CHAPTER XIV

THE DEVELOPMENT OF THERMODYNAMICS. THE PRINCIPLE OF MAYER AND JOULE. THE PRINCIPLE OF ENERGY

1. While the foundations of thermodynamics were being laid, the growth of ideas on the nature of heat did not stop. These ideas developed and first blossomed, apparently, with Sadi Carnot himself. Carnot died of cholera in 1832, and the notes he wrote after 1824 were first made known in an appendix to the reprint of his treatise on the motive power of heat which was published in 1878.[1] It is clear from these that Carnot; towards the end of his life, gave up his belief in the constancy of the quantity of heat, and assumed that heat was produced by the expenditure of mechanical work and inversely work by heat, and it also appears that he even knew fairly accurately the value of the mechanical equivalent of the unit quantity of heat.

Séguin[2] in 1839 knew the qualitative relation between heat and work. "There must", he said, "be an identity of nature between caloric and motion, so that these two phenomena are only the manifestation, under different forms, of the effects of one and the same cause. These ideas were transmitted to me long ago by my uncle Montgolfier."[3] He knew that steam, when it is immediately condensed, must warm the water more than it does after it has performed work; he considered the variation of the temperature of a gas when the volume is varied, in connection with the work performed;[4] and so on. But Séguin only gave a calculation of the mechanical equivalent of heat in 1847,[5] when he was moved to do this by a communication of Joule's,[6] and then he did so on the basis of data which he had himself collected.

The transformation of work into heat and reciprocally was expressed quite clearly by the Heilbronn physician Julius Robert Mayer in the year 1842. His publication also contained a fairly accurate determination of the mechanical equivalent of heat and the method of calculating this equivalent from the numbers then generally known.

With respect to the two last points as well as the first, Mayer has the priority of publication over all other physicists. He hardly touched upon the domain of experiment.

As early as 1843, Ludwig August Colding, the chief engineer of the city of Copenhagen, was occupied with similar thoughts. It has been urged against Mayer that he started from too general considerations belonging to the philosophy of nature and "metaphysics"; but this is true in a much greater degree of Colding. Forces are of a spiritual nature, in Colding's opinion, and they cannot be destroyed, but only transformed. According to his own account Colding was led to his views by d'Alembert's *Traité de Dynamique* in 1843. In this year he made a communication to the Scientific Society of Copenhagen, in which he considered, from his point of view, Dulong's experiments on the compression of gases, Rumford's experiments, and other things. He was urged by Oersted to undertake experimental researches, and then he generated heat by friction, found that the quantity of heat was proportional to the work, and discovered that the equivalent to the kilogram-calorie was the work of 350 kilogram-meters. In his general considerations he, as well as Carnot relied on the principle of the excluded *perpetuum mobile.* Further communications to the Copenhagen Society were made by him in 1848, 1850, and 1851.[7]

In 1843 James Prescott Joule began a magnificent series of experiments which were continued until the year 1878, in proof of the general validity of the proportionality between work and heat, and for the exact determination and proof of the constancy of the mechanical equivalent of heat.[8] With respect to more general and philosophical questions Joule was very silent, but, when he did speak, his utterances were very similar to those of Mayer. We cannot, indeed, doubt that such comprehensive experimental investigations having a common end could only have been carried out by one who was thoroughly permeated by a view of nature of great philosophical depth.

In Mayer's first publications, only the transformations of heat into work and reciprocally were considered. In his second publication of 1845,[9] the idea had already gained in generality, and had extended to what we now call "the principle of the conservation of energy". Every variation of physical or chemical state which is brought about by work is *equivalent* to this work, and can, on reversal of the process, again generate that work. A great program was put forward, which embraced physics, chemistry, and physiology, and which presented clearly the work of investigation to be carried out from this point of view. But in many of the cases enumerated, Mayer, on account of his lack of knowledge about the subjects concerned, had to content himself with the program and was not able to proceed to the actual carrying out of the work.

A necessary supplement to Mayer's memoir was formed by the tract of Helmholtz *Ueber die Erhaltung der Kraft* which appeared in 1847. What was a program with Mayer was here, two years later, carried out with great ability. What, with Mayer, makes more the impression of something immediately perceived, looks here more like the necessary result of a profound and thorough study. It is as though all the seeds which lay in physics had suddenly received new life and growth.

Rather later, but with very plentiful results, Gustav Adolph Hirn, an engineer in Colmar, took part in the building up of thermodynamics. The *Revue d'Alsace* of 1850 to 1852 contained general considerations by him, and this periodical for 1858 contained investigations on the mechanical equivalent of heat. In 1856 he proved that vapor loses heat by mechanical work, and that this heat therefore does not appear in the condenser of a steam engine.[10] To him was due probably the first exact experiment having for its object the application of metrical thermodynamics to physiology.

2. In the heading of this chapter we have called the theorem of the equivalence of heat and work "the principle of Mayer and Joule". In fact, since, for practical reasons, we cannot name the theorem after all the persons who took part in its discovery and foundation, it is best to connect it with the names of those who must be allowed the priority of publication in these two directions. It would be more difficult to name the theorem of the conservation of energy after particular persons: the germs of this theorem, the conviction of the impossibility of the *perpetuum mobile* and of the significance of work, lie such a long way back in time, and, on the other hand, these convictions have been clarified so gradually, that we can hardly name them after particular persons without injustice to others. Only think, for example, of the attitude that Sadi Carnot took towards this theorem. Still, the most powerful advocacy of the theorem of the conservation of energy was due to Mayer and Helmholtz.

Very bitter controversies have arisen about the first promulgation of these ideas, and in them detestable personal calumnies and offensive national chauvinism showed themselves. The preceding survey clearly shows that these ideas cannot be considered to be the exclusive property of a particular nation and still less of a particular person. They were prepared for, and, when the time was ripe for them, grew up almost simultaneously in different brains which were quite independent of one another. We ought to regard it as a piece of good fortune that the development of science is not limited to one nation or to one brain; and we should reflect on the different ways in which these ideas were fostered by different investigators with different personal characteristics, the gain which has accrued to science from this difference, and the gain which accrues from it to the theory of knowledge.

We are very liberal with the accusation that ideas are borrowed, and do not reflect that all investigators share in the common convictions of their time and consequently are more or less accessible to the same ideas. We ought also to consider more the ready stimulus to independent inquiry that comes from conversation, even from a single word, or from hearsay.[11] This mobility and ready transferability of thoughts, which make it impossible to gain and keep these thoughts as exclusive personal property, is again a piece of great good fortune. If it were possible to make thoughts our personal property, a regular caste of capitalists in thoughts would rise up and these would certainly be the most dangerous of all capitalists. Finally, we must remember that the borrowing of a thought is much more difficult to prove than the borrowing of a thing.

It is not to be disguised that, in the controversies about priority which were stimulated by the questions just mentioned, very important achievements have been given a very cool reception by learned and highly placed scientific authorities. Yet, when has it been demanded that anyone should be wholly without bias in matters which concern himself? Why should he be required to be so only in the domain of science?

3. After this general survey, we will consider some important points rather more closely. In the first place, we will turn to the notebook which Carnot left behind him. We cannot exhaust the whole mine of thoughts contained in this notebook without going too far from the object of this chapter, and thus we must here confine ourselves to the most important parts of it.

The variations of temperature produced by motion have been studied too little said Carnot. Where work is consumed or generated, noticeable variations in the distribution of heat occur, and perhaps also variations of the quantity of heat. This is the case with the impact of bodies. The explanation of the production of heat by variation of volume does not here suffice: a cube of lead is heated by hammering on all its faces in succession, without its volume being changed.

When a hypothesis is no longer sufficient for the explanation of the phenomena, we must let it drop. This is the case with the hypothesis of caloric, for this hypothesis does not allow of an explanation of the heating by impact or friction. If we pump air out of a receiver while we let the outer air into this receiver, the air compressed by the pump escapes with a higher temperature, which is therefore produced by the work performed. Gay-Lussac's experiment with the two balloons was mentioned, but Carnot did not seem to draw the proper conclusions from it.

Light is now admitted to be a result of vibrations of the ether; so also is radiant heat. Can motion (radiant heat) generate matter (caloric)?

When we generate motive power by the passage of heat from the body A to the body B, the quantity of this heat which arrives at B (if it is not the same as that which has been taken from A, and thus if a part has been really consumed to produce the motive power), is this quantity the same, whatever may be the body used to realize the motive power?

Can there be a way of consuming more heat in the production of the motive power and of making less heat arrive at the body B? Can it be even wholly consumed, so that none whatever arrives at the body B? If that were possible, we could create motive power without the consumption of anything combustible, and that by simple destruction of the heat of bodies.

Is it quite certain that steam, after having acted in an engine and having there produced motive power, is capable of raising the water of condensation, as if it had been immediately brought there? . . .

Heat is nothing but motive power, or rather motion which has changed its form. It is a motion in the particles of bodies. Wherever there is a destruction of motive power, there is at the same time a production of heat in quantity exactly proportional to the quantity of motive power destroyed. Inversely, wherever there is a destruction of heat, there is a production of motive power.

We can, then, lay down as a general thesis that motive power exists in an invariable quantity in nature, and that it is, properly speaking neither produced nor destroyed. It is true that it changes its form, that is to say, it produces sometimes one kind of motion and sometimes another, but it is never destroyed.

According to some ideas which I have formed on the theory of heat, the production of a unit of motive power necessitates the destruction of 2.70 units of heat.

An engine which produces 20 units of motive power per kilogram of coal must destroy $20 \times 2.70/7000$ of the heat produced by combustion; this fraction is about 8/1000, that is to say less than 1/100.[12]

Since Carnot always used the kilogram-calorie as the unit of heat and 1000 kilogram-meters as the unit of work, we have from what precedes a mechanical equivalent of heat of 0.370 kilogram-meters in round numbers.

Then followed a series of notes on experiments which were to be made, and which contained almost everything that Joule, Hirn, and others carried out. There is no doubt that Carnot considered, as early as 1824, that the foundations of the theory of heat then in vogue were not firm, and even at that time, planned a universal mechanical physics. It is not surprising that he developed these thoughts further in the course of years. However, it is difficult to determine, at the present time, in what way he arrived at a knowledge of the mechanical equivalent of heat. But let us suppose that the transformation of work into heat became clear to him in the phenomena of impact, and that he had transferred this view to the heating process that takes place on the compression of gases and then to the opposite process of cooling on gaseous expansion. Then he might have determined the heat necessary for a small isothermal gaseous expansion. If, now, he broke off in thought his cyclic process with air after the first operation, let the hypothesis of a sensible variation of specific heat with the variation of volume fall — exactly as he gave it up in Rumford's case —, and used the heat absorbed for the calculation not of the increase of volume but of the work, the mechanical equivalent would be given. Indeed, this way was not essentially different from that which was actually adopted after Carnot's time.

The case of Carnot is very instructive for those persons who consider it impossible that the same thought can arise at the same time in different brains. If only Carnot had lived a few years longer and if those thoughts of his which had been lost to knowledge for fortysix years had been made known earlier, the balance of fame would indeed have been in a very different position from that in which it is to-day.

4. We will now turn to the consideration of Mayer's work, and may confine ourselves to the most important features of it, especially as all that pertains to this investigator is now at the disposal of all who care to read about it.[13] Mayer received the impulse to his investigations by a chance. On letting blood in Java, the intense redness of the venous blood struck him. He connected this fact with Lavoisier's theory — the theory according to which animal heat is the result of a process of combustion. A diminished loss of heat owing to the surroundings

implies less combustion. All the activities of the organism are owing to combustion. The total emission of heat of an animal body must exactly correspond to the heat of the material burnt. But since we can also produce heat mechanically — by friction — and since this heat is to be taken account of in calculating the emission, there must be a fixed relation between the "mechanical force" (work) and the heat produced. This way of thinking also explains Mayer's tendency to look upon all processes of nature as substantial, as well as his conception "force", which is precisely that which had borne the name of "work" in mechanics for a long time past.

The working out of these ideas occupied his whole attention during his voyage and after his return. He was thoroughly filled with the importance of the matter:

> . . . I then applied myself again to physics, and clung to the subject with such love that I — and many will laugh at me for this — was little concerned with that far part of the world, and preferred to be aboard where I could work uninterruptedly and where I often felt myself, as it were, inspired in a way which, to the best of my remembrance, I have never been either before or since . . . . It is quite certain that the day will come when these truths will become the common property of science; but by whom this gift will be made and when it will happen, who can tell?[4]

At first his very defective knowledge of physics caused him grave difficulties. In his discussion by correspondence with his friends Baur and Griesinger, he confused *vis viva* (mv^2) with quantity of motion (mv). But gradually these difficulties were overcome.

Conversations with Nörrenberg and Jolly, which did not quite satisfy him, showed him the way to the region of experiment. Jolly later admitted quite openly that it was very difficult for him, with the prejudices of his school, to arrive at any understanding of what Mayer said. At Jolly's objection: "Then water must become warmer on shaking", Mayer went away without a word. After many weeks a man burst into Jolly's study and simply said: "It is so!"[15] It was Mayer, whom Jolly hardly recognized and who was under the impression that Jolly too had been dwelling on the same thought ever since the former interview.[16]

Mayer was unfortunate with his first manuscript; it was sent to Poggendorff for his *Annalen,* but it was never printed [in Mayer's lifetime]. If, as is probable, it contained mistakes similar to those in Mayer's letters to Baur, this attitude of a professional man whose outlook was a narrow one is quite comprehensible, although Mayer was certainly worthy of an answer from Poggendorff. A man whose mind was far wider, Liebig, published the second manuscript in his *Annalen.*[17]

5. This communication shows throughout such great originality on the part of Mayer that it offends against almost every custom of physical and mathematical terminology. The conceptions which Mayer knew did not suffice for him; he simply brushed them aside and put new ones in their place. And yet what Mayer said with his new names is so clearly shown to anyone who wishes to follow, that a misunderstanding is not possible. The attempt to draw consequences, which are to have physical validity, from general formal theorems repels the natural scientist as long as he has not made it clear to himself that those theorems merely expressed Mayer's strong and not then clarified formal need for a substantial conception of work and energy.

"The aim of the following lines", said Mayer,

> is an attempt to answer the question as to what we must understand by 'forces', and how such forces behave to one another . . . Forces are causes, and consequently they are subject to the maxim: *causu aequat effectum* . . . The first property of all causes is their incapability of being destroyed . . . If the given cause *c* has generated an effect *e* equal to it, then precisely by that generation, *c* has ceased to be; *c*, in fact, has become *e* . . . forces are therefore indestructible, transformable, and imponderable objects . . . Now, if it is established that, in many cases (the exception confirms the rule) no other effect can be found for a vanishing motion than heat, and for the heat that has arisen no other cause than motion, we must prefer the hypothesis that heat consists in motion to the hypothesis of a cause without an effect and of an effect without a cause, just as the chemist, instead of letting hydrogen and oxygen vanish and water arise in an unexplained or at least uninquired into way, affirms a connection between hydrogen and oxygen, on the one hand, and water on the other.

If Mayer had expressed himself, say, as follows: "Henceforth I will, because it corresponds to my need, only call that a 'cause' which has an effect which is *equivalent,* but not *equal,* to it, and from which the former can be again obtained; further, I will call a cause which is immaterial by the name 'force' ", then hardly anything could have been urged against him. Obviously there is no *a priori* proposition from which properties of nature can be deduced. But I may have a need for a certain form of conception prior to any special investigation of mine, and may then see whether I can satisfy it.

Mayer followed out his ideas with a powerful formal instinct. We can hardly believe, in the face of his own words, that his own intellectual position, from the point of view of the theory of knowledge, was ever quite clear to him. However he wrote to Griesinger;[18] "If you ask me how I got mixed up with the whole business, my simple answer is that, when I was on my sea voyage, I was almost exclusively occupied with the study of physiology, and found the new theory for the sufficient reason that I vividly felt the need for it . . .".

I arrived at the view that Mayer's theory has its origin in a formal need by trying to put myself into the intellectual position of Mayer. I have maintained this view for a long time — since 1871 — and have expressed it in various publications.[19] I believe that I can say that this view was very fully supported by the letters of Mayer first published in 1889 and 1893. If this is granted we shall not be inclined to speak any more of a "metaphysical" foundation of Mayer's theory.

6. But all attempts to represent Mayer's claims as unfounded must fall to the ground in view of the conceptual clearness to which he finally attained when he gave the magnitude of the mechanical equivalent of heat and distinctly stated, in a few words, the way in which it is calculated. He was the first of all physicists to see that no new experiments are necessary for this determination, and that generally known numbers suffice for it. He was also the first to grasp correctly Gay-Lussac's overflow experiment, and to make it the foundation of calculation.

"If", said Mayer,

> we apply the theorems which have been established to the relations between heat and volume of the various gases, we find that the depression of a mercury column which compresses a gas is equal (sic) to the quantity of heat liberated by the compression, and hence it results — if we put the ratio of the capacities of atmospheric air under the same pressure and volume equal to 1.421 — that the descent of a weight from a height of about 365 m corresponds to the heating of an equal weight of water from 0° to 1 °C.

The manner of calculation is therefore as follows. Imagine a cubic meter of air enclosed in a cube with five fixed sides and one side at the top movable upwards. On the top side weighs the pressure of the air, which can be represented by the weight of mercury column of one square meter cross-section and 0.76 m height. If the air is heated from 0° to 1 °C, the top side is raised by 1/273 of a meter, and, since the weight on this side is

$$0.76 \times 1000 \times 13.596 \text{ kg},$$

this corresponds to a work of 37.85 kg/m.

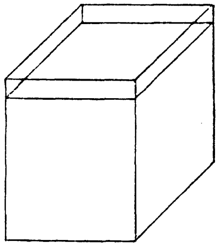


Fig. 78.

If the same air is heated from 0° to 1 °C in a cube with six fixed sides, the work spoken of comes to nothing; but, in this case, we need a smaller quantity of heat. The excess expenditure of heat in the first case is $1.2932 \times (C - c)$ in kilogram-calories, that is to say, the mass of a cubic meter of air multiplied by the difference of the two specific heats. C is 0.23750 and $C/c = 1.410$; consequently $c = 0.16844$ and $C - c = 0.06906$. Thus the quantity of heat in question, in kilogram-calories, is 0.8931.

The number of kilogram-meters divided by the number of kilogram-calories gives the mechanical equivalent of the unit of heat; that is to say the number of kilogram-meters which are equivalent to a kilogram-calorie is 423.8. Mayer only obtained 365 with the inaccurate numbers then at his disposal; somewhat later, Holtzmann used the same method, and concluded that the value lies between the limits 343 and 429.

7. We find Mayer's ideas greatly developed and generalized, though with preservation of their type, in his publication of 1845: *Die organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel: ein Beitrag zur Naturkunde.*

"*Ex nihilo nil fit*". An object which, when expended generates motion, was called by Mayer a "force". Force as a cause of motion was said to be "an object which cannot be destroyed . . . What chemistry has to achieve with respect to matter, that physics has to achieve with respect to force. The sole problem of physics is to learn to recognize force in its various forms and to investigate the conditions of its metamorphoses; for the creation or the destruction of a force lies outside the region of human thought and action."

The raising of a weight was conceived as a force (force of falling), as also was heat. The coals under the boiler of a steam engine give less heat to the outer world when the engine is working.[20] Gay-Lussac's experiment was discussed in a perfectly clear manner, and the heat of expansion of a gas was not ascribed to increase of volume but to work.[21] "Motion", said Mayer, "is latent heat". A gun loaded with shot is heated, with the same charge of powder, less than one which is loaded blank. We obtain electricity by the expenditure of a mechanical effect.

Spatial difference is force, chemical difference is force, and so on. "*In all physical and chemical processes, the given force remains a constant magnitude*". Then followed this schema for giving a survey of the various "forms of force";

I.		Force of falling.	Mechanical forces.
II.		Motion:	Mechanical effect.
	Imponderables	A. Simple,	
		B. Undulating	
III.		Heat.	
IV.		Magnetism.	
		Electricity, galvanic current.	
V.		Chemical dissociation of certain materials.	Chemical forces.
		Chemical combination of certain other materials.	

The known physical facts were arranged in this schema.

The sun was described as the source of life and motion on earth. The heat of the sun is stored up by plants as chemical forces which, when they are utilised by animals, produce the most various effects.

8. In Mayer's *Beiträge zur Dynamik des Himmels in populärer Darstellung* which appeared in 1848, the source of the force which gives rise to the huge yearly output of heat from the sun was investigated. Here he argued that the sun must have cooled long ago if it simply gave rise to the radiation as a glowing body. The consumption of the mass of the sun — conceived as fuel — would be far from sufficient for this radiation, and still less would the *vis viva* of the rotation of the sun. But the fall of meteorites into the sun was regarded as a sufficient source of force. Considerations in the influence of the cooling and of the phenomenon of the tide on the velocity of rotation of the earth fitted into these considerations. The phenomenon of the tides seems to be the

only one on earth whose source of force does not lie in the sun but in the moon.

Investigations like those of Mayer about organic life, the different sources of work, and the source of the sun's heat were taken in hand and carried out in a very similar way at a later date (1852–1854) by such an eminent physicist as William Thomson.[22] By this is best shown the value of certain foolish attacks on the significance of Mayer for physics.

If we take a survey of the achievements of Mayer, we must conclude that hardly any other natural scientist ever took a more important and comprehensive view, and this was the case even though Mayer was not very deeply learned in physics. Further, if we consider how slowly and gradually Mayer assimilated the elementary knowledge of physics, and how he never really quite mastered these things, how, for this reason, he could never see that the conceptions introduced by him as new had long been familiar under other names, and that his new views could quite well have been organically connected with the knowledge which had already been acquired,[23] we might almost cry out; what genius is possible without remarkable talent!

9. Quite another picture is given us by Helmholtz's work *Ueber die Erhaltung der Kraft* of 1847.[24] We cannot doubt that this work of Helmholtz's must, like that of Mayer's have been inspired by a comprehensive view of nature, and yet everything was there connected with scientific data, and everything appears natural and almost as a self-evident completion of what already known. What first strikes us about Helmholtz's work is the professional skill in the working out of details. This alone would have been a great merit even if Mayer's works had been generally known and recognized at the time, for Helmholtz offered just that which Mayer could not offer. And yet people would not understand even this exposition, as Helmholtz himself has related.[25] From this we see clearly enough how little a suggestion signifies if the force of one's own work and one's own need for clearness is not present. The suggestion was then in the air for *all*; but only in a few cases did it fall on fruitful ground.

10. Helmholtz's exposition must be admitted to be a masterly one even by those who are not quite of his way of thinking. We can, said Helmholtz, start either from the theorem of the excluded *perpetuum*

mobile or from the supposition that all physical phenomena are to be ascribed to central forces. These forces may be regarded as the ultimate invariable causes of the phenomena. If these forces suffice for a complete explanation and if no other explanation is possible this supposition has objective truth.

The object of Helmholtz's work is "to carry through all branches of physics" the law of the excluded *perpetuum mobile* in the same way that Carnot and Clapeyron had used it. In systems of material points which quite generally follow the law of the conservation of *vis viva*, the forces of the simple points are central forces. In such a system the increment of *vis viva* is always equal to the work performed by the central forces. The work which is at our disposal (force of tension – "*Spannkraft*" as Helmholtz called it) is therefore always decreased by just so much as the *vis viva* increases, and inversely. Thus in such a system the sum of the force of tension and *vis viva* always remains constant, and this is the "law of the conservation of force". At the present time we call, after Rankine, the force of tension "potential energy" and the *vis viva* "kinetic energy", and the whole law the "law of the conservation of energy". The principle of virtual velocities appears as a special application of this law.

The applications of the law in mechanics were only mentioned briefly, as they were already known.

The "force equivalent of heat" was treated at greater length. Joule's experiments for the determination of the mechanical equivalent of heat were referred to; and the fact was mentioned, that we can charge a battery of Leyden jars by means of mechanical work with the electrophorus, and generate heat by its discharge. Heat can also vanish and generate work by its vanishing, as appears from an experiment of Joule's which is analogous to that of Gay-Lussac. Holtzmann's calculation of the equivalent of heat[26] by the method used by Mayer was compared with the results of Joule's experiments, and Helmholtz remarked that Holtzmann's calculation is only permissible if the specific heat of the gas is independent of the volume (which actually follows from Joule's overflow experiment). Holtzmann's formula agrees for gases with that of Clapeyron; but, according to Helmholtz, it gives at the same time Carnot's function (C). We have

$$1/C = a/k(1 + at),$$

where k is a constant.

"The force equivalent of electrical processes" appears either by electric charges moving with their conductors and consequently performing work, or by discharge and development of heat. This development of heat was found, for the discharge of a jar, from the electrical work performed, by the theory of potential, and was found to be, in agreement with the experiments of Riess, proportional to the square of the charge and inversely proportional to the capacity.

With respect of galvanic electricity, Helmholtz showed that a *perpetuum mobile* would be possible if only one conductor of the second order, which was not electrolyzed by the conduction, existed.

The law of the conservation of force requires that the total heat developed by the stationary current of a battery — the heat developed by the current being added — is equal to the heat developable by the preceding chemical processes. According to Lenz's law, the heat developed by the current is proportional to $J^2 Wt$, where J is the intensity of the current, W the resistance, and t the time; and, by Ohm's law, it is also proportional to EJt, where E is the electromotive force. But since the chemical displacement is proportional to J, E must be proportional to the heat of displacement (per unit of current).

The source of force of thermo-electric currents was found in Peltier's process, and it was deduced that with equal intensity of current the heat developing and binding force rises with the temperature in the same measure as the electromotive force.

Especially remarkable is the analysis of magneto-induction. In a battery of electromotive force E and current J, the chemical heat $EJdt$ or the work $aEJdt$, where a is the mechanical equivalent, is developed in the unit of time. The work developed in conduction is $aJ^2 Wdt$. If at the same time a magnet is also moved by the current, whose variation of potential with respect to the conductor through which unit current flows is $(dV/dt)dt$, then the work transferred to the magnet is $J(dV/dt)dt$ and, by the law of the conservation of force, the equation

$$aEJdt = aJ^2 Wdt + J(dV/dt)\,dt,$$

or

$$J = \frac{E - (1/a)(dV/dt)}{W},$$

subsists; that is to say, the electro-motive force in the circuit appears diminished by $(1/a)(dV/dt)$, or this expression represents the electromotive force induced by the magnet.

The processes which take place in plants and animals were discussed in an analogous way.

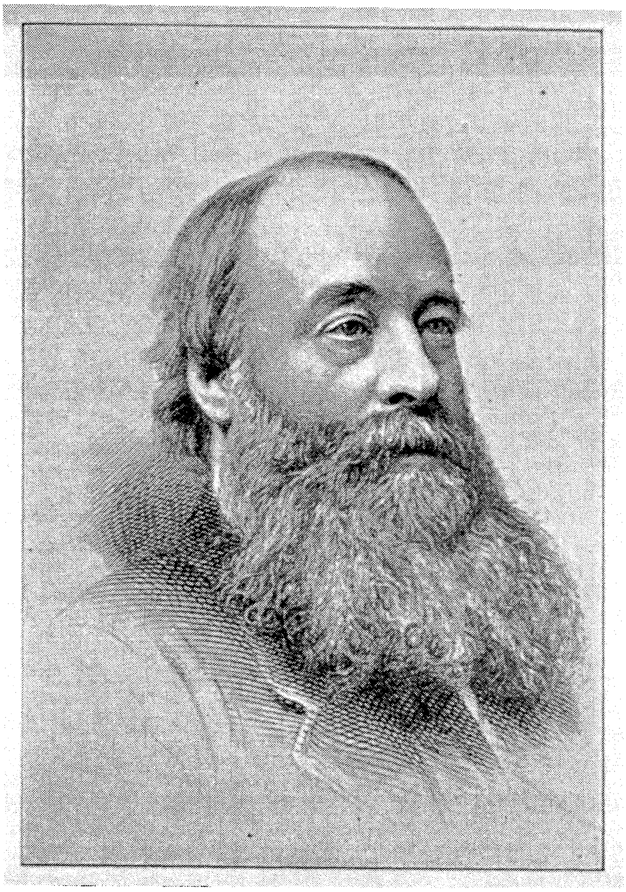
"The aim of this investigation", said Helmholtz,

> which I hope may excuse its hypothetical parts, is to put before physicists, in as complete a way as possible, the theoretical, practical, and heuristic importance of this law, whose complete verification must be considered as one of the principal problems of physics in the immediate future.

In this place we can only mention the most important of the extremely rich contents of the memoir.

It may surprise many people that both Mayer and Helmholtz used the name "force" for the conception of work. We have already shown how this use arose in Mayer's case. Probably Helmholtz adapted his terminology to the name "*puissance motrice*" used by Carnot and Clapeyron. Joule too denoted the conception of work as "mechanical power". We see then that the objection of an unsuitable terminology, which has strangely enough been urged exclusively against Mayer, must be brought against the other investigations of that time. Clausius was the first (1850) to speak of "work" ("*Arbeit*"), and Thomson used in 1851 the expression "the work done". The name "work", introduced by Poncelet in 1826, then seems not to have come into general use. The name "energy" was, indeed, used in pure mechanics by Thomas Young in 1807, but it was first transferred gradually to the whole domain of physics by the English physicists after 1850.

11. The works hitherto considered in detail were exclusively of a theoretical nature. We will now turn to the experimental researches of James Prescott Joule. From 1840 Joule had been busied with the determination of the laws according to which a conductor is heated by a galvanic current.[27] He found that the quantity of heat evolved is proportional to the resistance and to the square of the intensity of the current both in the case of metals and in that of electrolytes; and gave the amount of heat evolved in calories when the units of resistance and intensity of current were arbitrarily chosen. Further he found that the total amount of heat evolved in the circuit is equal to the heat of displacement of the chemical process which goes on simultaneously in the galvanic battery. A pound of zinc develops 1320 °F in a pound of water in Daniell's battery, but 2200° in Grove's battery. Of this heat a part will, as Joule said, become latent by the interposition in the circuit



J. P. Joule

of an apparatus for the decomposition of water, and this part can again be obtained by the explosion of the gases produced.

Pushing these investigations further, Joule[28] determined the heating action of the induction currents which are generated in a small electromagnet enclosed, coil and all, in a glass vessel containing water and revolving between the poles of a powerful electromagnet. He had no doubt that such currents would act according to the same laws as any other currents, but wished to establish the fact decisively by experiment. Indeed, if we consider heat not as a substance but as a state of vibration, we may, in Joule's opinion, expect that heat will be induced by the simple mechanical revolution of a coil before the poles of a magnet. But it still seemed to him doubtful whether heat is really generated or merely transferred from the coils in which magneto-electricity is induced. The latter view did not seem to be untenable, considering that he had proved that the total amount of heat evolved by the voltaic battery is definite for the chemical changes taking place at the same time. Indeed, it might seem that, in the voltaic circuit as in Peltier's experiment, "arrangement" only, and not generation, of heat takes place. A wish to clear up this uncertainly drove him to experiment.

The small compound electromagnet was (Fig. 79) immersed in a glass vessel filled with water and protected from radiation and convection of heat, and rotated between the poles of first powerful electromagnets and then steel magnets, while the conducting wires led to a mercury commutator and thence to a galvanometer. Rotation for a

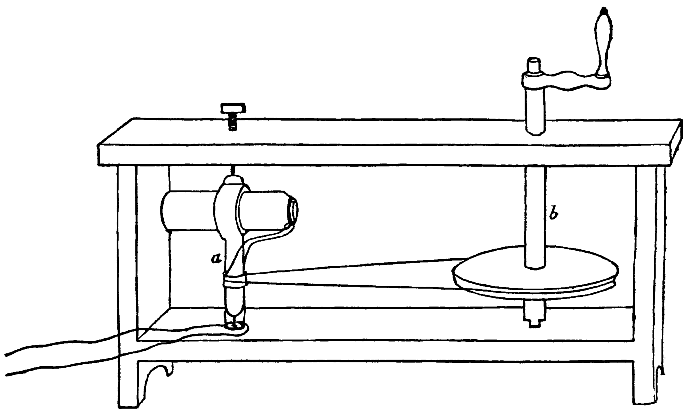


Fig. 79.

quarter of an hour was repeated to determine the heating influence of the neighborhood when the magnet was withdrawn. The terminal wires of the revolving electromagnet were connected with one another when the galvanometer was withdrawn, and then the intensity of current increased correspondingly to the diminished resistance. The stationary electromagnet was excited by currents of different intensities. From all these experiments resulted that, with induction currents as well as voltaic ones, the quantity of heat evolved is proportional to the resistance and to the square of the intensity of the current. The variation of temperature was determined by a sensitive thermometer introduced into the glass tube before and after the experiment, and the quantity of heat was obtained by the determination of the water value of the rotating tube.

The experiments were repeated when a current from a battery was sent through both the rotating coil and the galvanometer, and the current was thus weakened or strengthened by the induction current according to the direction of rotation. In the first case the apparatus worked as an electro-motor and performed work; in the second case it worked as an induction machine and consumed work.

In this case again was verified the law which holds, with respect to the development of heat, for ordinary galvanic currents. Now the chemical displacement is proportional to the intensity of the current, but the development of heat is proportional to the square of the intensity of the current; consequently the development of heat corresponding to a definite chemical displacement is subject to a decrease or increase by the induction current which is proportional to the intensity of these currents. As Joule[29] said:

> Now the increase or diminution of the chemical effects occurring in the battery during a given time is proportional to the magnetoelectrical effect, and the heat evolved is always proportional to the square of the current; therefore the heat due to a given chemical action is subject to an increase or to a diminution directly proportional to the intensity of the magnetoelectricity assisting or opposing the voltaic current. *We have therefore in magnetoelectricity an agent capable by simple mechanical means of destroying or generating heat.*

The Joule found in induction a *mechanical* means of destroying or generating heat.

It is proper to consider this important point still more closely. If e is the chemical heat per unit of time for unit of current, and if we choose the units in such a way that the heat generated by the current J with resistance w in the unit of time can be expressed by wJ^2, the equation

$$eJ = wJ^2$$

follows for the stationary current which must make the whole heat developed coincide with the heat of displacement of the battery. Now this is only possible for one special value of J, namely $J = e/w$ or $Jw = e$, which corresponds to Ohm's law. If, now, the induction current is i, the equation

$$e(J \pm i) = w(J \pm i)^2$$

cannot subsist at the same time as the above one. But if we put

$$E(J \pm i) = w(J \pm i)^2,$$

or

$$E = wJ \pm wi = e \pm wi,$$

we see that, for the new case, the chemical heat per unit of current and unit of time, namely E, is increased or diminished with respect to e by an amount wi proportional to i. We notice that this consideration is allied to that which Helmholtz set up in 1847 on induction.

Since we can generate or annihilate heat by induction, it appeared to Joule of the greatest interest to inquire whether there is a constant ratio between the quantity of heat gained or lost and the "mechanical power" lost or gained. For this object, the experiments described needed only to be varied in so far as the apparatus for rotation was driven by sinking weights with the same velocity as it was before driven by hand. Evidently Joule had to determine the weight q which generated that velocity and merely overcame friction when induction currents were not developed, and also the weight p which generated the same velocity when induction currents were developed. If, then, h is the space fallen through, $(p - q)h$ is the work expended on the development of the induction currents or the heat. Joule found that an expenditure of work measured by 838 foot-pounds is necessary for the heating of one pound of water by one degree Fahrenheit.

At the end of his memoir, Joule mentioned some experiments in which water was forced by pressure through narrow tubes and was thereby heated. These experiments gave the number 770 as mechanical equivalent in the same units. "I shall", said Joule,[30] "lose no time in repeating and extending these experiments, being satisfied that the grand agents of nature are, by the Creator's fiat, *indestructible*; and that wherever mechanical force is expended, an exact equivalent of heat is *always* obtained".

12. In a popular lecture of charming simplicity and clearness, delivered at Manchester in 1847, Joule said[31]:

> we might reason, *a priori*, that such absolute destruction of living force ($mv^2/2$) cannot possibly take place, because it is manifestly absurd to suppose that the powers with which God has endowed matter can be destroyed any more than that they can be created by man's agency; but we are not left with this argument alone, decisive as it must be to every unprejudiced mind.

Then experiment was referred to.

As, we see, Joule was, at the very beginning of his work, in full possession, so to speak, of the principle of the conservation of energy. For, although this principle was not expressly stated, yet it was widely used by him in order to follow up all conceivable transformations of energy — of chemical energy into electrical, mechanical, or thermal energy, and inversely. However, Joule's philosophical views seem, when we get a sight of them, to rest, as far as their expression goes, on no better grounds than the initial propositions of Mayer which have been so sharply criticized. However, in justice to Joule, we must add that he would hardly have agreed to the proposition that the question as to whether the conservation of energy can actually be deduced from the properties of God or not should be decided by, say, an Ecclesiastical Synod. The actual source of his conviction was certainly other than theological.

At bottom, the path by which Joule reached his discovery is very like that which Mayer followed. Mayer set out from the heat of combustion of the animal body, Joule from the heat of chemical displacement of a galvanic battery. In both cases it appeared that the sum of all the effects is connected with a determined material expenditure. Hence a *substantial* view of all these effects lay ready to hand. When at any time the sum did not agree with that at other times, the source of the increment or diminution was investigated, and this source was found in mechanical work. Joule viewed mechanical work as of the same substance as heat so much the more easily as studies on electromagnetic motors, which were carried out from 1838 to 1841, had convinced him that their performance of work is connected with a material expenditure — the consumption of zinc in the battery —, and is proportional to this expenditure.

Thus it was not a metaphysical conviction, but the need of good management of resources and of a calculation which is easily surveyed, which Joule, living amongst engineers, experienced in daily life and in technology[32] brought with him into the domain of science. He felt, so to speak, gratified to find God's world such that he could satisfy this need. Thus, all was exactly as it was in the case of Mayer, with the exception of one characteristically English trait. The sound method of natural investigation is almost innate in Englishmen, and they are certainly brought up to it. The Englishman is never surrounded with metaphysical clouds, — at least he never considers them as the chief thing. Every view is for him an occasion for testing by experiment, and inversely every experiment has an influence on his view. This uninterrupted mutual adaptation of theory and experience can be followed excellently in Joule's work.

13. The memoir just mentioned was followed by many others, all of which had the object of determining as accurately as possible the mechanical equivalent of heat. Here only the most important of these memoirs will be mentioned; in the first place, we will refer to that of 1845.[33] In this memoir, the mechanical equivalent was determined by the simultaneous determination of the mechanical work necessary for the compression of air and of the quantity of heat generated thereby.

In a water calorimeter protected from conduction of heat, a condensing pump C and a copper receiver R were immersed (Fig. 80). Air dried by being passed through the vessel G full of small pieces of chloride of calcium and sucked through a spiral tube immersed in a water bath W of known temperature, and compressed in R. The quantity of water in the calorimeter was chosen so great that the final elevation of temperature was only small, and thus Joule could suppose that the pressure increased according to Mariotte's law; whence resulted a simple calculation of the work of compression. If we imagine the whole volume of air which is to be compressed (v_1) to be contained in a cylinder of any cross-section under the pressure p_1, and compressed — the heat generated being conducted away — to v_2, and if $p_2 = p_1 v_1 / v_2$, then the work is represented by the quadrature of the equilateral hyperbola shown in Figure 81. The quantity of heat generated is the water value of the calorimeter multiplied by the elevation of tem-

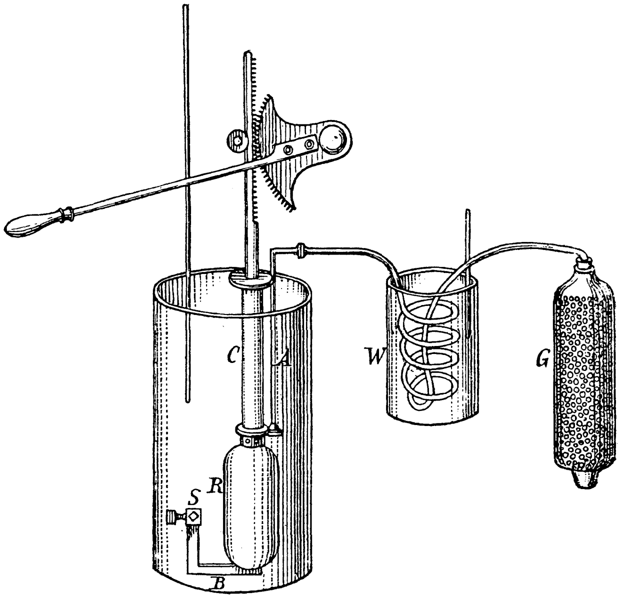


Fig. 80.

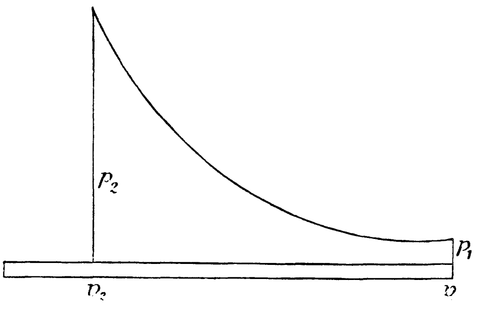


Fig. 81.

perature. In order to eliminate the heat generated by the friction of the piston, the air pipe A was closed, and the pump was worked at the same velocity and for the same time as before. But since the piston, on actual pumping, is pressed with constantly increasing pressure against the walls of the cylinder, the pump was set in motion even without a valve when the receiver was full. The mechanical equivalent was found from these experiments, to be, in the well-known foot-pound-Fahrenheit units, 795.

The close agreement between the new number and that (838) which followed from the electromagnetic experiments, in which experiments there can be no question of heat becoming latent, made it seem probable to Joule that even variations of density of the air in themselves make no heat free or latent, that is to say, the capacity of a mass of air for heat is independent of the volume. By following out this thought and by the intention to submit it to the test of experiment he gave new life to the almost dead Gay-Lussac's experiment.

From a receiver R (Fig. 82), in which dry air is condensed to twenty-two atmospheres, Joule let the air overflow into a vacuous receiver, both receivers being in the same calorimeter. The water, when stirred, showed no variation of temperature. Thus there is no variation of temperature when air expands without performing work.

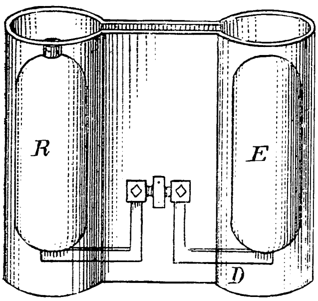


Fig. 82.

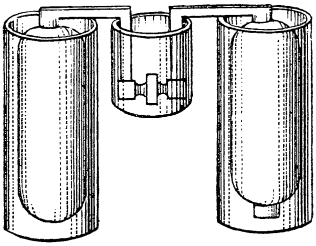


Fig. 83.

When the experiment was repeated with R in one calorimeter and E in another (Fig. 83), a cooling of 2.36° was shown in the former after the overflow, and a warming of 2.38° in the latter. Paying attention to the necessary corrections, we must consider the generation of cold on the one hand equal to the generation of heat on the other.

If we let compressed air stream out of a receiver which is immersed, together with a spiral tube, in a calorimeter, and we measure the volume of the air escaping and overcoming the pressure of the atmosphere, we can also calculate the work of expansion and determine the quantity of heat taken from the calorimeter. For the equivalent, the number 820 resulted. On the basis of the experiments, Joule questioned Carnot's theory.

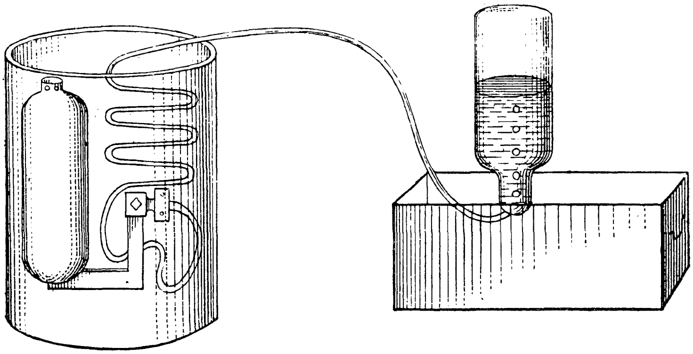


Fig. 84.

14. Those experiments of Joule's which became the best known and which, on this account, we only mention, refer to the generation of heat by friction in liquids (water and mercury). These experiments began in 1845[34] and attained a very complete form in 1849.[35] The fluid content of a calorimeter of total water value m was driven by a paddle wheel between sets of stationary vanes, and the elevation of temperature u was observed. The wheel was driven by a weight P which fell through the height h. Let the work expended in kilogram-meters be Ph, the heat generated is, in kilogram-calories, mu. The quotient Ph/mu then gives the mechanical equivalent in the units now usual. Joule gave the number 423.55 as the best which followed from the friction experiments.

15. The experiments of Hirn[36] must be briefly referred to. Hirn used a freely suspended and hollowed out piece of lead of the mass m which lies by a stone anvil of the weight Q also freely suspended. A freely suspended iron hammer of the weight P (Fig. 85) which, when at rest, touched the lead, was raised to the height h and was then allowed to impinge on the piece of lead. The hammer then rebounded to the small height h_1, while the anvil rose the height h_2. Thus, from the work Ph of the hammer, we must take away $Ph_1 + h_2$. The temperature of the lead has, suppose, been raised by u; and so if the specific heat is s, the production of heat is msu. The mechanical equivalent is determined by the quotient

$$[P(h - h_1) - Qh_2]/msu.$$

Some historically important experiments, which, however, were not very accurate, and whose analysis would be rather complicated, con-

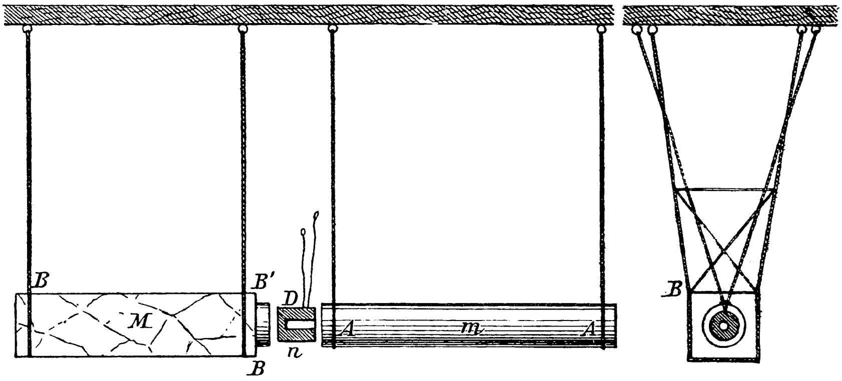


Fig. 85.

sisted in the proof that the working steam in a steam engine loses the equivalent of the work in heat, and, when it has arrived in the condenser, heats the condenser less than if it had been immediately introduced there.

Some especially interesting experiments extending into the domain of physiology rest on the following idea. If a man keeps at rest, he simply gives off a quantity of heat which corresponds to the quantity of oxygen consumed during that time. If the man mounts a hill, he raises his own weight and performs work. The heat equivalent of this work must be lacking in the heat given out. If, finally, the man descends a hill, gravity performs work, and this work appears not as *vis viva* but as heat, and increases the heat given out.

In order to be able to carry out calorimetrically the investigation referred to, Hirn[37] shut up a man in a glass box which served as calorimeter, and in which there was a treadmill which was turned uniformly by a steam engine. The man could either (1) sit still at I, or (2) rise at II as quickly as the steps of the wheel descended, or descend at III as quickly as the steps rose. A pipe brought air for breathing purposes to the man from the outside, and a second pipe carried the air breathed out into a receiver whose contents were accurately analyzed afterwards. The consumption of oxygen could be determined in this manner in all three cases.

In order to determine the heat generated, Hirn waited for the stationary state of the calorimeter in each one of the three cases. In this state, the calorimeter loses as much heat to the neighborhood as is

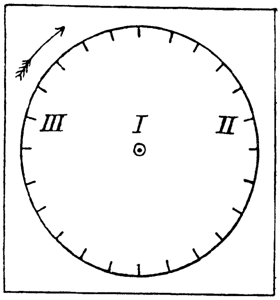


Fig. 86.

generated in it in the same time. If we set up, by way of experiment, the same stationary state by the regulation of a hydrogen burner put in the calorimeter in the place of the man, and of which the consumption of gas can be exactly determined, we obtain the quantity of heat generated in the three cases.

Hirn gave numerical results for the cases I and II. In the case of complete rest, 29.65 g of oxygen per hour were absorbed and 155 calories generated, and therefore 5.22 calories per gram of oxygen. When the man performed work of 27.448 kg/m, he consumed in the same time 131.74 g of oxygen, and should therefore have produced 687.68 calories. But actually, he only generated 251 calories, and therefore spent 436.68 on work and other processes which could not be determined as heat. In the third case, the heat generated rose from six to seven calories per gram of oxygen.

Hirn too was inclined to hold that the principle of energy was self-evident. "Nothing", said he,[38] "comes out of nothing and nothing goes to nothing; such is the basis of the mechanical theory, such is the axiom which I will continually apply from one end of this work to the other . . ."

"If, on the other hand, the body is inelastic, it stops on the plane at once and loses its whole motion of translation. Thus there is in this case an inexplicable, because impossible, annihilation of work".[39]

16. This survey shows it to be indubitable that neither the discovery of the equivalence of heat and work nor the law of the conservation of energy belongs to one nation or to one person. We can rather say that, with the exception of Carnot, whose ideas apparently only came to light *once*, any single one of the investigators who took a great part in this

discovery might have fallen out of account without physics having ceased to proceed in the path of development that was opened up for it. The work of one would have been replaced by that of others.

Without doubt, however, the cooperation of various national and personal individualities has had a very beneficial influence. Owing to it the principle of energy has become accepted quickly and universally. The need of the principle was most strongly expressed by Mayer, and he also laid down its applicability to all domains of physics. To Helmholtz is due the most complete critical elaboration in detail of the principle, and an exposition of the connexion of this principle with our previous knowledge. Finally, Joule introduced the new method and way of thinking into the domain of metrical experiment in a masterly manner.

The enumeration of later eminent achievements in this domain, which fill a very extensive literature, would be beyond the province of this book.

CHAPTER XV

THE DEVELOPMENT OF THERMODYNAMICS. UNIFYING THE PRINCIPLES

1. Carnot had shown in 1824 that if work is to be performed by heat, the heat must sink to a lower level of temperature; Mayer in 1842 and Joule in 1843 had shown that heat vanishes when work is performed; and finally, Carnot, Mayer, Joule, and Helmholtz had, in the period from 1824 to 1847, transferred the principle of the excluded *perpetuum mobile* from the domain of mechanics to the whole domain of physics, and had strongly emphasized its importance. It might now have been expected that the unifying of these principles in such a way that a consistent view of nature could be formed would present no great difficulty, especially as Boltzmann had already attempted such a unification, and Helmholtz had clearly indicated the kind of unification that was necessary. Carnot's function showed itself to be determinable in the most simple way by means of this unification.[1]

But this unification still required a great mental effort. If we put ourselves into the intellectual position of that time, we can see this very well. Does heat, when it performs work, behave like water in a mill, which is still present after it has done its work but at a lower level; or does it behave like coal, which is consumed for heating the working steam-engine? These two views seemed to contradict one another, and they were considered to be irreconcilable. Which view was to be accepted? It could hardly be believed that both of them were valid.

2. This state of the question becomes quite clear to us when we read what such a man as William Thomson[2] said, as late as 1849. Although he recognized the importance of Joule's experiment on the generation of heat by induction currents, he considered it possible that heat was taken from the inducing magnet and transferred to the induced one. Though he was inclined to admit the generation of heat by work, a demonstration of the inverse process seemed to him still to be lacking. And yet for Thomson the maxim of the impossibility of a *perpetuum mobile* was firmly established.

"The extremely important discoveries", said Thomson,[3]

recently made by Mr. Joule of Manchester, that heat is evolved in every part of a closed electric conductor moving in the neighbourhood of a magnet, and that heat is *generated* by friction of fluids in motion, seem to overturn the opinion commonly held that heat cannot be *generated*, but only produced from a source where it has previously existed either in a sensible or in a latent condition.

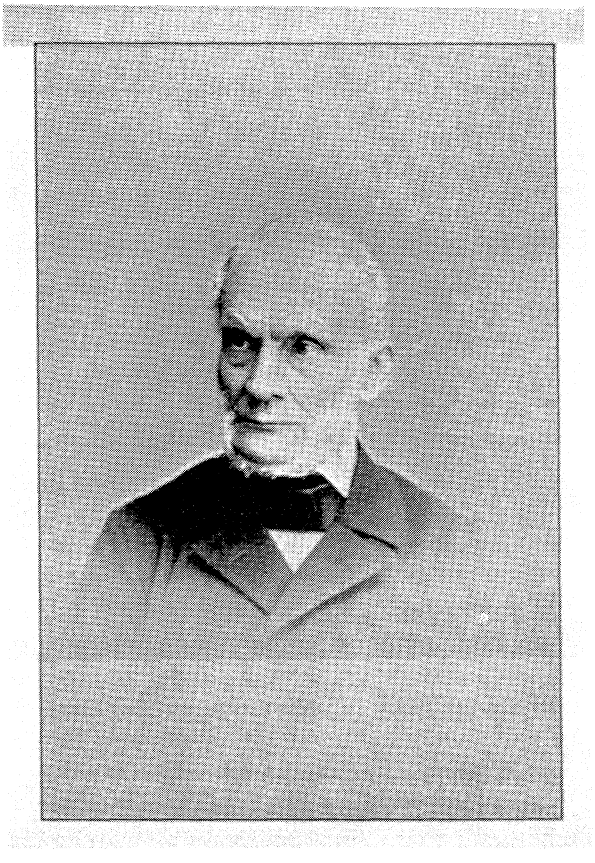
In the present state of science, however, no operation is known by which heat can be absorbed into a body without either elevating its temperature or becoming latent and producing some alteration in its physical condition; and the fundamental axiom adopted by Carnot may be considered as still the most probable basis for an investigation of the motive power of heat; although this, and with it every other branch of the theory of heat, may ultimately require to be reconstructed upon another foundation, when our experimental data are more complete. On this understanding, and to avoid a repetition of doubts, I shall refer to Carnot's fundamental principle, in all that follows, as if its truth were thoroughly established.

Thomson could not reconcile either Joule's or Carnot's view with the principle of the conservation of energy. What becomes of the heat which flows by conduction to a lower temperature without performing work? What effect does this heat generate in place of the work that it might have performed, in view of the fact that no energy can be lost in nature? We shall see that this question, when we bear in mind the principle of energy, is by no means an inapposite one even at the present time.

"When", said Thomson,[4]

'thermal agency' is thus spent in conducting heat through a solid, what becomes of the mechanical effect which it might produce? Nothing can be lost in the operations of nature — no energy can be destroyed. What effect then is produced in place of the mechanical effect which is lost? A perfect theory of heat imperatively demands an answer to this question; yet no answer can be given in the present state of science. A few years ago, a similar confession must have been made with reference to the mechanical effect lost in a fluid set in motion in the interior of a rigid closed vessel and allowed to come to rest by its own internal friction; but in this case the foundation of a solution of the difficulty has been actually found in Mr. Joule's discovery of the generation of heat by the internal friction of a fluid in motion. Encouraged by this example, we may hope that the very perplexing question in the theory of heat, by which we are at present arrested, will, before long, be cleared up.

It might appear that the difficulty would be entirely avoided by abandoning Carnot's fundamental axiom; a view which is strongly urged by Mr. Joule (at the conclusion of his paper 'On the Changes of Temperature produced by the Rarefaction and Condensation of Air'.[5] If we do so, however, we meet with innumerable other difficulties — insuperable without further experimental investigation, and an entire reconstruction of the theory of heat from its foundation. It is in reality to experiment that we must look — either for a verification of Carnot's axiom, and an explanation of the difficulty we have been considering, or for an entirely new basis of the Theory of Heat.



R. Clausius.

The situation at that time can hardly be characterized more distinctly and frankly. The difficulty was not, however, cleared up in the way Thomson expected — by experiment —, but by a careful criticism of the various theoretical points of view. This critical revision is due to Clausius.

3. Clausius[6] first perceived that we can assume, with Carnot, the dependence of the performance of work on the quantity of heat transferred without having to give up the principle of Mayer and Joule of the equivalence of heat and work. It is not necessary to maintain, with Carnot, the invariability of the total quantity of heat. We can suppose without contradiction that, when work is performed by heat, one quantity of heat sinks to a lower level of temperature while another quantity of heat which is equivalent to the work performed vanishes.

Thus if, with Carnot, the work W is merely a function of the quantity of heat Q transferred and of the temperatures t_1 and t_2, and therefore

$$W = F(Q, t_1, t_2),$$

or rather

$$W = QF(t_1, t_2),$$

since, under the same circumstances, if a double quantity of heat is transferred, there will be a corresponding double quantity of work, then, according to Clausius, we also have $W = Q'/A$, where Q' is a quantity of heat which has vanished in proportion to the work performed W, and A is the heat equivalent of the unit of work. We can thus put

$$Q' = AQF(t_1, t_2),$$

or more shortly

$$Q' = QF(t_1, t_2).$$

The equivalence of heat and work was called by Clausius the "first law" of thermodynamics. The equation of Carnot, modified in accordance with this law, which expresses a relation between the heat transferred and the vanished heat — that is, between two different transformations of heat — was called the "second law". This second law says that the ratio of the heat which is transformed into work to the heat transferred from a higher to a lower temperature depends merely on the two temperatures. The same relation subsists on inversion of the cyclic process between the heat generated by work and that transferred from a lower to a higher temperature.

Imagine any body, for example a gas, which, starting from an initial state which is determined eiter by pressure (p) and volume (v) or by pressure and temperature (t) or by volume and temperature, goes through any cyclic process and returns to the same initial state. Certainly the body contains the same quantity of heat at the end of the operation as it did at the beginning. If we imagine the total quantity of heat to be invariable, the algebraic sum of the quantities of heat given to the body and those taken away from the body in the cyclic process would have to be zero, or the sum would have to be determined at every moment of the process by the initial and final values of p and v or p and t or v and t, that is to say, it would have to be a function of them. This view, which Clapeyron and his followers held, was recognized by Clausius as invalid. For, since the quantities of heat which are taken away from, and given to, the body depend respectively on the work performed and expended, and since this work varies greatly according to the kind of process, the algebraic sum of these quantities of heat may be no function that can be given of the independent variables p and v or p and t or v and t. Thus, the questions relating to this were dealt with by Clausius in a way essentially different from that in which they were dealt with by Clapeyron.

If we investigate the relation $Q' = QF(t_1, t_2)$ for a reversible cyclic process carried out with a gas, the same relation holds for any other body if the values of t_1 and t_2 are kept the same. Clausius carried out this investigation on the simple supposition that a gas which varies in volume when the temperature is constant only absorbs or parts with so much heat as is equivalent to the work performed by it or on it. This supposition was not new: Mayer had made it on the grounds of Gay-Lussac's overflow experiment, Joule's experiments had again shown its value, and Helmholtz had distinctly referred to it. But Clausius put into closer connexion with one another the isolated ideas which were already known.

By what we have just said the principal achievement of Clausius is characterized in a general fashion. Although Clausius simply made use of familiar ideas, his critical treatment of them made their interrelations much clearer, and he succeeded in bringing them into a single and consistent system. In view of the situation which preceded this, Clausius's achievement appears as a very important one.

4. It is not our task here to give an exposition in detail of the very extensive researches of Clausius. On the contrary, we have only to consider the fundamental clarification that resulted from them. We obtain the best insight if we begin with his research on gases.

According to the law of Mariotte and Gay-Lussac we have

$$pv = R(\alpha + t),$$

where $\alpha = 273$ and R is a constant for any gas. If the volume of the unit of mass of the gas varies by dv and the temperature by dt, the quantity of heat dQ must be added. By this addition, the inner heat U of the gas is increased and outer work is performed. If A is the heat equivalent of the unit of work, we have, by the first law of thermodynamics

$$dQ = \frac{\partial U}{\partial t}\,dt + \frac{\partial U}{\partial v}\,dv + Ap\,dv. \tag{1}$$

If the variation of volume is neglected, we have the result that v is a constant or that $dv = 0$, and thus

$$\frac{\partial Q}{\partial t} = \frac{\partial U}{\partial t} = c. \tag{2}$$

In Clausius's view, c, the specific heat at constant volume (referred to the unit of mass) is a constant independent of the temperature and volume. This view is based on Regnault's experiments. But from the experiments of Gay-Lussac and Joule follows that no heat is consumed on expansion without work. Consequently we also have $\partial U/\partial v = 0$. Thus the equation takes the form

$$dQ = c\,dt + Ap\,dv. \tag{3}$$

If we put $dp = 0$, or $p = \text{const.}$, in

$$p\,dv + v\,dp = R\,dt, \tag{4}$$

then from (3) we get

$$\partial Q/\partial t = c + AR, \tag{5}$$

where, accordingly, $c + AR$ denotes the specific heat at constant pressure.

If we put $dt = 0$ in (3), we get, for a variation of volume without variation of temperature,

$$\partial Q/\partial v = Ap. \tag{6}$$

We might call Ap "the specific heat of variation of volume at constant temperature". If we put $dt = 0$ in (4), we get $p\,dv = -v\,dp$, and from (3) we then get

$$\partial Q/\partial p = -Av \tag{7}$$

as the specific heat of variation of pressure at constant temperature.

If we substitute in (3) $dt = (p\,dv + v\,dp)/R$, we find

$$dQ = (c/R + A)p\,dv + cv\,dp/R \tag{8}$$

which expresses Q by p and v. For $dp = 0$ we have

$$\partial Q/\partial v = (c/R + A)p \tag{9}$$

for the specific heat of variation of volume at constant pressure.

For $dv = 0$ we find on the other hand

$$\partial Q/\partial p = cv/R \tag{10}$$

as the specific heat of the variation of pressure at constant volume.

These expressions are developed here more fully than is usual, in order that we may be able simply to refer to them.

5. According to the law of Mariotte and Gay-Lussac, we have

$$pv/(\alpha + t) = p_0 v_0/(\alpha + t_0),$$

and consequently the constant R is equal to $p_0 v_0/(\alpha + t_0)$, where p_0, v_0, and t_0 are any corresponding values for the unit of mass of the gas. The value of R is different for different gases, and indeed is inversely proportional to the density of the gas. By what precedes the difference

$$C - c = AR \tag{11}$$

has a different value for every gas, and constant for that gas, which, because A is always invariable, is inversely proportional to the density of the gas. The quotient

$$C/c = (c + AR)/c \tag{12}$$

has also, because c does not depend on the temperature and volume, a different but constant value for every different gas.

If we refer the specific heats to equal volumes of gas at equal pressures and equal temperatures, and call these specific heats Γ and γ for constant pressure and constant volume, we get, by division of the equation

$$C - c = AR = \frac{Ap_0 v_0}{a + t_0} \tag{13}$$

by v_0, the equation

$$\Gamma - \gamma = \frac{Ap_0}{a + t_0} \tag{14}$$

Thus, this difference is, because of the disappearance of v_0, equal for all gases at equal pressure and equal temperature.

If we divide the last equation by γ, we find

$$\frac{\Gamma}{\gamma} - 1 = \frac{Ap_0}{\gamma(a + t_0)}, \tag{15}$$

that is to say, the excess of Γ/γ over 1 is inversely proportional to the specific heat of the gas at constant pressure (per unit of volume), as Dulong[7] has asserted on the basis of experiments already mentioned.

By this, the theorems on gases which were partly derived by Carnot, Clapeyron, Poisson, and others on the basis of not quite valid assumptions, were completed and corrected.

6. If we put $dQ = 0$ in equation (3) and substitute in the same equation for p the value that follows from the law of Mariotte and Gay-Lussac, we have

$$c\,dt + AR(a + t)\,dv/v = 0.$$

This equation corresponds to the variation in temperature of a gas when its volume varies without addition of heat. After the separation of variables we have

$$dt/(a + t) + AR\,dv/cv = 0,$$

of which the integral is

$$(a + t)\,v^{AR/c} = \text{const.}$$

If we put in this integral

$$AR/c = (C - c)/c = C/c - 1 = k - 1,$$

it becomes

$$(\alpha + t)v^{k-1} = \text{const.},$$

or

$$\frac{\alpha + t}{\alpha + t_0} = \left(\frac{v_0}{v}\right)^{k-1}, \tag{16}$$

and, by using the law of Mariotte and Gay-Lussac:

$$\frac{\alpha + t}{\alpha + t_0} = \frac{vp}{v_0 p_0},$$

we also have

$$\left(\frac{\alpha + t}{\alpha + t_0}\right)^{k} = \left(\frac{p}{p_0}\right)^{k-1} \tag{17}$$

and

$$\frac{p}{p_0} = \left(\frac{v_0}{v}\right). \tag{18}$$

The equations (16) to (18) contain the laws deduced by Poisson. For $dt = 0$, the equation (3) is

$$dQ = Ap\,dv$$

or

$$dQ = AR(\alpha + t)\,\mathrm{d}v/v,$$

and its integral is

$$Q = AR(\alpha + t)\log v + \text{const},$$

or

$$Q - Q_0 = AR(\alpha + t)\log(v/v_0). \tag{19}$$

The quantity of heat absorbed or given off on the expansion or compression of v to v_0 at constant temperature depends only on the ratio v/v_0. This is Carnot's theorem with the completion which results from a knowledge of the mechanical equivalent of heat. On the substitution of $p_0 v_0/(\alpha + t_0)$ for R, we also see from the equation

$$Q - Q_0 = Ap_0 v_0 \log (v/v_0) \tag{20}$$

that all gases of equal initial pressure and equal initial volume absorb or give off the same quantity of heat with the same alteration of volume (Dulong). But these quantities of heat are also independent of the temperature and proportional to the initial gaseous pressure.

7. In his first memoir, Clausius's first object was to complete and correct the representation given by Carnot and Clapeyron, and so what he said was much indebted to what they said. According to the older view, the transference of the quantity of heat Q in a cyclic process from A (at the temperature t_1) during the variation of state ab, and

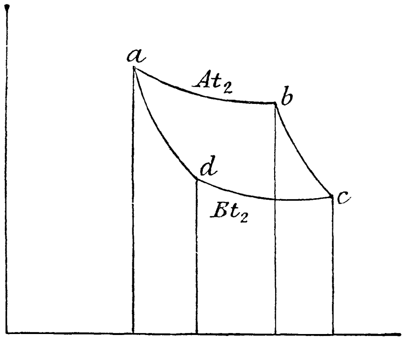


Fig. 87.

completely given off to B (at the temperature t_2) in the variation of state cd, is the one and only equivalent of the work W represented by the area $abcd$. Clausius, however, showed that the quantity of heat $Q + Q'$ taken from A is greater than the quantity Q given to B. While Q is transferred from t_1 to t_2, the other quantity of heat Q', which is equivalent to the work W, vanishes in the cyclic process. For the further quantitative pursuit of these relations, cyclic processes between infinitely near limits were supposed.

The way in which Clausius considered the subject was, on the whole, very like Carnot's. But while Carnot set out from the theorem that work cannot be obtained from nothing, Clausius relied on the theorem that heat cannot be transmitted without expenditure of work from a colder to a hotter body. We see that the two theorems mutally imply each other as soon as we regard the equalization of heat connected with the differences of temperature as a source of work.

The equivalence between heat and work was expressed in equation (1), and this equation can be split up into the two following equations.

$$\partial Q/\partial v = \partial U/\partial v + Ap,$$

$$\partial Q/\partial t = \partial U/\partial t.$$

If we differentiate the first equation partially with respect to t and the second with respect to v, we get

$$\frac{\partial}{\partial t}\left(\frac{\partial Q}{\partial v}\right) = \frac{\partial^2 U}{\partial t\,\partial v} + A\,\frac{\partial p}{\partial t},$$

$$\frac{\partial}{\partial v}\left(\frac{\partial Q}{\partial t}\right) = \frac{\partial^2 U}{\partial v\,\partial t}.$$

If we take the lower equation from the upper one, we get

$$\frac{\partial}{\partial t}\left(\frac{\partial Q}{\partial v}\right) - \frac{\partial}{\partial v}\left(\frac{\partial Q}{\partial t}\right) = A\,\frac{\partial p}{\partial t}. \tag{21}$$

Indeed, as U is a function merely of the state determined by v and t, $\partial^2 U/\partial\, t\, \partial v = \partial^2 U/\partial\, v\, \partial t$, while Q depends also on the outer work $p\, dv$, and therefore on the path of the variation of state. Accordingly, Q is not a function which can be generally given of the two independent variables v and t, and on this account the left-hand side of (21) is not zero.

For a gas the equation (21), if we pay attention to the law of Mariotte and Gay-Lussac, takes the form

$$\frac{\partial}{\partial t}\left(\frac{\partial Q}{\partial v}\right) - \frac{\partial}{\partial v}\left(\frac{\partial Q}{\partial t}\right) = \frac{AR}{v}, \ldots \tag{22}$$

which also expressed the first law of thermodynamics.

This also results from a consideration of Clapeyron's cyclic process between t, $t - dt$, v, $v + dv$, by the aid of Figure 88. In fact, we get, in the well known way, $R\,dt\,dv/v$ for the work performed by the gas. The heat

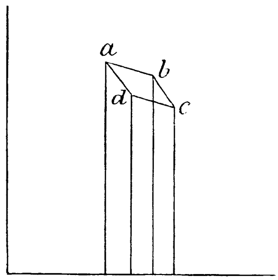


Fig. 88.

introduced on the path ab is $(\partial Q/\partial v)\,dv$. If we develop by Taylor's series the expression for the heat taken away on the path cd, and subtract it from the foregoing expression, we get, for the vanished heat,

$$\left[\frac{\partial}{\partial t}\left(\frac{\partial Q}{\partial v}\right) - \frac{\partial}{\partial v}\left(\frac{\partial Q}{\partial t}\right)\right] dv\,dt.$$

Division of the work performed by the heat which has vanished must give $1/A$, and this gives the equation (22).

8. By considering the same cyclic process we find, quite generally when we take account of the first law of thermodynamics, Carnot's ratio of the work performed to the heat transferred. The work performed is again $(\partial p/\partial t)\,dt\,dv$, and, for a gas in particular, $R\,dt\,dv/v$. The heat absorbed on the path ab is different from that given off on the path cd by an infinitesimal of the second order — the left-hand side of (22) multiplied by $dv\,dt$. Thus we can express the heat transferred simply by $(\partial Q/\partial v)\,dv$, and, for a gas in particular, by $Ap\,dv = AR(\alpha + t)\,dv/v$. But division of the work performed by the heat transferred gives dt/C. Hence

$$dt/A(\alpha + t) = dt/C.$$

Accordingly the function denoted by Clapeyron as "Carnot's function" C has quite generally the value

$$C = A(\alpha + t). \tag{23}$$

Clapeyron's expression

$$\frac{R\,dt}{v(\partial Q/\partial v) - p(\partial Q/\partial p)} = \frac{dt}{C}$$

at once gives the same result if we bring into account at the same time the law of the equivalence of heat and work and carry out the calculation indicated. In this we have to borrow the value $\partial Q/\partial v$ from equation (9) and the value of $\partial Q/\partial p$ from equation (10).

If we imagine the cyclic process to be carried out with saturated vapor, we get, according to Clausius,

$$(s - \sigma)\ (\partial p/\partial t)\,dt/r = dt/C,$$

where s is the volume of unit weight of the saturated vapor at t, σ is that of the unit weight of fluid, and r is the latent heat of vaporization. The derivation corresponds to Clapeyron's method.

By the comparison of Clausius's value of C with the determinations of Clapeyron (s) and with those which Thomson, in the work cited above, carried out on the basis of Regnault's measurements of r and $\partial p/\partial t$ for steam, Clausius arrived at the conclusion that his expression for C is really the correct one. The values of C, indeed, form, for the temperatures indicated, the following series:

Temperature	35.5°	78.8°	100°	156.8°
Clapeyron	1	1.13	1.22	1.37
Thomson	1	1.12	1.17	1.31
Clausius	1	1.14	1.21	1.39

9. In the first of his works which we have cited above, and which is dated 1849, William Thomson held Carnot's point of view. But even then he took steps to make Carnot's theory suitable for practical use. Let W be the work which is obtained by the letting down of the quantity of heat Q from $(t + 1)^0$ to t^0 by a cyclic process. The quotient $\mu = W/Q$,

which gives the work for one unit of heat under these circumstances was called "Carnot's coefficient" by Thomson. This coefficient depends merely on t. Thomson then determined μ by Clapeyron's method, with the help of Regnault's numbers, as we have already mentioned, from degree to degree, between 0° and 230°. If, now, a cyclic process is to be carried out between the lower temperature t_0 and the higher temperature t_1, he imagined a very great number of thermodynamic engines intercalated between t_0 and t_1, and every such engine to work only in a very small interval of temperature, so that every one of the engines takes the heat given off by that working in the next higher interval of temperature and gives it to the engine working in the next lower interval of temperature. If μ is determined as a function of the temperature, the work W of the transferred heat Q is given by

$$W = Q\int_{t_0}^{t_1} \mu\, dt.$$

This idea is, as Thomson himself announced in a second memoir, invalid, because each following engine working in the next lower interval of temperature transfers a smaller quantity of heat, since part of the heat which is taken in has vanished on the performance of work. In this second communication[8] the works of Mayer and Joule were cited with full acknowledgement, and it was mentioned that Rankine and Clausius had greatly helped the progress of thermodynamics by giving up the assumption of the invariability of the quantity of heat.

10. Thomson then united the principle of Joule with that of Carnot, and developed theorems which he discovered independently of Clausius, but in respect of which he claimed no priority over Clausius. The foundation of his work is the axiom: "*It is impossible, by means of inanimate material agency, to derive mechanical effect from any portion of matter by cooling it below the temperature of the coldest of the surrounding objects*". To this statement, Thomson added the following note: "If this axiom be denied for all temperature, it would have to be admitted that a self-acting machine might be set to work and produce mechanical effect by cooling the sea or earth, with no limit but the total loss of heat from the earth and sea, or, in reality, from the whole material world". That this axiom and that of Clausius are only different in form was explicitly remarked by Thomson.

The principle of the conservation of energy was illustrated by the following example. Let there be three equal and similar galvanic batteries furnished with equal and similar electrodes; let A_1 and B_1 be the terminals of the electrodes of the first battery, A_2 and B_2 the terminals of the corresponding electrodes of the second, and A_3 and B_3 of the third battery. Let A_1 and B_1 be connected with the extremities of a long fixed wire; let A_2 and B_2 be connected with the poles of an electrolytic apparatus for the decomposition of water; and let A_3 and B_3 be connected with the poles of an electromotor. Then if the length of the wire between A_1 and B_1, and the speed of the engine between A_3 and B_3, be so adjusted that the strength of the current may be the same in the three circuits, there will be more heat given out in any time in the wire between A_1 and B_1 than in the electrolytic apparatus between A_2 and B_2, or the working engine between A_3 and B_3. But if the hydrogen were allowed to burn in the oxygen, within the electrolytic vessel, and the engine to waste all its work without producing any other than thermal effects, the total heat emitted would be precisely the same in each of these two pieces of apparatus as in the wire between A_1 and B_1.

11. Thomson's work is remarkable for its brevity and for the readiness with which it allows of a comprehensive view of it to be taken. The two "fundamental propositions" — Clausius's "laws" — were obtained in the following manner.

Let a body undergo the variation of volume dv and the variation of temperature dt. The quantity of heat which must be added for this purpose is

$$M\,dv + N\,dt,$$

where M and N are functions of v and t. The mechanical equivalent of this heat is, if J denotes Joule's number $(1/A)$,

$$J(M\,dv + N\,dt).$$

With the surface pressure p on the body, the external work $p\,dv$ is also performed. We form the difference of the two expressions, and take the sum with respect to a closed cyclic process. Then, since the work performed must be the same as the mechanical equivalent of the quantity of heat added,

$$\int [(p - JM)\,dv - JN\,dt] = 0.$$

The integrand is therefore completely determined by the values v and t, and is consequently a complete differential of a function of the two independent variables v and t. Consequently the equation

$$\frac{\partial(p - JM)}{\partial t} = \frac{\partial(-JN)}{\partial v},$$

or

$$\frac{\partial M}{\partial t} - \frac{\partial N}{\partial v} = \frac{1}{J}\frac{\partial p}{\partial t}, \tag{24}$$

subsists. This equation, which contains the first law of thermodynamics, is identical with the equation (21) of Clausius.

The second fundamental proposition, in consequence of Clapeyron's considerations, resulted in the form

$$\frac{(\partial p/\partial t)\, dt\, dv}{M\, dv} = \frac{(\partial p/\partial t)\, dt}{M} = \mu\ dt, \tag{25}$$

and since, dt only appears in the equation as a factor,

$$\frac{\partial p/\partial t}{M} = \mu, \tag{26}$$

where the function of Carnot (μ) only depends on the temperature and not on the material.

12. Then followed a very simple and illuminating consideration which resulted from uniting the first and second fundamental propositions of thermodynamics. The numerator on the left-hand side of equation (25) denotes the work performed in a cyclic process between t and $t + dt$, or the mechanical equivalent of the quantity of heat dq which has vanished in this process and which is infinitely small in comparison with the transferred heat $q = M\, dv$. The equation can therefore be written

$$J\, dq/q = \mu\ dt. \tag{27}$$

This equation subsists even when the variations of volume are considerable, provided only that dt remains infinitely small. Thomson then imagined an infinitely great number of thermodynamic engines arranged in a series between the temperatures T and S: the source of the first engine is a given source, and the refrigerator of each intermediate engine is the source of that which follows it in the series, and each one

Fig. 89.

hands on the quantity of heat it receives to the next in the series with an infinitely small loss (dq). Integration of (27) gives

$$\log \frac{H}{R} = \frac{1}{J} \int_T^S \mu \, dt,$$

where H denotes the quantity of heat flowing through the level S, and R that flowing through the level T. The equation

$$R = He^{-\frac{1}{J}\int_T^S \mu \, dt} \tag{28}$$

follows from this; but, since the work performed W is the mechanical equivalent of the heat that has vanished between S and T, we have

$$W = J(H - R)$$

or

$$W = JH\left[1 - e^{-\frac{1}{J}\int_T^S \mu \, dt}\right] \tag{29}$$

If, then, we can determine μ experimentally from degree to degree, we can arrive at the maximum efficiency of a thermodynamic engine which works between any temperatures. We see that we can never have $W = JH$. Yet we approach this limit more and more as the difference of temperature between which the engine works is greater. By the formula (28), the table of the earlier memoir of Thomson's is again applicable.

Here was mentioned the fact that only a part of the mechanical equivalent of heat is transformed into work, but that the remainder is "irrecoverably lost to man, and therefore 'wasted', although not *annihilated*". In this remark lay the germ of investigations which are to be spoken of later.

13. With respect to the value of μ, Thomson[9] announced that Joule, in a letter to him of December 9th, 1848, had expressed the view that μ is inversely proportional to the temperature above the absolute zero, so that

$$\mu = ka/(1 + at).$$

Thomson, since then, had recognized that, corresponding to Joule's principle, we must have $k = J$. Accordingly he wrote

$$\mu = Ja/(1 + at),$$

which agreed with what Clausius said.[10] It was the experiments on the compression of air which suggested this idea to Joule.[11]

If, now, we write, in order to obtain conformity with Clausius's later work,

$$\mu = J/(\alpha + t) = J/T,$$

where T is the absolute temperature, we have

$$-\frac{1}{J}\int_{T_2}^{T_1} \frac{J\,dT}{T} = -\log\frac{T_1}{T_2},$$

where T_2 is substituted for T and T_1 for S. The exponential in (28) then takes the value T_2/T_1, and we get

$$R = HT_2/T_1,$$

or

$$R/T_2 = H/T_1. \tag{30}$$

This completely coincides with the result of Clausius's later work. For the work performed we have

$$W = JH(T_1 - T_2)/T_1 \ldots \tag{31}$$

Though Clausius overtook Thomson in his first memoir, it will soon appear that Thomson was here ahead of Clausius. By this, both investigators showed themselves to be of equal merits in this question.

The remainder of Thomson's memoir was concerned with the derivation, by combination of the equations (24) and (26), of laws on general properties of bodies and in particular on their specific heats. But these laws lie apart from our principal subject.

14. Thomson,[12] in a further memoir, returned to the idea of an absolute scale of temperature. He had suggested that the degrees should be so determined that the unit of heat, when it is transferred from degree to degree in a reversible cyclic process, gives rise to the same work. Now, since Carnot's function is

$$\mu = J/(\alpha + t),$$

we see that μ becomes less for one degree of the air thermometer as t rises. Consequently the degrees, according to the then defined scale, would be greater in comparison with the degrees of the centigrade scale as the temperature is higher. This would be a great inconvenience in the new scale, which had been set up when it was still assumed that the quantity of heat is constant. The knowledge obtained meanwhile and the very look of the equation (30) must have suggested to Thomson another definition of the absolute scale of temperature. He imagined a reversible

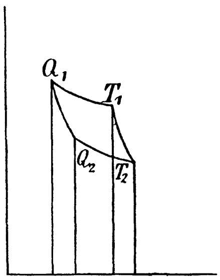


Fig. 90.

cyclic process in which quantities of heat Q_1 and Q_2 can be absorbed or emitted only at two temperatures T_1 and T_2. The temperatures are now so to be numbered that they are proportional to the quantities of heat in question, that is to say that

$$T_1/T_2 = Q_1/Q_2,$$

or that the equation (30) subsists. By this definition, the new scale coincides with the scale of the air thermometer.

If we write the equation (30) in the form

$$Q_1/T_1 - Q_2/T_2 = 0, \tag{32}$$

and imagine a reversible cyclic process of any complexity wished which can be resolved into parts of the form shown in Fig. 90, and which are fundamental to (32),[13] then for every such part an equation analogous to (32) holds. Consequently for the whole process

$$Q_1/T_1 + Q_2/T_2 + Q_3/T_3 + \ldots + Q_n/T_n = 0,$$

where the Q's denote the quantities of heat absorbed or emitted at temperature T and where the summation is to be understood algebraically — the quantities of heat absorbed by the engine being taken as positive and those emitted being taken as negative.

For such a process the first law of thermodynamics may be expressed

$$W + J\Sigma Q = 0, \tag{33}$$

and the second law

$$\sum (Q/T) = 0. \tag{34}$$

15. We will only briefly mention that Thomson[21] had already remarked, in a paper of 1852, the loss of mechanical energy with irreversible cyclic processes. The formulae (30) and (31) permit of the determination of the magnitude of this loss. Since only a part of the energy of heat can be transformed into mechanical energy, and another part of the energy of heat is irrecoverably lost for mechanical energy, mechanical energy continually decreases in quantity. Just as the earth was once upon a time uninhabitable, it will again become uninhabitable. The *real* energy-value of a quantity of heat dq at any temperature is $J\,dq$, but the *practical* value is $J\,dq(T - T_0)/T$ (cf. formula (31)), where T_0 is the lowest temperature at which the heat can be carried over. If we give to the expression the form

$$J\,dq - JT_0\,dq/T,$$

and sum the values of dq for a process in which T continually varies, the total value is

$$J(q_1 - q_0) + JT_0 \int (dq/T).$$

In this q_1 is the total heat absorbed and q_0 the total heat emitted. For a closed reversible process we have $\int (dq/T) = 0$. But with irreversible processes we have an expenditure of mechanical energy to the amount of $JT_0 \int (dq/T)$.[15]

16. In his second communication on the fundamental principles of thermodynamics, Clausius[16] took a freer standpoint. If in a cyclic process heat is "transformed" into work, another quantity of heat sinks to a lower temperature. Heat of higher temperature is "transformed" into heat of lower temperature. Inversely heat can arise by the expenditure of work, and at the same time another qantity of heat is transferred from a lower to a higher temperature. Thus, two mutually dependent kinds of "transformations" occur simultaneously. Thus we can again transform the heat which has passed over from a higher to a lower temperature to heat of the earlier and higher temperature if we substitute for the fall in temperature of the heat another "equivalent" transformation of work into heat. Clausius denoted transformations which proceed "of themselves", that is to say without another compensating transformation being necessary at the same time, as "positive" transformations. Accordingly positive transformations are:

1. Transformation of heat of a higher temperature into heat of a lower temperature,
2. Generation of heat from work.

On the contrary negative transformations are:

3. Transformation of heat of a lower temperature into heat of a higher temperature;
4. Transformation of heat into work.

In a reversible process, both kinds of transformations compensate each other. Accordingly, we may ask how we are to estimate in general the equivalence values of transformations in order that this compensation may happen. This results from the consideration of the simplest cyclic process of Carnot. The equivalence value of the quantity of heat Q' at the temperature t_1, when transformed into work, must be expressed by $-Q'f(t_1)$, and that of the transference of the quantity of heat Q from t_1 to t_2 must be represented by $QF(t_1, t_2)$. If the quantity Q rises from t_2 to t_1, we have

$$QF(t_2, t_1) = -QF(t_1, t_2),$$

or

$$F(t_2, t_1) = -F(t_1, t_2).$$

As we now know, a greater quantity of heat $(Q' + Q)$ is, contrary to the original supposition of Carnot, absorbed in the variation *ab* of state in the cyclic process represented in Figure 91, than is given out (Q) in

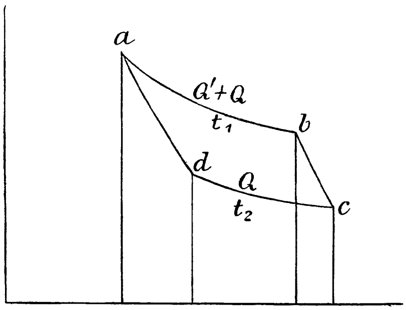


Fig. 91.

the variation of state *cd*. Now, we can regard the heat absorbed in *ab* as transformed into work and that given off in *cd* as arising from work, and then the equation

$$-(Q' + Q)f(t_1) + Qf(t_2) = 0$$

holds.

But we can also suppose that Q' is transformed into work, and that Q is let down from t_1 to t_2. This gives the equation

$$-Q'f(t_1) + QF(t_1, t_2) = 0.$$

If we take the latter equation from the former one, we get, Q vanishing,

$$F(t_1, t_2) = f(t_2) - f(t_1).$$

The transference of the heat Q from t_1 to t_2 has, accordingly, the same equivalence value as the transformation of Q into work at t_1 and the reverse transformation into heat at t_2. Accordingly, both equivalence values are reduced to one. As we see, a mere attentive consideration of the single variations of state of the cyclic process leads to the same result without any calculation.

If we introduce $1/T_1$ and $1/T_2$ as short for $f(t_1)$ and $f(t_2)$ respectively, the equation for the cyclic process just considered is

$$-\frac{Q'}{T_1} + Q\left(\frac{1}{T_2} - \frac{1}{T_1}\right) = 0. \qquad (35)$$

In order to be able to choose the temperature of the transformation of heat into work differently from the temperatures of transference, Clausius carried out the consideration we have just indicated for a much more complicated cyclic process. Imagine (Fig. 92) the process (I) *oabcdeo* carried out; Q at t is transformed into work, Q_1 sinks from t_1

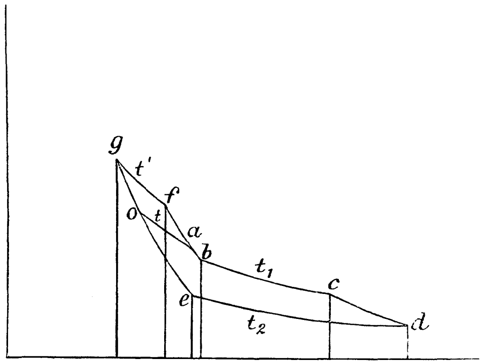


Fig. 92.

to t_2. In process (II) *oedcbfgo,* Q_1 returns from t_2 to t_1, while Q' at t' arises from work. Both processes together correspond to the process *oafg* alone, which is identical with the above discussed more simple one (Fig. 91). To the latter reduces the chief point of Clausius's consideration, which we have described above, so that the whole long way round appears unnecessary.

17. After this, Clausius gave a proof, by introducing the formulae developed in his first memoir, that the temperature function T which had hitherto been left undetermined was the absolute temperature, so that $T = \alpha + t$.

The artificial and timid trait of Clausius's memoir lies in the fact that a result was here obtained with the appearance of freedom from suppositions and by a roundabout way, though the result, most probably, was actually found in quite another way. After the general expression for Carnot's function was determined, there was no difficulty in determining, either by Thomson's method or by another one, the relation of the heat transformed into work to the transferred heat, for a cyclic process taking place between finite differences of temperature. We can hardly suppose that Clausius, in the period 1851—1854, did not attempt this. But from the complete expression, the "equivalence values" can be immediately read off.

A very convenient way of calculation for such a cyclic process, which results immediately from the formulae of his first memoir, was given much later by Clausius.[17] If we denote the quantities of heat absorbed in *ab* (Fig. 93) and given off in *cd* by Q_1 and Q_2 respectively, and the

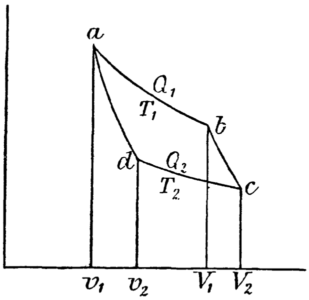


Fig. 93.

volumes and absolute temperatures in the way that we can see from the figure, we get, by (17),

$$T_2/T_1 = (v_1/v_2)^{k-1} = (V_1/V_2)^{k-1}$$

or

$$V_1/v_1 = V_2/v_2 \tag{36}$$

But, by (19), we have

$$Q_1 = RT_1 \log (V_1/v_1)$$

and

$$Q_2 = RT_2 \log (V_2/v_2) \tag{37}$$

From these, paying attention to (36), we have

$$Q_1/Q_2 = T_1/T_2,$$

$$Q_1/T_1 - Q_2/T_2 = 0. \tag{38}$$

This last equation is identical with Thomson's equation (30).

If we divide Q_1 into the transformed heat Q' and the transferred heat $Q(= Q_2)$, we get (35), and from this equation the equivalence values can be read off. The ratio of the heat transformed into work to the transferred heat is

$$Q'/Q = (T_1 - T_2)/T_2. \tag{39}$$

The ratio of the heat transformed into work to all the heat expended, or the economical coefficient, is on the other hand

$$Q'/(Q + Q') = (T_1 - T_2)/T_1. \tag{40}$$

If we are presented with the equation (31) or (40) and if we feel, in conformity with Mayer's way of thinking, the need of conceiving processes in a substantial way, in spite of all transformations, we seek for just that way of estimating the transformed quantities of heat that is adapted to this view. The way of estimation that we seek lies in the equivalence values that are apparent from the equation. This idea is a very beautiful one, and can hardly be lost in this exposition which we have tried to make both honest and free from all that is not essential.

18. Clausius counted, for every quantity of heat Q brought to a source of heat at the temperature T, the equivalence value as $+Q/T$, and, for every quantity Q' taken away at the temperature T', the equivalence value as $-Q'/T'$, and found, for every reversible cyclic process however complicated, the algebraic sum of the equivalence values to be

$$Q_1/T_1 + Q_2/T_2 + Q_3/T_3 + \ldots = \sum Q/T = 0, \tag{41}$$

or when the temperatures vary continuously

$$\int dQ/T = 0. \tag{42}$$

Both these last equations coincide with Thomson's result given in equation (34).

A simple proof of these two equations was given later by Clausius,[18] and in this he followed Zeuner,[19] who, it appears, first used the method described in the following lines.

Imagine any reversible cyclic process only composed of such variations of state as take place either only at constant temperature (isothermally), as in *ab, cd, ef,* and so on, or only in a non-conducting jacket without absorption or emission of heat (adiabatically),[20] as in *bc,* *de*, *fg*, and so on. Such a process can be replaced by the partial processes indicated in Figure 94. Since an equation of the form (38) holds for every one of these partial processes, the equation (41) results of itself for the whole process. But also, if the variations of state do not take place discontinuously but continuously, we can come as near as we wish to any given process by infinitely small isothermal and adiabatic steps. By this the equation (42) can be proved. In essentials, Thomson's derivation of the equation (34) also rests on this principle, though the form of the principle is somewhat different.

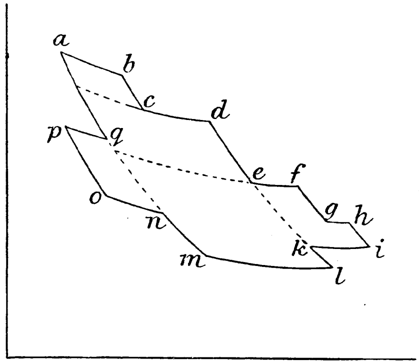


Fig. 94.

If the cyclic process is not reversible, the positive transformations are present in excess, and we therefore have, for every cyclic process, the relation

$$\int (dQ/T) \underset{>}{=} 0, \tag{43}$$

in which the upper or lower sign holds according as the process is reversible or not.

19. If we imagine a reversible process, the temperatures of the bodies which touch one another are always the same. In this case we can regard T as the variable temperature of the body which undergoes the cyclic process. But since for a closed process

$$\int (dQ/T) = 0,$$

the value of the integral in this case is completely determined by the momentary state; that is to say, dQ/T is a complete differential dS of a quantity S which is characteristic of the state of the body. Clausius[21] called S the "entropy" of the body. Two states of a body may differ from one another both in energy[22] and in entropy. As an example, the energy and entropy of a perfect gas may serve. If the temperature rises by dT, the increase of energy is

$$dU = c\,dT,$$

and, by integration,

$$U_1 = U_0 + c(T_1 - T_0), \tag{44}$$

where U_0 is the energy at the initial temperature T_0. Since, further,

$$dQ = cdT + ART\,dv/v,$$

we have

$$dS = dQ/T = c\,dT/T + AR\,dv/v;$$

and therefore

$$S = S_0 + c\log(T_1/T_0) + AR\log(v_1/v_0) \tag{45}$$

is the expression for the entropy of a gas, where S_0 is the initial value which corresponds to the initial temperature T_0 and the initial volume v_0.

A reversible variation of state for which the added or subtracted heat dQ is zero, and therefore also $dS = dQ/T = 0$, does not give rise to any variation of entropy. Such a variation of state was called by Gibbs "isentropic".

20. Maxwell[23] considered entropy as a characteristic of state analogous to temperature. If we wish to compare the entropy S_1 of the unit of mass of a body in any state with the entropy S_0 in a normal state which is chosen at will, we have merely to bring this mass in a reversible way from the first to the second and to determine the corresponding value $\int_0^1 (dQ/T)$. The simplest process would be to bring the mass by an isothermal variation at the arbitrary temperature T from the isentropic curve S_1 to the curve S_0. If for this purpose the quantity of heat Q must be taken away from the mass, we have

$$S_1 = S_0 + Q/T.$$

If we do not know the course of the isentropic curves, it is best to refer the entropy to a definite normal state which is determined by the normal pressure p_0 and the temperature T_0 and which corresponds to the intersection of a definite isothermal line with a definite isentropic curve. For example, let us bring the body isentropically to the normal temperature, and then isothermally to the normal pressure. If the quantity of heat Q is taken away from the body by the latter operation, the entropy was greater by Q/T_0 than in the normal state.

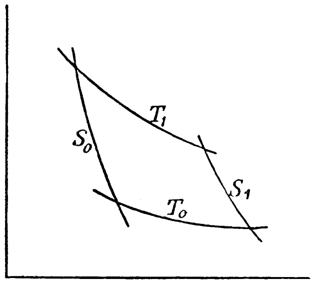


Fig. 95.

As an example, the following case may serve. Suppose that a gas expands without performance of work from v_0 to v_1 by streaming out into empty space. Its temperature remains T_0. For reversible isothermal compression to the original volume v_0 the work $RT_0 \log(v_1/v_0)$ is necessary, by formula (19); and this generates the heat $ART_0 \log(v_1/v_0)$ which, on the isothermal compression, is conducted away. Accordingly the entropy S_1 at the volume v_1 is greater than the entropy S_0 at v_0 and the same temperature. Indeed

$$S_1 = S_0 + AR \log(v_1/v_0),$$

which is also apparent from equation (45).

21. In a former memoir, Clausius[24] had remarked that the transformations denoted as positive are uncompensated and therefore usually in excess. Such transformations are very various. They include the equalization of temperature by conduction and radiation, the generation of heat by friction and electric currents, the expansion just considered of gases under a resistance which is smaller than the force of expansion —

where therefore *vis viva* is generated which ultimately again passes over into heat and so on. In all these cases there is an increase of entropy. If the quantity of heat dq passes over from a body whose temperature T_1 is greater than T_2 to a body of the temperature T_2, the entropy of the former decreases by dq/T_1 while that of the latter increases dq/T_2. The whole entropy increases, and

$$dq\left(\frac{1}{T_2}-\frac{1}{T_1}\right)=dq\,\frac{T-T_2}{T_1T_2}\,.$$

Gas which overflows into empty space without variation of temperature cannot be brought back to its original state without a decrease of entropy: its entropy has therefore increased on the overflowing. Clausius[25] thence concluded that:

1. The energy of the world is constant;
2. The entropy of the world tends to a maximum.

We see that both these theorems and Thomson's theorems on the wasting of mechanical energy are in essentials a quantitative sharpening of Carnot's ideas.

22. Maxwell[26] directed attention to analogies of thermodynamical and mechanical concepts. Work (w), pressure (p), and volume (v) stand in the relation

$$w=p(v'-v);$$

heat (Q), temperature (T), and entropy (S) satisfy the equation

$$Q=T(S'-S).$$

Thus the quantities standing in a vertical row in the following schema can be put in analogy:

$$\begin{array}{lll} w & v & p \\ Q & S & T. \end{array}$$

Of course, since an analogy always contains an arbitrary element, we can find many other analogies. The fullest study of the relations referred to has been made by Arthur von Oettingen.[27]

The conception of entropy as a characteristic of state analogous to temperature appears at first sight strange, because of the roundabout definition of entropy which we have had to give by the equation

$dS = dQ/T$. But if anyone had no sensation of temperature, he might come into the position of defining temperature by $T = dQ/dS$, analogously to a velocity, or by $dT = dp/L$, analogously to entropy, where $L = p_0 v_0 \alpha / v$ would be dependent on the constants of the air-thermometer used.

23. We will merely mention that the theorems of thermodynamics were soon extended to electrical phenomena. William Thomson first published, in 1851, a short communication on thermoelectricity, and then, in 1854, a longer memoir on "Thermoelectric Currents".[28] In the meantime Clausius also busied himself with allied questions. He determined by the methods of the theory of potential the work of the electric forces when a conductor is discharged, and thence deduced the heating of the discharger by the fundamental propositions of thermodynamics.[29] The work (w) performed during the discharge and the quantity of heat which is generated and is proportional to it is given by $W = Q^2/2C$, where Q is the quantity of electricity and C is the capacity. Helmholtz had laid the foundation for this view in 1847.

Clausius[30] further explained by the work performed by the current Joule's law of the heating of a conductor. A most interesting research concerns thermoelectricity. The thermoelectric pile was considered by both Thomson and Clausius[31] as a thermodynamic engine to which Carnot's theorem can be applied. Indeed, since the Seebeck's current which flows through the warmer end where the soldering is, cools this place according to Peltier's law, while the current flowing through the cooler end warms this end, there is, in essentials, a flowing of heat from a higher to a lower temperature, and in this process lies the source of work of the thermoelectric current. On this occasion, Thomson discovered the transportation of heat by an electric current in a homogeneous and unequally heated conductor.

24. Almost simultaneously with Thomson and Clausius, William John Macqnom Rankine[32] took part in the construction of thermodynamics. The motion supposed to constitute heat consists, according to him, in certain molecular vortices. He[33] emphasized the importance of the absolute zero-point of temperature, made many discoveries, and contributed greatly to the terminology of the subject.

Rankine's works are not, however, of such great importance in point of principle as those of Thomson and Clausius. The contributions to thermodynamics of Thomson and Clausius must be regarded as of equal importance. We may even assume that thermodynamics would have followed very nearly the same plan of structure as that just considered if one of the two chief architects of this science had taken no part in its construction. However, with respect to the form of exposition, there is an important distinction between them. Thomson's exposition is always quite frank about the difficulties which he met, the paths followed by him are always the shortest and simplest, his methods always are quite perspicuous, and the motives which guided him in his investigations are evident to everyone. Clausius's exposition on the other hand always bears a trait of ceremoniousness and reserve. We often are in doubt as to whether Clausius was more concerned to tell us of something or to keep something from us. Instead of simple experiences which serve as foundations for his deductions, these deductions are built on specially assumed axioms, which have the appearance of greater reliability without really guaranteeing more than those experiences. Clausius also was addicted to creating new names and conceptions which were not always necessary. But all these personal peculiarities cannot affect our reverence for him as one of the founders of thermodynamics.

CHAPTER XVI

CONCISE DEVELOPMENT OF THE LAWS OF THERMODYNAMICS

1. Now that we have considered the ideas of thermodynamics one by one, and the long roundabout ways in which they have developed historically, it is advisable to glance over the whole path of development briefly in a sort of perspective.

A *reversible* cyclic process gives the maximum of work which can correspond to the transference of a definite quantity of heat from a higher to a lower temperature. This maximum is the same for all kinds of matter, for, if not, a *perpetuum mobile* would be possible. Further, with a given quantity of heat, this maximum depends only on the temperatures. It is thus only necessary to determine this relation for *one* kind of matter. Carnot had arrived at this stage.

We choose, with Carnot, the following cyclic process. We allow a gas

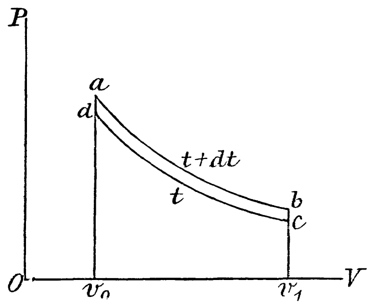


Fig. 96.

of the temperature $t + dt$ to expand isothermally from v_0 to v_1, then cool it by dt, compress it isothermally at t to v_0, and then heat it again by dt. The infinitely small quantities of heat taken away and supplied on the cooling and warming by dt are inconsiderable in comparison with the other quantities of heat. The work W corresponding to the area *abcd*, divided by the heat Q absorbed on the expansion (or emitted on the compression) of the gas, gives the relation sought for all kinds of matter and for the temperatures t and $t + dt$. This was also found by Carnot.

But we have $p = R(\alpha + t)/v$, and consequently

$$W = R(\alpha + t + dt) \int_{v_0}^{v_1} (dv/v) - R(\alpha + t) \int_{v_0}^{v_1} (dv/v) = R\,dt \int_{v_0}^{v_1} (dv/v).$$

If the heat absorbed during the isothermal expansion of the gas is the equivalent of the work performed during this expansion (Mayer, Joule, Clausius, Thomson), we have

$$Q = \frac{R}{J}(\alpha + t) \int_{v_0}^{v_1} \frac{dv}{v},$$

where J is Joule's number for the mechanical equivalent, and the difference of t and $t + dt$ may remain unconsidered. The generally valid relation of Carnot which is sought is therefore, for $t + dt$ and t,

$$W/Q = J\,dt/(\alpha + t),$$

or, referred to the absolute temperature,

$$W/Q = J\,dT/T. \tag{1}$$

If, now, we bear in mind the fact that the work W is also the equivalent of a vanished quantity of heat, namely, of the infinitely small excess dQ of the heat supplied in the process ab above the heat taken away in the process cd (Clausius, Thomson), we can put $W = J\,dQ$, and the equation (1) becomes

$$J\,dQ/Q = J\,dT/T,$$

or

$$dQ/dT = Q/T, \tag{2}$$

from which we see that, when the interval of temperature is extended from T to $T + dT$, and therefore when dT grows, the values of Q and T are always proportional to one another. Integration gives $Q = kT$, where k is any constant. If, thus, we set off the absolute temperatures T_1 and T_2, between which a cyclic process takes place, as abscissae, and the quantities of heat Q_1 and Q_2 absorbed (or given off, as the case may be) at these temperatures as ordinates, then one and the same straight line

(Fig. 97) represents all the factors in the case.[1] The principal equation is

$$Q_1/T_1 = Q_2/T_2, \tag{3}$$

from which Thomson's and Clausius's investigations set out.

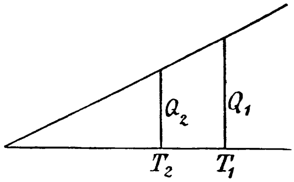


Fig. 97.

2. We can get the equation (3) in another very simple way by starting from the non-dependence of the specific heat (c) of the gas on its volume, and use a cyclic process devised by Clapeyron.

We heat a unit mass of the gas at constant volume v_0 from T_2 to T_1, expand it isothermally to v_1, cool it to T_2 when its volume is v_1, and

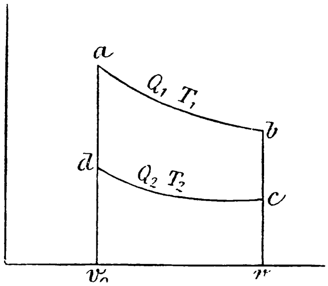


Fig. 98.

compress it isothermally to v_0. The quantities of heat communicated by the heating and taken away on the cooling are the same. But in order to be able to carry out the process in a reversible way, imagine, with Clapeyron, an infinite number of bodies of very great capacity for heat and graduated temperatures to be intercalated between T_2 and T_1. The gas is heated by successive contacts with these bodies, and is cooled without any useless loss of heat by a series of contacts in the reverse order.

The only quantities of heat coming into consideration are Q_1 absorbed in the process ab and Q_2 emitted in the process cd. These quantities behave as the quantities of work in the processes ab and dc, or as the tensions of the gas at the same volume, or as the temperatures T_1 and T_2. Consequently we again get the equation [2]

$$Q_1/Q_2 = T_1/T_2.$$

3. The relation given by equation (3) is evidently not dependent on the *form* of the process but only on the temperatures and the reversibility (Carnot). This relation can therefore be immediately applied to a process with adiabatic variations of state, as represented in Figure 99.

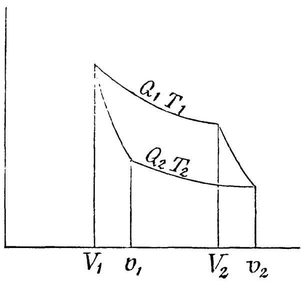


Fig. 99.

But since the equations

$$Q_1 = ART_1 \log (V_2/V_1),$$

$$Q_2 = ART_2 \log (v_2/v_1)$$

then hold, we get, with the help of (3)

$$V_2/V_1 = v_2/v_1,$$

and for the adiabatic variations of state

$$T_1/T_2 = f(V_1/v_1);$$

that is to say, the ratio of the initial and final temperatures in the adiabatic variation is merely a function of the ratio of the initial and final volumes.

4. Vapors are, on account of their more complicated properties, less

suitable to give a view of thermodynamic processes which is clear in point of principle. Indeed, our knowledge of vapors has been greatly helped by thermodynamics. Accordingly, we will here only notice in what way a cyclic process with vapors differs from such a process with gases. If both processes take place between the temperatures T_1 and T_2, where T_1 is greater than T_2, and the heat given to the working bodies at T_1 is Q_1 in both cases, then the heat taken away from the same bodies at T_1 is in both cases $Q_2 = Q_1 T_2/T_1$, and the performance of work is the equivalent of $Q_1 - Q_2 = Q_1(T_1 - T_2)/T_1$. However the difference of the pressures at T_1 and T_2 is much greater for vapors than for gases, and consequently, for the same performance of work, the variation of volume is much less for vapors than for gases. The circumstance that a great part of the quantities of heat Q_1 and Q_2 which are supplied and taken away is "latent" and does not appear as work is, for gases, which are without latent heat, compensated by the fact that in their case much more of the work of expansion is destroyed by the work of compression. On account of the great variations of volume which gases must go through in order to perform any fairly great amount of work, processes with gases have no advantages from a practical and technical point of view, although of course, under ideal suppositions, they are of equal value with processes with vapors.

5. All the facts whose knowledge is essential for the preceding developments had been known for almost a quarter of a century before Sadi Carnot published anything. If, now, when he had assumed the equivalence of heat and work, he had in a moment of inspiration seen through Gay-Lussac's overflow experiment it would have been possible to develop the laws of thermodynamics in a few minutes. Actually the discovery of these laws took thirty years longer.

CHAPTER XVII

THE ABSOLUTE (THERMODYNAMIC) SCALE OF TEMPERATURE

1. It was possible to establish the universally valid thermodynamical relations derived in the two last Chapters on the basis of particular, well-known, and simple properties of gases. In the short derivation given in §1 of Chapter XVI, it appears quite clearly that, besides the axiom of Carnot and Mayer and the law of Mariotte and Gay-Lussac, the fact of experience to which Gay-Lussac closely approached and which was established by Joule is fundamental. According to their experiment, a gas which is expanded only loses the heat equivalent of the external work performed; or, on compression with an expenditure of work, only gains the heat equivalent of this work. We merely express the same fact differently by saying that there is no internal work in the gas on variations of volume, or that its specific heat is independent of the volume. The expression of the theorems is greatly simplified by supposing that the tension of the gas with unvaried volume gives the measure of the temperature. Thomson's absolute (thermodynamic) scale of temperature, according to its *second* definition, which is referred in §14 of Chapter XV, then coincides with Amontons's absolute scale of tension. Of course all this is only exactly correct if the suppositions of the derivation are exactly correct too.

According to William Thomson's conjecture, the internal work on variations of volume of the gas is not wholly absent but is merely very small. By a sensitive method of experiment, which Thomson devised and carried out in cooperation with Joule, this conjecture was verified. On expansion of gases a small amount of internal work is performed, and, on account of this, the gases, even when all external work is absent, are slightly cooled.

Now if the *tensions of gases* are retained as a measure of temperature, the relations derived are not exactly correct. If, on the contrary, we wish to keep the theorems that we have found in their beautifully simple form we must choose a new measure of temperature. The last way was chosen by Thomson.

2. Before we enter into details about the researches of Thomson and

Joule, we will advance a general consideration. Carnot's equation (25) of Chapter XV is

$$\frac{(\partial p/\partial t)\, dv\, dt}{M\, dv} = \mu\, dt. \tag{1}$$

For a gas of the well-known properties, we have for Carnot's function

$$\mu = J/(\alpha + t),$$

which equation is no longer accurate if the gas has not exactly the properties mentioned. We might imagine a perfect gas of the kind referred to, use it for a definition of temperature, and maintain the relation of μ and t. But a real determination of temperature would not be possible in this way. But the following way reaches the desired end.

Imagine a cyclic process with any body between infinitely near limits of temperature, and suppose that Carnot's function μ is experimentally determined for *any* scale of temperature in its whole range. For an elementary cyclic process we then have, as in §12 of Chapter XV,

$$J\, dQ/Q = \mu\, dt.$$

If, now, we define the temperature T arbitrarily as J/μ, by which the old simple relation $\mu = J/T$ is again established, the preceding equation transforms, because, $dT = -J\, d\mu/\mu^2$, into

$$J\, dQ/Q + J\, d\mu/\mu = 0,$$

whose integral is

$$Q_1/Q_2 = \mu/\mu_1,$$

or

$$Q_1/Q_2 = T_1/T_2, \tag{2}$$

where the T's correspond to the new definition of temperature. The new T's almost coincide with those of the gas scale, the deviations from it can be determined by the experimental method that we have indicated, and thus the gas scale will be reduced to the absolute (thermodynamic) scale of the new definition. This is the kernel of Thomson's idea, and we will now enter into some details about it.

3. Even before the investigations he undertook together with Joule, Thomson[1] laid down a general method for the determination of μ for

different temperatures. If we write Carnot's equation (1) in the form

$$M = \frac{1}{\mu}\,\frac{\partial p}{\partial t},$$

where M is the heat absorbed by any body in the element of its isothermal expansion, the heat absorbed for a finite expansion of this nature is, by the use of the same equation,

$$Q = \int_v^{v'} M\,dv = \frac{1}{\mu}\,\frac{\partial}{\partial t}\int_v^{v'} p\,dv. \tag{3}$$

But since the external work is

$$W = \int_v^{v'} p\,dv,$$

We have quite generally

$$Q = \frac{1}{\mu}\,\frac{\partial W}{\partial t}.$$

For a perfect gas, that is to say, one in which

$$pv = p_0 v_0\, \alpha \cdot (a + t),$$

we have in particular

$$W = p_0 v_0\, \alpha \cdot (a + t)\log(v'/v)$$

and

$$\partial W/\partial t = p_0 v_0\, \alpha \cdot \log(v'/v),$$

and consequently

$$W/Q = \mu(a + t).$$

From this follows that:

(*a*) The ratio of the work of compression to the heat obtained (or of the work of expansion to the heat lost) is constant for the same temperature; but that:

(*b*) This ratio is not independent of the temperature except when μ is inversely proportional to the absolute (Amontons's) temperature; and that:

(*c*) This ratio only corresponds to Joule's number J if $\mu = J/(a + t)$.

Thus Thomson was of the opinion that "Mayer's hypothesis", which lies at the basis of his calculation of the equivalent, cannot be agreed to without more experimental evidence, although this hypothesis had shown itself to be very approximately correct within the limits of Joule's investigations.

4. For the exact verification of the hypothesis, Thomson considered that Joule's overflow experiment was not sensitive enough, and devised, after many attempts, the following method. Imagine a very long spiral tube lying in water of constant temperature, and through which air is forced uniformly by means of a forcing pump. The air takes the temperature of the water. At a place of the tube which is well protected from a conduction of heat to it is a plug S (Fig. 100) of wool or silk,

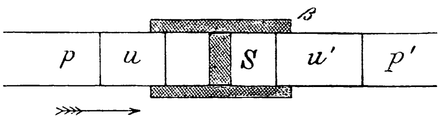


Fig. 100.

through whose pores the air passes uniformly with friction from the pressure p to the lower pressure p'. After this the air continues to flow slowly and uniformly, without perceptible *vis viva.* Immediately behind the plug S in the slow and already uniformly flowing air is a sensitive thermometer. Since here the air is continually replaced by other air which goes through the same process of expansion, even small variations of temperature are easier to determine exactly than in the single overflowing in Joule's experiment. All the occurrences of external work can be determined and compared with the variations of heat which occur, and by this means any internal work which may be present must appear.

If in the unit of time the volume u of air is forced into the tube under the pressure p and, after passing through the plug, expands to u' and comes under the pressure p' the following processes are to be considered. At first the pump expends in the unit of time the work pu on the gas, while the gas flowing away behind the plug again accomplishes the work $p'u'$, so that $pu - p'u'$ is the total work expended on the gas. We may imagine before the plug a piston which, under the pressure p in the unit of time, compresses the volume u, and behind the plug another similar piston which yields before the volume of air u'.

Further, the air on passing through the plug accomplishes in the unit of time the external work $\int_u^{u'} p\,dv$ by its expansion. But since the kinetic energy generated in the pores finally vanishes by friction and so on, the equivalent of heat for this work appears as it did for the work mentioned before, namely

$$\frac{1}{J}\left(\int_u^{u'} p\,dv + pu - p'u'\right).$$

From this we subtract the heat absorbed on the (isothermal) expansion of the gas, and this is, by equation (3),

$$\frac{1}{\mu}\int_u^{u'} \frac{\partial p}{\partial t}\,dv.$$

5. By experiments, now, is found a small cooling of the gas by δ °C. If K is the capacity for heat of the gas which enters through the plug in the unit of time, we have

$$-K\delta = \frac{1}{J}\left(\int_u^{u'} p\,dv + pu - p'u'\right) - \frac{1}{\mu}\int_u^{u'} \frac{\partial p}{\partial t}\,dv,$$

or more shortly,

$$-K\delta = \frac{1}{J}(W + pu - p'u') - \frac{1}{\mu}\,\frac{\partial W}{\partial t}.$$

Thus the heat generated by the external works is not quite sufficient to account for the heat absorbed on expansion of the gas. This indicates a consumption of heat by internal work.[2]

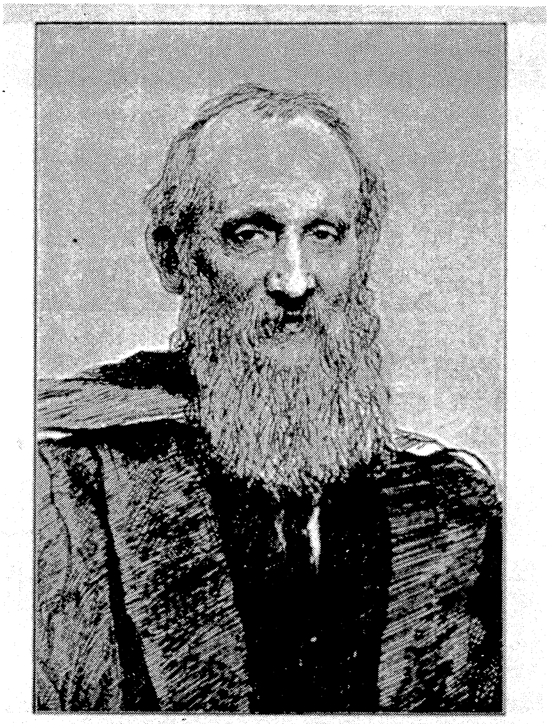
We write the last equation in the form

$$\frac{1}{\mu} = \left[\frac{1}{J}(W + pu - p'u') + K\delta\right] \Big/ \frac{\partial W}{\partial t}. \tag{4}$$

If δ were zero, and if a gas were to behave exactly according to the law of Mariotte and Gay-Lussac we should have $pu = p'u'$ and, by the known expressions for W and $\partial W/\partial t$, we should again have

$$\frac{1}{\mu} = \frac{a + t}{J},$$

which corresponds to the suppositions of earlier chapters.



Lord Kelvin.

If these suppositions are dropped, we can yet determine the value of μ for every temperature of an arbitrary scale by a determination of δ and with the help of Regnault's observations on the deviations of gases. To this the scale of the new definition of temperature can be correspondingly reduced.

6. This was attempted by Thomson and Joule. Since it is not a question of the *absolute* magnitude of the degrees of the new scale for the maintenance of the equation (2), this magnitude can be so chosen that it coincides for 0° and 100° with the centigrade scale and the air thermometer scale. Without going into further details, we may mention that the cooling for air at a pressure of half an atmosphere above the external pressure of the air is, in round numbers, 0.1 °C, for a pressure of an atmosphere and a half, 0.2 °C, in round numbers, and so on. For carbonic acid gas the coolings are greater and for hydrogen they are smaller. For higher temperatures the cooling decreases. Thomson and Joule[3] finally give the following comparisons of the temperatures (T − 273.7) of the new scale and the temperatures (θ) of an air thermometer of constant volume which gives a pressure of 760 mm at 0°:[4]

$T - 273.7$		θ	
0°		0°	
20	20	+	0.0298
40	40	+	0.0403
60	60	+	0.0366
80	80	+	0.0223
100	100	+	0.0000
120	120	−	0.0284
140	140	−	0.0615
160	160	−	0.0983
180	180	−	0.1382
200	200	−	0.1796
220	220	−	0.2232
240	240	−	0.2663
260	260	−	0.3141
280	280	−	0.3610
300	300	−	0.4085

It thus appears that the new temperature scale very nearly coincides with that of the air thermometer. The question as to how accurate the result of the investigation of Joule and Thomson is of not very great importance. Much more important is the giving of the *principle* by which every scale can be reduced to the newly defined scale. By this principle thermodynamics first received clear theoretical finality. By the extension of experimental means, the thermodynamic scale will become more accurate and capable of comparison with any other scale within a greater range than was hitherto possible.[5]

CHAPTER XVIII

CRITICAL REVIEW OF THE DEVELOPMENT OF THERMODYNAMICS. THE SOURCES OF THE PRINCIPLE OF ENERGY [1]

1. In an exceptionally clear and simple popular lecture, which Joule delivered in the year 1847,[2] he explained that the living force which a heavy body has acquired by its descent through a certain height and which the body carries with it in the form of velocity, is the equivalent of the attraction of gravity through the space fallen through, and that it would be "absurd" to assume that this living force could be destroyed without some restitution of that equivalent. He then added: "You will therefore be surprised to hear that until very recently the universal opinion has been that living force could be absolutely are irrevocably destroyed at any one's option." To-day the law of the conservation of energy, wherever science reaches, is accepted by all and receives applications in all domains of natural science.

The fate of all momentous discoveries is similar. On their first appearance, they are regarded by the majority of men as errors, as Mayer, Helmholtz, and even Joule found. Gradually, however, people are led to see that the new view was long prepared for and ready for enunciation, only that a few favoured minds had perceived it much earlier than the rest. The majority of the man who use it cannot enter into a deep-going analysis of it; for them, its success is its proof. It can thus happen that a view which has led to the greatest discoveries, like Black's theory of caloric, may actually become an obstacle to progress by its blinding our eyes to facts which do not fit in with our ideas. If a view is to be protected from this dubious rôle, the grounds of its evolution and existence must be examined from time to time with the utmost care. We will here try to do this for thermodynamics and the principle of energy.

2. The most multifarious physical changes, thermal, electrical, chemical and so forth can be brought about by mechanical work. If such alterations can be completely reversed, they yield anew the mechanical work in exactly the quantity which was required for the production of the change in question. This is the principle of the conservation of energy, "energy" being the term used for that indestructible something which

characterizes the difference of two physical states and of which the measure is the mechanical work which has to be performed in the passage of one state to the other.

How did we acquire this idea? The opinions which are held concerning the foundations of the law of energy diverge very widely from one another. To many physicists it now suddenly appears to be evident a priori. Others trace the principle to the impossibility of a *perpetuum mobile* which they regard as self-evident. Others start from the theory that all physical processes are purely mechanical processes and hence deduce the impossibility of a perpetuum mobile in the whole physical domain. Other inquirers, finally, are for accepting only purely experimental establishment of the law of energy. We will investigate these views, and it will appear from the discussion to follow that there is also a logical and purely formal source of the principle of energy which has hitherto been little considered.

The principle of energy in its modern form is not identical with the principle of the excluded *perpetuum mobile*, but it is very closely related to it. The latter principle, however, is by no means new, for centuries ago it guided the greatest investigators like Stevinus, Galileo, Huygens and others, on their paths of discovery. This has been shown in detail in my book of 1872 cited in the first note of this chapter and in my *Mechanics*. But since the correctness of this principle was felt long before the structure of mechanics was raised, and since the principle even contributed in a high degree to the founding of mechanics, it is probable that it does not really rest on knowledge acquired in mechanics but that its roots are to be found in more general and deeper convictions. We will return to this point.

As far as the history of physics reaches, from the time of Democritus to the present day, there has been an unmistakable tendency to explain all physical processes mechanically. This is sufficiently illustrated by the quotations from Huygens and Sadi Carnot given in §§1 and 4 of Chapter XIII. This tendency is also quite intelligible: motions of bodies are the simplest and easiest events to follow by the help of our senses. We reproduce mechanical processes almost without effort in our imagination. The connexion of pressure and motion is very familiar to us from daily experience. All changes which the individual personally produces in his environment, or which humanity brings about by means of the technical arts, are effected through the instrumentality of motions. Of necessity therefore, motion appears to us as an important and the best-known physical factor. Moreover a mechanical side is shown by almost all physical events. The sounding bell trembles, the heated body expands, the electrified body attracts other bodies. Why, therefore, should we not attempt to explain or represent the less known by the better known? In fact, there can be no objection to illustrating the properties of physical events by mechanical analogies.

But modern physics has proceeded too far in this direction, and has taken these attempts much too seriously and too literally. I cannot follow Wundt in setting up as an axiom of physics that all physical causes are motional causes, and finding that change of place is the only change of a thing in which a thing remains identical with itself. We have only to remember that the Eleatics encountered difficulties of exactly the same sort in motion as Wundt did in qualitative change. In this way we come to regard as non-existent everything in the world which we do not immediately understand. On this principle it would really be the simplest course to deny the existence of the whole phenomenal world. This is the point at which the Eleatics ultimately arrived, and the school of Herbart stopped little short of the same goal.

Physics treated in this sense supplies us with a very artificial scheme of the world, in which we hardly recognize reality. It happens, in fact, to men who give themselves up to the mechanical and atomic view for many years, that the familiar world of sense suddenly becomes, in their eyes, the supreme "world-riddle".

Thus, without considering physical processes to be identical with mechanical ones, we can use mechanical processes for illustration of physical ones. The only permanent thing mechanical physics has done consists of the illustration of whole chapters of physics by mechanical analogies — think of the theory of light, for instance — or in the determination of exact quantitative relations between mechanical and other physical processes, as is the case in thermodynamics.

3. The principle of the excluded *perpetuum mobile* can be seen in the clearest and easiest way in the domain of pure mechanics; and indeed it first took root in that domain. If, now, we conceive all physical processes as mechanical ones, we naturally conclude that there must be an analogous principle for the whole domain of physics. Even Helmholtz, not much more than half-a-century ago, tried to give a firm basis for the principle of energy, from which it could be deduced, by assuming that all physical processes are conditioned by motions of

atoms under the influence of central forces. We will not attempt to deny that mechanical physics has been of use in this direction, whether we can still maintain the truth of much or little of it.

It is only from *experience* that we can know whether and how thermal processes are connected with mechanical ones. Technical interest and a need for clearness met in the brain of Sadi Carnot and drew his attention to this point. The great industrial importance of the steam-engine was very influential here, although it is only a historical accident that the development of science referred to was not connected with electrotechnics. Franz Neumann, indeed, followed exactly Carnot's way of thinking when establishing the laws of induced electric currents (1845)[3]. The peculiarity in Carnot's idea consists in the fact that he was the first to exclude the *perpetuum mobile* in a wider domain than that of pure mechanics, and assumed that even a use of thermal processes cannot give a *perpetuum mobile.* However, the modern principle of energy was not held by Carnot, for he still kept Black's notion of caloric, which completely dominated Black for psychological reasons that we have already discussed.

4. A new transformation of a formal nature was necessary to enable the modern principle of energy to appear. Black's notion had to be destroyed by Mayer and Joule; and a new and more abstract notion, which is here to have the qualities of a substance, put in its place.

Here too the psychological circumstances which gave power to the new idea lie before us quite clearly. Mayer was led, by the striking redness of venous blood in a tropical climate, to pay attention to the smaller output of heat from the human body in this climate and the correspondingly smaller consumption of material. Since every performance of mechanical or other work by the human body is connected with consumption of material, and work can again generate heat by friction, heat and work appear as of the same kind, and between them there must be a proportional relation. Not each single quantity indeed, but the suitably estimated sum of both of them appears as connected with a proportional consumption of material, and like a *substance.*

By quite analogous considerations which were connected with the economy of galvanic batteries, Joule arrived at his views. He found experimentally that the sum of the heat developed by the current, of the heat of burning of the explosive gas produced, and of the work of the electromagnetic current, expressed in suitable units — in short, of all the performances of the battery — is proportional to the consumption of zinc. Accordingly this sum itself is like a *substance.*

When once the point of view of a theory of energy has got a foothold in the domain of the theory of heat, it can be carried without difficulty into all domains of physics. Mayer and Joule soon regarded the sum of all energies as constant, that is to say, they viewed this sum as a substance.

Mayer was so impressed by the view he had reached that the conservation of what he called "force" and we call "work" seemed to him evident *a priori.* "The creation and the destruction of a force", said he, "lies outside the province of human thought and activity". Also Joule expressed himself in much the same way: "We might reason, *a priori*, that such absolute destruction of living force cannot possibly take place, because it is manifestly absurd to suppose that the powers with which God has endowed matter can be destroyed any more than that they can be created by man's agency: but we are not left with this argument only, decisive as it must be to every unprejudice mind". Strange to say, such utterances have stamped Mayer, but not Joule, as a "metaphysician". But we may be quite certain that both men only gave expression. Half unconsciously, to a strong *formal* need of the new and simple view, and that both would have been greatly surprised if anyone had suggested to them that they should allow the validity of their principle to be decided, say, by a congress of philosophers.

These men carried out their work very differently in most respects, although in some respects the ways were very much alike. Mayer pleads for the formal need with the great instinctive force of genius — we might almost say with a kind of fanaticism — but we must remember that he did not lack the conceptual power to calculate the mechanical equivalent of heat before all other investigators from numbers which had long been known and were at everyone's disposal; and to set up a program for the new theory which embraced all physics and philosophy. Joule, on the other hand, devoted himself to the thorough foundation of the principle in all domains of physics with experiments wonderfully prepared and carried out. Somewhat later Helmholtz also took the question in hand in a quite independent and different way. Besides the professional ability with which Helmholtz showed himself capable of subduing all the unsolved problems of Mayer's program — and others besides — we here see, and are astonished by, the critical clearness of sight of this young man of only twenty-six. His exposition has not the impetus of Mayer's; to him the principle of the conservation of energy is not a fact evident *a priori*. What follows, he asks, *if* it holds? It is in this hypothetical form that he ovecomes his problem.

I must confess that I have often been surprised at the aesthetic and ethical taste of many of our contemporaries who persist in mixing up odious national and personal questions with this matter, instead of congratulating themselves on the good fortune that allows many men of such greatness to work at the same subject at the same time, and without rejoicing in the differences of great intellects which is so fruitful and instructive to us from the point of view of theory of knowledge.

When I say that the step first taken by Mayer was a *formal* transformation, this needs some support, for it is usually to Mayer and Joule that we ascribe the discovery "that heat is motion". For — so we read in popular writings — if the quantity of heat can be increased and diminished, it cannot be matter and must be motion. Mayer himself never agreed to this conclusion, and we may convince ourselves that this conclusion never played an important part in the great progress in physics due to the discovery of the conservation of energy.

5. Certainly only *experience* can teach us whether motion takes the place of a loss of heat or inversely; and in what measure it does so. The fact of the connexion of the two and the magnitude of the mechanical equivalent are thus, without any doubt, results of experiment. But there still remains a great deal of room for the *formal* viewing of the matter. That falling through a height h generates a velocity v, that with this velocity the original height can again be reached, and that quantitatively $v = \sqrt{2gh}$, can only be determined by experiment. But in this there is nothing about an equivalence; for the last equation had been used for a long time without any thought of an equivalence. But if I say that the v is to be worth as much to me as the h which it can overcome, this is a form of view which may correspond to my need. I may very well feel this need without being able to satisfy it, as happened in the case of Mayer for as long as he supposed that v and gh were equivalent. Only when I measure the value of the velocity by $v^2/2$ and that of the height of falling by gh, do I succeed in satisfying my need.

Thus I say also that heat is to be worth as much to me as the work used to make it appear. By a lucky shot, and favored by historical circumstances, Mayer at once found the correct evaluation which satisfied his needs. But the notion of caloric here again plays a quite unimportant part, as we see by the following considerations.

"The quantity of water remains constant when it performs work because it is matter. The quantity of heat varies because heat is immaterial". These two statements will appear satisfactory to most people; and yet both are quite worthless. We will make this clear by the following question which bright students, when the theory of energy was less a subject of popular lectures than it is now, have sometimes put to me. Is there a mechanical equivalent of electricity just as there is a mechanical equivalent of heat? Yes, and no. There is no mechanical equivalent of quantity of electricity as there is an equivalent of quantity of heat, because the same quantity of electricity has a very different capacity for work, according to the circumstances in which it is placed; but there is a mechanical equivalent of electrical energy.[4]

Let us ask another question. Is there a mechanical equivalent of water? No, there is no mechanical equivalent of quantity of water, but there is a mechanical equivalent of weight of water multiplied by its distance of descent.

When a Leyden jar is discharged and work thereby performed, we do not imagine that the quantity of electricity disappears as work is done by it, but we simply assume that the electricities come into different positions, equal quantities of positive and negative electricity being united with one another.

What, now, is the reason of this difference of view in our treatment of heat and of electricity? The reason is purely historical, wholly conventional, as is shown by what follows. In 1785 Coulomb constructed his torsion balance, by which he was enabled to measure the repulsion of electrified bodies. Suppose we have two small balls, A and B, which are similarly electrified over their whole surfaces. These two balls will exert on one another, at a certain distance r of their centres from one another, a certain repulsion p. We bring into contact with B, now, a ball C, let both be equally electrified, and then measure the repulsion of B from A and of C from A at the same distance r. The sum of these repulsions is again p. Accordingly the repulsion has remained constant in this division. If we ascribe this effect to a special agent, then we infer naturally its material nature and the constancy of its quanity.

In 1838 Riess constructed his electrical air thermometer. This gives a measure of the quantity of heat produced by the discharge of a jar.

This quantity of heat is not proportional to the quantity of electricity contained in the jar by Coulomb's measure; but, if q be this quantity and c be the capacity, it is proportional to $q^2/2c$, or the energy of the charged jar. If, now, we discharge the jar completely through the thermometer, we obtain a certain quantity of heat W. If we make the discharge through the thermometer into a second jar, we obtain a quantity less than W. But we may obtain the remainder by completely discharging both jars through the air thermometer, when it will again be proportional to the energy of the two jars. On the first incomplete discharge, accordingly, a part of the electricity's capacity for work was lost.

When the charge of a jar produces heat, its energy is changed and its value by Riess's thermometer is decreased. But the quantity by Coulomb's measure remains unaltered.

Now let us imagine that Riess's thermometer had been invented before Coulomb's torsion balance, which is not a difficult feat of imagination, since both inventions are independent of each other. What would then be more natural than that the "quantity" of electricity contained in a jar should be measured by the heat produced in the thermometer? But then this so-called "quantity of electricity" would decrease on the production of heat or on the performance of work, whereas, according to our present ideas, it remains unchanged. In the first case, therefore, electricity would not be a substance but a motion; whereas now it is still a substance. The reason, therefore, why we have other notions of electricity than we have of heat, is purely historical, accidental, and conventional.

This is also the case with other physical things. Water does not disappear when work is done by it. Why? Because we measure quantity of water with scales, just as we do electricity. But suppose the capacity of water for work were called "quantity"; and had to be measured, therefore, by a *mill* instead of by *scales*; then this "quantity" would disappear as it performed work. It may, now, be easily conceived that many substances are not so tangible as water. In that case we should be unable to carry out the one kind of measurement with the scales, while many other modes of measurement would still be left to us. The mode of measurement we use would, then, have an influence on these ideas of ours.

In the case of heat, the historically established measure of "quantity" happens to be the work value of the heat. Accordingly, this quantity disappears when work is done. But that heat is *not* a substance follows from this as little as does the opposite conclusion that it *is* a substance.

If we explode a mixture of oxygen and hydrogen in a eudiometer tube, the phenomena of oxygen and hydrogen vanish and are replaced by those of water. We say, now, that water consists of oxygen and hydrogen; but this oxygen and this hydrogen are merely two thoughts or names which we keep ready to describe phenomena which are not present, but which will appear again whenever, as we say, we decompose water.

It is just the same with oxygen as with latent heat. Both may appear when at the moment, they cannot yet be noticed. If "latent" heat is not a substance, oxygen need not be one.

We see from this that the notion that heat is a material body is quite irrelevant, and simply depends on the point of view chosen at our own will. Today it would be more convenient to say "energy of heat" instead of "quantity of heat". In the cases considered by Black, the energy of heat behaves like matter in so far as it does not change into other forms of energy.

6. We are now prepared to answer the question as to the sources of the principle of energy. All knowledge of nature is derived in the last instance from experience. In this sense they are right who look upon the principle of energy as a result of experience.

Experience teaches that the sensible elements α, β, δ, γ ... into which the world may be resolved are subject to change. It tells us, further, that certain of these elements are connected with other elements, so that they appear and disappear together; or that the appearance of one group of these elements is connected with the disappearance of another. The sensible elements of the world (α, β, γ, δ, ...) show themselves to be interdependent. Facts may be so nearly related as to contain the same kind of α, β, γ, ..., but the α, β, γ, ... of one differ from the α, β, γ, ... of another only by the number of equal parts into which they can be divided. These facts then only differ quantitatively; and rules can be given for deducing from the number of the parts of one group of those α, β, γ, ... the number of the parts of another groups. Between the measures equations exist. The simple fact of change brings it about that the number of these equations must be smaller than the number of the α, β, γ, ... If the former be smaller by one than the latter, then one group of the α, β, γ, . . . is uniquely determined by the other group.

The quest of relations of this last kind is the most important function of special experimental research, because we are enabled by it to complete in thought facts that are only partly given. Obviously only experience can ascertain that between the α, β, γ, . . . relations exist, and of what kind they are. Further, only experience can tell that the relations that exist between the α, β, γ, . . . are such that changes of them can be reversed. If this were not the fact, there would be no possibility of a principle of energy. Consequently, in this sense the ultimate source of the principle of energy is experience.

But this does not exclude the fact that the principle of energy has also a logical root. Let us assume on the basis of experience that one group of sensuous elements α, β, γ, . . . determines uniquely another group λ, μ, ν, . . . Experience further teaches that changes of α, β, γ, . . . can be reversed. It is then a logical consequence of this observation that every time that α, β, γ, . . . assume the same values, this is also the case with λ, μ, ν, . . . or that purely periodical changes of α, β, γ, . . . can produce no permanent changes of λ, μ, ν, . . . If the group λ, μ, ν, . . . is a mechanical group, then a *perpetuum mobile* is excluded.

It will be said that this is a vicious circle. But psychologically the situation is essentially different whether I think simply of the unique determination and reversibility of events, or whether I exclude a *perpetuum mobile*. The attention takes in the two cases different directions. Without any doubt, it was this firm logical connexion of ideas, a fine instinctive feeling for the slightest contradiction, which guided the greatest investigators such as Stevinus and Galileo. By it our thoughts lose a degree of freedom and the possibility of error is lessened. In this general logical conviction which preceded the founding of the science of mechanics lies the other root that the principle of the excluded *perpetuum mobile* has besides experience.

The principle of excluded *perpetuum mobile* is naturally closest to our usual ideas when it is stated for the domain of pure mechanics. This principle can, as has appeared, be found without using the concept of "work". However, if we introduce the formal substance-like conception of the sum of work and *vis viva*, we get Huygens' principle of *vis viva*.

If we exclude *perpetuum mobile* from the whole domain of physics, this principle is then very closely connected, though not identical, with the principle of energy. In order to get the principle last named, we must have the arbitrary, formal, substance-like view of work and of every physical change of state connected with the performance or expenditure of work, which corresponds to our need of simplicity and economy. The need we have just referred to could only be satisfied by the creation of special concepts of measurement. But with this the development in question was, in essentials, brought to a close.

7. It seems to me, now, that by the separation of the experimental, logical, and formal roots we put aside the mysticism that people are so prone still to drag into the principle of energy. This principle cannot be set up and applied without the knowledge of important facts constituting the dependence of different reactions on one another; but still a very important part of the principle is our own spontaneous and formal view of the facts. It is not here so much a question of the discovery of new facts: the facts on which the principle is based were, in great part, known for a long time, but had escaped notice. But just as was the case with Copernicus, the question was chiefly the discovery of a *form* of viewing facts. On this question I can still hold to the point of view I expressed in 1872.

CHAPTER XIX

EXTENSION OF THE THEOREM OF CARNOT AND CLAUSIUS. THE CONFORMITY AND THE DIFFERENCES OF ENERGIES. THE LIMITS OF THE PRINCIPLE OF ENERGY

1. It lies in the nature of our knowledge when it is developing that we represent newly perceived facts by the help of conceptions which we have already acquired. For this purpose we either ascribe to the new phenomena properties which have become known to us from another quarter, or deny to them properties which have been falsely ascribed to them. In what is new we either find analogies or points of difference with what is already known.

Indeed, the first great step in Carnot's discovery was the consideration of an analogy between water which, by falling, performed work; and heat which, by sinking in temperature performed work. Carnot was led to his reversible cyclic process by paying attention to the fact that heat, like water, must not flow away without being used if the maximum of work is to be performed.

This maximum's independence of the working substance results from the assumption that the principle of the excluded *perpetuum mobile* holds in all domains of phenomena. Further, Carnot kept to the analogy between water and heat even so far as to suppose with Black that the quantity of the heat used was constant; and this supposition he himself had to give up later on. Paying attention to this correction, Clausius's form of Carnot's theorem at once results, and this form will be discussed in what follows.

2. Zeuner, in order to throw light on the meaning of the expression Q/T, which he called "weight of heat (*Wärmegewicht*)", tried to imitate Carnot's cyclic process with heat by a process with heavy masses. With regard to Zeuner's exposition, we must remark that it entirely corresponds to Carnot's view. This view, indeed, is dominated wholly by the analogy, referred to. However, the analogy between a thermal and a mechanical process may be carried further than Zeuner has done. In his process, the moving of weights backwards and forwards to different heights has only a very external likeness to the absorption and emission

of heat in performance or consumption of work. Imagine (Fig. 101) a very large receptable A filled with liquid to the height h_1, and suppose that A is in connection with a smaller receptacle k. Let us move the vertical side of k through the space m, and then shut off k from A and then again move the side so far that the height of the fluid in k sinks to h_2. Then let us connect k to a very large receptacle B containing fluid to the height h_2, and let us diminish the size of k until the weight of fluid P taken from A on the displacement m is given up to B. Then k contains, when it is isolated from all other receptacles and brought back to its initial volume, the original quantity of fluid at the original height h_1.

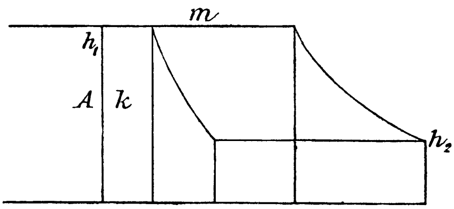


Fig. 101.

In this process the energy $W' = P(h_1 - h_2)/2$ is expended in external work while the energy $W = Ph_2/2$ is transferred from h_1 to h_2. Thus we have the equation

$$\frac{W'}{h_1} + W\left(\frac{1}{h_2} - \frac{1}{h_1}\right) = 0, \tag{1}$$

which is identical in form with (35) of Chapter XV. If we take the energy absorbed by A to be $W_1 = Ph_1/2$ and that given off to B to be $W_2 = Ph_2/2$, we have

$$-\frac{W_1}{h_1} + \frac{W_2}{h_2} = 0, \tag{2}$$

which is identical in form with (38) of the same chapter.[1]

We see here that the same weight is taken from A as is given up to B. On the other hand a greater energy is taken from A than is given up to B. The energy is accordingly analogous to the quantity of heat, but to the mass and weight corresponds the "weight of heat" of Zeuner. I believe that I was the first to bring forward in an earlier work[2] of mine the historical circumstances which led to such different views with respect to the energy of heat and other forms of energy. The difference lies partly in the subject itself and partly in historical conventions.

3. In the same work I also tried to generalize Carnot's ideas. I had noticed that, for all forms of energy, if a part W' of energy is transformed in any way into another part W of energy, the remainder sinks from a level V_1 to a lower level V_2, and the equation (1) holds if we substitute V_1 and V_2 for h_1 and h_2 respectively. Naturally the other equations given with (1) also hold.

The reversible cyclic process which performs work is not limited to thermal processes. There is no difficulty in imagining analogous cyclic processes for any other events whatever electrical, for instance.[3] For example, let A be a body of very great capacity which is charged to the potential V_1, and B another such body with the potential V_2. Let a sphere k which is in conduction with A increase isopotentially from the radius r_0 to the radius r_1. In this process the energy

$$W_1 = (r_1 - r_0)\,V_1^2$$

is taken away from A. Further, let k be insulated and expand adiabatically to the radius r_2, when its potential decreases to V_2, so that $r_1 V_1 = r_2 V_2$. Further, let an isopotential contraction to the radius r_3 take place when k is in conduction with B, until the whole quantity taken from A is given up to B. Finally, let k be again insulated and compressed adiabatically to r_0, so that the sphere with the original charge again has the original potential. From the last condition we have $r_0 V_1 = r_0 V_2$.

Further, we have

$$(r_2 - r_3)\,V_2 = (r_1 - r_0)\,V_1.$$

The energy given to B is therefore

$$W_2 = (r_1 - r_0)\,V_1 V_2.$$

Thus the equation (1) or (2) holds, as we can see by substitution.

For the economical coefficient we have, in the case of heat,

$$\frac{Q}{Q + Q'} = \frac{T_1 - T_2}{T_1}\;;$$

and, in the case of electricity, with an analogous notation:

$$\frac{W'}{W+W'} = \frac{V_1 - V_2}{V_1}.$$

4. The purpose of the reversible cyclic process with Carnot is merely, on the one hand, the avoiding of useless losses of energy which cannot be transformed into mechanical work, and, on the other hand, the setting aside of the unknown incalculable (latent) energies. In the cases where we need not pay attention to these two circumstances, we do not need to consider a cyclic process for the establishment of the theorem of Carnot and Clausius. For this purpose it is enough to know the transformability of energies into one another and the fall of potential of the decreased kind of energy which accompanies it.

We can say quite generally: If of one kind of energy $W' + W$ of potential V_1 the part W' is transformed into one or many other forms, the remainder W suffers a fall of potential to V_2, and the equation

$$-\frac{W'}{V_1} + W\left(\frac{1}{V_2} - \frac{1}{V_1}\right) = 0$$

holds. From this equation the other equations connected with it follow.[4]

For working electrical bodies embedded in insulators the consideration of a cyclic process is unnecessary if we simply count together all the transformed energy, whether it appears as kinetic or potential energy, mechanical work, heat, or in any other form. The process mentioned in §2 of this chapter is thus only a special case of that given here; the former process only having regard to the maximum of mechanical work. Besides, in regard to heat we can also imagine processes for which the consideration of a cyclic process is unnecessary. If, for example, a *perfect* gas of capacity c (at constant volume) expands adiabatically and reversibly performing work, the quantity of heat transformed Q' is $c(T_1 - T_2)$, and the quantity of heat transferred (Q) is cT_2, and the above forms of equation are then given at once.

5. This parallelism in the behaviour of different forms of energy, and indeed from the general point of view last mentioned, was referred to both in the text and in a note of my work of 1872. In the case of each kind of energy, we have to consider the *energy* value and the *level* value.

The *quantity* values, which are given by the quotients *W/V*, are referred to particularly in the text but not in the note. Still, in my *Mechanics*, which first appeared in 1883,[5] I spoke of the quantity values of different energies which correspond to one another. Even at the present time I consider that my very short exposition meets the case, and I have, it seems to me, nothing important to take back.[6]

Ideas which are allied to mine in form or matter were later often expressed by Popper[7] (1884), Helm[8] (1887), Wronsky[9] (1888), Meyerhoffer[10] (1891), and Ostwald[11] (1892).

Of course I am far from suggesting that even those authors to whom my work was known received the stimulus to their investigations merely from it.[12] They would have to have been very near to my standpoint already in order to have extracted all the consequences from my short exposition. Personal differences in the way the matter is viewed appear quite clearly enough to keep us from making such a supposition. Besides this, it is here merely a question of the generalization of one of Carnot's ideas, to which Carnot himself gave the chief stimulus. On the other hand attentive readers of my publication of 1872 will see that the later work mentioned added little that was new to me.

6. So far we have spoken principally of the *agreement* with one another of the forms of energy. The *differences* of these forms from one another must not be overlooked, and heat especially offers differences as compared with other forms. If we take the standpoint mentioned above we see that: (1) The mere *exact* knowledge of the conservation of energy is sufficient to obtain the theorem of Carnot and Clausius: and (2) Because this theorem holds for the different forms of energy, a special position for heat is not conditioned by this theorem.

7. As regards the first point. We have already mentioned that a full insight into the conservation of energy not only allows us to recognize the transformation of one kind of energy *A* into another *B* but also the fall of potential of *A* and the rise of potential of *B* which is necessarily connected with this transformation. That the two properties of the transformation of energy have been formulated in two different theorems — the two "laws of thermodynamics" — is only due to the historical accident that a period of twenty years lies between our knowledge of them. Only after ten years more were both properties expressed in the theorem of Carnot and Clausius which is simply a more

complete expression of the fact of which the first law shows only one side.[13] In the history of physics this is not without an analogue. As I have shown elsewhere,[14] the law of inertia accompanied the perception that forces are circumstances that determine acceleration through two centuries as an independent theorem, although in this case both theorems are identical — the latter is the negative inversion of the former.

8. With respect to the second point, the following is clear. Analogy is not identity; heat may therefore have other properties which are peculiar to itself. In fact, it has such properties; but this particularity lies in circumstances independent of the theorem of Carnot and Clausius. Every transformation of a kind of energy A is connected, in the case of heat as well, with a fall of potential of the kind of energy considered. But while, for other kinds of energy, with the fall of potential a transformation is inversely connected — and consequently a loss in energy of the kind which falls in potential —, heat behaves in another way. Heat *may* suffer a fall in potential without experiencing a loss of energy — at least according to the usual way of measuring it. If a weight sinks, it must necessarily generate kinetic energy or heat or some other energy. An electric charge, too, cannot undergo a fall of potential without a loss of energy, that is to say without transformation. Heat, on the other hand, may be transferred with a fall of temperature to a body of greater capacity, though the energy of heat remains the same. This is what besides its property of energy, gives heat the character of a material substance.[15]

If between two bodies of temperatures T_1 and T_2 and capacities c_1 and c_2 there is equilibrium owing to conduction, the temperature of equilibrium T is given by the equation

$$(c_1 + c_2)\,T = c_1 T_1 + c_2 T_2,$$

where Black's "quantity of heat" — or rather the energy of heat remains unaltered. Again the equilibration of potential of electrically charged bodies corresponds to the equation

$$(c_1 + c_2)\,V = c_1 V_1 + c_2 V_2,$$

but the electrical energy after the equilibration is less than the sum of energies $W_1 + W_2$ before the equilibration. In fact, the former is

$$\frac{c_1}{c_1 + c_2} W_1 + \frac{2}{c_1 + c_2} \sqrt{c_1 c_2 W_1 W_2} + \frac{c_2}{c_1 + c_2} W_2 < W_1 + W_2.$$

Nothing would prevent us from putting instead of the numbers for the temperatures T now used, the roots of these numbers $\tau = \sqrt{T}$; then we could measure the energy of the heat by $c\tau^2/2$ quite analogously to the electrical energy $V^2/2$. But the incongruence spoken of would not be altogether removed by this, but would appear in another place.[16]

9. This peculiarity of heat has also special consequences. For a body which undergoes any closed reversible cyclical process we have, according to Clausius, $\int (dQ/T) = 0$, or, if we break off the reversible process at any instant, the value of $\int (dQ/T)$ is completely determined by the state of the body at that instant — indeed it characterizes that state. On this account, the quantity spoken of was given a special name, "entropy" by Clausius.

The analogous quantity for a reversible energy-process of another kind — for example an electrical process — is $\int (dW/V)$. The equation of this expression to zero for a closed process would only give the self-evident theorem that the body when it reaches its original state again contains the same quantity of electricity. We have for the simplest reversible Carnot's process, whatever the form of energy.

$$-\frac{W_1}{V_1} + \frac{W_2}{V_2} = 0,$$

that is to say, the variation of the entropy of the working body or the sum of the variations of entropy of the two bodies of great capacity is zero.

For heat, this relation may be disturbed in the case of an irreversible process. While the energy value of a heavy mass, of an electric charge, and so on, sinks as the height of the level decreases in proportion to this height, this is not the case with heat. Indeed, to take an extreme case, the level may sink on mere conduction and without variation of the energy value so that $W_1 = W_2$. Since $V_1 > V_2$, the entropy increases in this process. Thus although an analogue of entropy can be set up for every kind of energy, this quantity is only capable of increase in the case of heat.[17]

10. If different level values of energies of the same kind meet together, it depends wholly on special physical circumstance whether the energies are in equilibrium, and whether and what transformations of energy occur. To the equilibrium of mechanical energies belongs movability, to the equilibration of electric energies belongs conductivity. The principle of energy only determines the amounts of the transformation, not the circumstances under which the transformation takes place. It is a matter for the special domains of physics to determine the circumstances. On the equilibration of mechanical, electrical, and other differences of level oscillations — periodical transformations of potential into kinetic energy — may happen by which the same circumstances may recur. The process as a whole is then reversed, although the elements of the process are not reversible in Carnot's sense.[18] In so far as a transformation into heat here takes place reversal does not take place. With differences of temperature, transformations of thermal energy into other forms may take place, — as on the setting up of a thermoelectric current. But in this case a simple equilibration of level may occur without a transformation.[19]

Thus there are special physical experiences which lie outside the scope of the theorem of Carnot and Clausius and from which results the difference in the behavior of heat and the other kinds of energy. It is also clear that a complete identity of the laws of the transformation of all energies into one another would not correspond to our picture of the world. If these laws were identical, to every transformation an inverse transformation would have to belong, and all physical states which once existed would have to be again capable of existence. Then indeed time itself would be reversible, or rather the idea of time could never have arisen.[20]

11. When we first notice this agreement in the law of transformation of energies, it appears surprising and unexpected because the reason for it does not immediately appear. But from one who follows the historical and comparative method this reason cannot long remain hid.

Work has been a fundamental conception of mechanics and an important conception in the theory of machines since the time of Galileo, although it was long without the name now used for it. The transformation of work into *vis viva* and reciprocally is closely allied to the energy view which Huygens was the first to use on a somewhat large scale, though Thomas Young was the first to use the name "energy".

If we add the invariability of weight, — or properly speaking *mass*, — it is an immediate consequence of the definition, so far as mechanical energy is concerned, that the capacity for performing work (the potential energy) of a weight is proportional to the height of level (in a geometrical sense), and that this capacity decreases proportionally to the height of level on sinking of the weight and consequent transformation of the potential energy. The zero-level is quite arbitrary in this process. Therefore we get the equation

$$\frac{W_1}{h_1} = \frac{W_2}{h_2},$$

from which follow all the results considered above.

If we reflect upon the great start in development which mechanics had as compared with all other domains of physics, it is not surprising that people attempted to apply mechanical conceptions wherever possible. Thus, for example, the concept of *mass* was imitated in Coulomb's concept of *quantity of electricity*.[21] In the further development of the theory of electricity, the conception of work was likewise immediately applied in the theory of potential, and the height of electrical level was measured by the work done to bring a unit quantity to this level. Thus the above equation with all its consequences was obtained for electrical energy as well. The case was similar with other energies.

However, thermal energy seems to be a case by itself. That heat is a energy could only be found by special experiments. But the measure of this energy by Black's "quantity of heat" depends on chance circumstances. In the first place the chance small variability of capacity for heat c with temperature and the chance small deviation of the thermometric scales commonly used from the scale of tensions of gases brings it about that the conception "quantity of heat" can be established and that the *quantity* of heat ct which corresponds to a difference of temperature t is nearly proportional to the *energy* of heat. It is quite a chance historical circumstance that Amontons, in papers of 1699 and 1702, fell upon the idea of measuring temperature by the tension of a gas. He did not think of work performed by heat. But from his method resulted that the numbers which express temperature are proportional to the tensions of a gas, and therefore to the work performed by a gas with equal variations of volume. Thus it happened that heights of temperature and heights of the level of work are proportional to one another. This relation was first consciously established by William Thomson in his scale of absolute temperature.

If characteristics of thermal state which strongly deviate from the tensions of gases had been chosen, this relation might have turned out to be very complicated, and the coincidence considered at the beginning between heat and the other energies would not have held. It is very instructive to reflect about this.

There is thus no law of nature in the conformity of the behaviour of energies, but this conformity is conditioned by the uniformity of our way of viewing the facts, and it is partly a matter of luck.[22]

12. From the standpoint which we have indicated above we notice, besides the conformity of energies, a special difference between heat and other forms of energy. It is true that the ratio of the transformed energy to the transferred energy is for all forms $(V_1 - V_2)/V_2$, and the ratio of the transformed energy to the total expenditure, — the economical coefficient, — is, for all forms, $(V_1 - V_2)/V_1$; but the zero-point of the level is, for all energies with the exception of heat, *arbitrary* or at least variable according to circumstances, while for heat, on the other hand it lies fixed at −273 °C. The reason of this is that the physical states of bodies are usually determined by differences of the value of the potential from those of neighboring bodies, while with respect to the states considered here the *temperatures* and not the differences of temperatures are what matter.[23] Whether the body is rigid, fluid, or gaseous is determined by its *temperature*, and in particular *gaseous tension*, with which we have to do here, is proportional to the absolute temperature. The absolute zero-point must therefore be retained if the energies of heat are to remain proportional to the heights of level; and this is the condition of the conformity considered here.[24]

According to Carnot's view the same coefficients of ratios which hold for gases retain their value for all bodies at the same temperature. Thus it seems as if the absolute zero-point had a quite special physical meaning. Indeed it has been supposed that a cooling below this temperature is not thinkable, that a body of temperature −273 °C contains no thermal energy and so on. But I believe that these conclusions rest on an impermissible and too artificial extrapolation. I have remarked elsewhere that the numbers which indicate temperature are simply signs of order which we coordinate to certain characteristics of the thermal state by some rule or other. The finiteness or infiniteness of this system of signs can give no information about the finiteness or infiniteness of the series of states of heat: this is wholly a matter of experience.[25]

With regard to the point here discussed, the following must be added. The principle of the excluded *perpetuum mobile* only tells us that we can obtain the same work from any body between the given temperatures T_1 and T_2 as we have found empirically to be the maximum with a perfect gas. The formula by which this effect is represented does not matter in the least. But the principle does not allow us to draw any conclusion at all about the behaviour of a perfect gas outside the limits of the thermal state within which alone it has been tested; and thus does not allow of any inference about the behaviour of any other body outside these limits. If we could experiment with a perfect gas with zero tension, this gas would not be suitable for purposes of work. But from this would not follow that at this and lower thermal states still other means — thermoelectric currents for instance — could not produce work.[26]

13. Let us now glance at the development of the conception of energy. This concept owes its origin to analogy. It is always the strongest and most familiar ideas and conceptions which are brought forward for the representation of new facts, and which strive in a sense to replace less familiar ideas. The *conception of substance* is one of the most familiar of conceptions which has arisen unconsciously. By "substance" is usually understood what is *absolutely* permanent. However, I think I have shown that there is nothing which is absolutely permanent and that there only exist permanencies of reaction, — to use a chemical[21] expression, — and permanencies of combination or condition. Every physical permanency always finally reduces to the fact that one or many *equations* are satisifed, and therefore it reduces to a permanent law in the change of processes.[27]

This holds even in the simplest cases. If a rigid body moves and yet appears to retain all its properties with the exception of its position, this view cannot withstand an exact criticism. All the reactions of the body — those with respect to the sense of sight and touch, for instance — are altered in this process, and are, besides, the *same* under *similar* recurring circumstances. A more mobile, and therefore from a physical point of view more useful idea of substance arises from the considera-tion of a fluid or a divisible (quasi-fluid) body. Here it is a sum of reactions which remains permanent: what is lacking in one place appears at another. The mathematical form which the conception of substance takes is the conception of a continually invariable sum. The practised mathematical imagination makes only a slight distinction in the case when the elements considered give some constant sum — or in particular a zero sum — or whether they fulfil any other condition or equation. However, the making constant of a sum finds the most fertile application as the most primitive and simple expression of the mathematical conception of substance.

Wherever and whenever a reaction vanishes and one of the same kind appears elsewhere, the need arises of a simple and easily imagined view of this process, and consequently the conception of substance comes into play. In this way the conceptions *quantity* of heat, *quantity* of electricity, and so on, arise owing to the observation that a body is warmed, electrified, and the like at the expense of others. With the need for an application of the conception of substance, the problem is of course only proposed and not solved. Only a special and attentive investigation and observation of the facts shows that the sum of the products of the masses and variations of temperature (of bodies of the same kind) gives the constant sum of the "quantities of heat", and the sum of forces towards a given electrical charge at a definite distance gives the constant "quantity of electricity".

The idea of substance which has become familiar is not given up, even when it is no longer quite suitable, but is suitably modified. Thus Black, instead of giving up the constancy of the quantity of heat for the case of the processes of melting and vaporization, preferred to retain this constancy and to regard a melted or vaporized mass as *equivalent* to a vanished quantity of heat. With the assumption of "latent" heat, the principle of summation of reactions merely of the *same kind* was broken through, and an important step taken in the direction of the modern view of Robert Mayer. The modern principle of energy, however, goes still farther and introduces such a valuation of the most different reactions that when *all* are added together the same constant sum results for all processes. Thus we can conceive this sum as one substance.

We can, as I have shown elsewhere,[28] apply the conception as substance everywhere; for example we can express the law of Mariotte and Gay-Lussac in the form

$$\log(p) + \log(v) + \log(T) = \text{const.}$$

Of course this view only holds for the limited domain of facts for which it was established. But this is so in other cases as well; with respect to Black's "quantity of heat" for instance. When Clausius found by physical investigations the equation

$$\frac{Q}{T_1} + Q\left(\frac{1}{T_2} - \frac{1}{T_1}\right) = 0$$

for a reversible process, and derived from it the "equivalence value" $\mp Q/T$ for a quantity of heat taken away from — or supplied to — a body, or transformed into work — or arising from work, as the case may be —; then this equivalence value was a method of valuation which was *intentionally* so chosen that the conception of energy as a substance would be carried out. But even for an irreversible process *this* conception as a substance no longer holds. We may apply the equivalence value only to the quantities of heat supplied and taken away, and not to the quantities remaining in the bodies, unless we wish to obtain wholly different results from those which Clausius did. If, for example, we equalize by conduction the temperatures T_1 and T_2 of two bodies with the capacities c_1 and c_2, the heat of one will sink and the heat of the other will be raised. If we apply the equivalence value represented by the second term of the left-hand of the above equation, we find the sum of the equivalence value to be

$$\frac{1}{T}\left[c_1(T_1 - T) + c_2(T_2 - T)\right] = 0,$$

while in Clausius's sense the sum of the equivalence values is positive.[29]

We see that when we apply the equivalence value to the heat already contained in a body, Black's substance takes the place of that of Clausius. The conceptions "quantity of heat", "weight of heat", "equivalence value", and "entropy",[30] must therefore be carefully held apart just as the domains of facts for which they were established.

14. When we have got so far, we naturally ask whether the substance view of the principle of energy — which is certainly valid within very wide limits, holds *without* limit. The measure of energy rests on the fact that we can make any physical reaction vanish and replace it by mechanical work, and conversely. But there is no meaning in attributing a work value to a quantity of heat which cannot be transformed into work.[31] Accordingly it seems that the principle of energy, just like any

other substance view, only holds for a limited domain of facts, and over this limit we are only too prone to stray.

I am certain that a doubt as to the unlimited validity of the principle of energy will seem as surprising at the present time as a doubt of the constancy of the quantity of heat would have seemed to the followers of Black. But we must reflect that every dominating theory tends to extend its dominion beyond what is proper. Leslie calculated the force of tension and mass of the material of heat with the same certainty and faith in his convictions as at the present time people calculate the masses, velocities, and mean paths of the molecules of a gas. It is not a question here of a dispute about facts, but about the suitability of a view.

18. The following are the principal results of the foregoing investigation. The various energies show in their behavior a likeness which has its historical ground in the fact that the heights of level were measured in units of mechanical work. With respect to the energy of heat, however, this likeness is owing to a historical chance. Besides this agreement the energy of heat deviates from other energies in the fact that it can undergo a fall in potential without a decrease of energy, and that the zero-point of the level cannot be chosen arbitrarily. The principle of energy consists in a special form of viewing facts, but its domain of application is not unlimited.

CHAPTER XX

THE BORDERLAND BETWEEN PHYSICS AND CHEMISTRY

To enter into chemical — and especially the thermochemical — questions which have acquired such a rich and extensive literature, is beyond the scope of this book. But physical considerations which lead directly up to these questions will be considered here. These considerations are due to James Thomson[1] who first made them known in 1873.

If we consider water in two different states or "phases" (using Willard Gibbs's expression) as liquid and vapour, then to every temperature corresponds a definite pressure — the maximum vapour pressure under which both phases can coexist, which belongs to that temperature. Diminution of the pressure would be immediately compensated by new evaporation, but increase of pressure would liquefy the vapor, so that water and vapor subsist simultaneously at given temperature only at a definite common pressure $p = \psi(t)$.

Water subsists together with ice at 0 °C and atmospheric pressure. Increase of pressure lowers, and diminution of pressure raises, the melting point by 0.0075 °C for every atmosphere. Thus there belongs also for the coexistence of ice and water at a given temperature a definite pressure $p = \phi(t)$.

Since ice evaporates, ice can also subsist together with steam. It might now be thought that at 0 °C ice, water and steam coexist. It is, however, to be taken into consideration that 0 °C is the melting point of ice for *atmospheric pressure* while steam at this temperature has only a very slight tension (4.57 mm mercury). Thus, if the whole system of the three bodies is supposed to be at atmospheric pressure, the steam is liquefied and thus only ice and water remain. But if the pressure of the system is lowered to approximately 4.57 mm, the melting point of the ice is raised by approximately 0.0075 °C. Thus ice of 0 °C temperature cannot subsist together with water and steam.

It is seen from the foregoing that at a temperature which is very nearly $t = +0.0075$ °C and a pressure which is very nearly $p = 4.57$ mm ice, water and steam subsist together, and they do so only under these circumstances. Laying off the temperatures on OT (Fig. 102) and the pressures in the direction OP, the curve $p = \phi(t)$ for ice and water is almost a straight line AB, where OA, represents the atmospheric pressure, OB the temperature $+0.0075\,°\text{C}$, and O the zero-point. The curve MN or $p = \psi(t)$ which corresponds to the coexistence of water and steam is also drawn. For the point of intersection K of the two, t and p have definite values t and p, for which ice, water, and steam coexist.

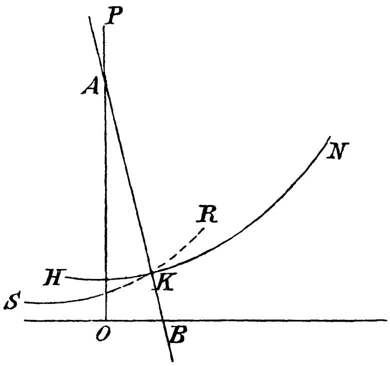


Fig. 102.

The curve RS or $p = \chi(t)$ which holds for the coexistence of ice and steam, Regnault thought to be identical with $p = \psi(t)$ (water and steam). The point of intersection of the three curves φ, ψ, χ is K, a triple point. We can convince ourselves, however, of the difference of the curves ψ and χ by considering cyclic processes which are quite similar to those employed by James Thomson for ascertaining the diminution of the melting-point of ice by pressure as we have seen in §17 of Chapter XIII. For the curve ψ we have

$$\frac{(\partial p/\partial t)\,dt\,dv}{(\partial Q/\partial v)\,dv} = \frac{dt}{\mu},$$

and likewise for χ:

$$\frac{(\partial p'/\partial t)\,dt\,dv}{(\partial Q'/\partial v)\,dv} = \frac{dt}{\mu},$$

where μ denotes Carnot's function. Since $\partial Q/\partial v$ corresponds only to the conversion of water into vapor and $\partial Q'/\partial v$ to the melting of ice *and* its conversion into steam vapor, we have for the triple point, using the well-known latent heats for vaporisation and fusion, $\lambda = 606.5$ and $l = 80$

$$\frac{(\partial p/\partial t)}{(\partial p'/\partial t)} = \frac{(\partial Q/\partial v)}{(\partial Q'/\partial v)} = \frac{\lambda}{\gamma + l} = \frac{606.5}{686.5} = \frac{1.00}{1.13}.$$

It is clear from this that the curves ψ and χ cut in the point K at an angle different from zero, and that they are therefore distinct.

The points of the plane POT represent the different states of pressure and of temperature. In the accompanying Figure 103, the field

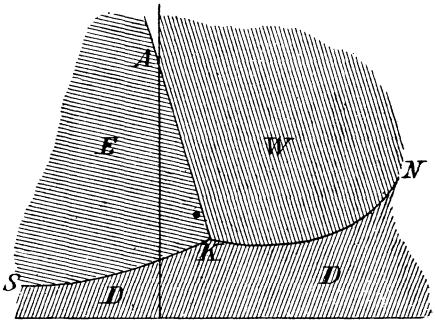


Fig. 103.

is divided into three parts for ice (E), water (W) and steam (D) by three curves KN, KA, KS (ψ, ϕ, χ). Proceeding along one of the curves we observe the coexistence of two phases; at K that of three. If we cross over the curve KA from left to right, we encounter the melting of ice; in the contrary direction, the freezing of water. Analogously, the other two curves divide two phases from one another. Advancing along the curve NK beyond K, the further course depends upon accessory conditions. If, for example, a constant space contains much steam and little water, by cooling below K the water is converted into ice and the further process takes the course KS. If much water and very little steam are cooled below K the result is that the steam is suppressed by the freezing and a part of the water expands, while part of the water remains liquid by increase of pressure and lowering of the melting point. The process then takes the course KA. These physical considerations are very similar to those which Gibbs and others have set up on chemical equilibrium.[2]

2. Another question which illustrates the connection between thermo-

dynamical and chemical problems relates to the electromotive force of galvanic elements. Helmholtz and William Thomson started from the view that the work which the galvanic current can perform is the mechanical equivalent of the algebraic sum of the heats of combination and dissociation of the simultaneous chemical processes — the "heat-tone" ("*Wärmetönung*") of these processes. If V is the electromotive force (potential-difference) in mechanical measure, and the unit of electric quantity in mechanical measure flows off, then V is also the work that can be performed upon the unit of quantity. But, according to Faraday, to the unit of quantity there corresponds a definite amount of chemical transformation of substance in the galvanic element with definite heat-tone. The mechanical equivalent of this heat-tone is now, corresponding to the principles of energy, to be equal to the work V. For Daniell's element this is very nearly correct, and in this Thomson's or Helmholtz's rule for a long time found confirmation. But this rule would be correct in general only if every element were to operate without change of temperature, so that with the single exception of Joule's heat of the current, no other thermal change would enter. But according to investigations by F. Braun, Helmholtz, Jahn and others there are elements which become heated and others which become cooled. With such elements, according to Helmholtz[3] one can conceive a thermodynamical process carried out, and can apply Carnot's theorem to them.

Imagine two bodies of great capacity for heat, A of temperature $T + dT$ and B of temperature T. To start with, let a galvanic element be in contact with A and have the electromotive force $V + dV$. The unit of quantity of electricity ($E = 1$) is allowed to flow exceedingly slowly in the sense of the electromotive force, by which the current produces the work $V + dV$. If then the element is cooled to T, causing the electromotive force to sink to V, and if the unit of quantity is forced

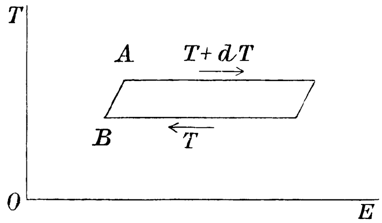


Fig. 104.

back while the element is in contact with the body B, then to this belongs the expenditure V of work. The element is again heated to its original state $T + dT$ and has then passed through a reversible cyclic process and has performed the work dV. Suppose now that the heat tone which corresponds to the unit of quantity is $Q > V$; then by conducting away the unit of quantity the element would become cooled by $V - Q$. By the isothermal process in connection with the body A, however, it retains its temperature and withdraws from the body A the quantity of heat $V - Q$. In bringing back the unit quantity of electricity under expenditure of work, however, the element must become heated by $V - Q$. In the contact with B, however, the process proceeds isothermally and the quantity of heat $V - Q$ is given up to B. Thus the transference of the quantity of heat $V - Q$ (in mechanical measure) corresponds to dV, the work performed, and therefore by Carnot's theorem we have the equation

$$\frac{dV}{V-Q} = \frac{dT}{T}$$

or

$$V - Q = T\frac{\partial V}{\partial T},$$

Thus it is seen that V increases with T if $V - Q$ is positive, and inversely. In fact, if V increases with T while $V - Q$ is negative, by carrying out the process clockwise (see Fig. 104) work is performed and, in addition, heat is transferred from B to A, from a lower to a higher temperature, which is inconsistent with Carnot's fundamental principles. The quantity of heat $V + dV - (Q + dQ)$, to be exact, is withdrawn from or supplied to the body A, while at B the corresponding gain or loss amounts to $V - Q$. As we know from previous investigations, however, this infinitely small difference does not come into consideration in comparison with the finite difference. Only when $V - Q = O$ do the above processes take place isothermally, without the help of the bodies A and B. This is true, very nearly, for Daniell's element.

The maximum work that can be performed (in the above case, the V of the equation) Helmholtz called the "decrease of free energy". We see that an equation corresponding to the last one given holds in general, if we reflect that the decrease U of the total energy of a system, in a reversible isothermal process is $W - Q$, where W denotes the work performed in the process and Q the heat supplied to the system from outside. If we complete this process so that it becomes a reversible cyclic process between $T + dT$ and T, Carnot's theorem $dW/Q = dT/T$ holds good. From the two equations we get

$$W - U = T\frac{\partial W}{\partial T}.$$

3. Helmholtz had given some developments which, it is true, are somewhat more general, but otherwise agree in essentials with the older ones of Massieu. Massieu showed that the quantities U and S, energy and entropy of a system, are related. If we suppose the heat measured in units of work, then for a process the supply of heat is

$$dQ = dU + pdv,$$

or since

$$dS = dQ/T,$$

$$TdS = dU + pdv.$$

The identity

$$d(TS) = TdS + SdT$$

then allows us to write

$$d(TS) - dU = d(TS - U) = SdT + pdv.$$

If, now, we introduce a new function:

$$H = TS - U,$$

then

$$S = \partial H/\partial T, \qquad p = \partial H/\partial v,$$

$$U = TS - H = T\frac{\partial H}{\partial T} - H$$

and these equations allow U and S to be expressed in terms of the function H. The relation between U and S is represented by the equation

$$\frac{\partial S}{\partial T} = \frac{1}{T}\frac{\partial U}{\partial T}.$$

Helmholtz's equations are obtained by putting $H = -F$. The quantity F is the free energy and the work performed corresponds to the decrease of the free energy.

We see that these developments only repeat in another form what is contained in the two fundamental formulae of thermodynamics. But Helmholtz's remark on the applicability of these fundamental formulae to the electrochemical processes of the galvanic circuit was important.[4]

CHAPTER XXI

THE RELATION OF PHYSICAL AND CHEMICAL PROCESSES

1. In recent times various relations between chemistry and physics have been submitted to investigation. The old idea of making chemistry applied physics — and particularly applied mechanics — thus received new encouragement. But if by this idea is meant that the laws discovered in physics will suffice without extension and generalization to allow us deduce all chemical processes, it appears to me scarcely less naive than that of Thales, which would deduce everything from the properties of water. It is not likely that a wider field of experience should be completely contained in a smaller one which is already known. Analogies between physical and chemical processes indeed exist, but they would have to be more comprehensive before we can believe in the identity of these two fields of experience. Some physical laws — the conservation of mass, the conservation of the quantity of electricity, the conservation of energy, the law of entropy, and so on — reach indeed over into chemistry; but an unprejudiced survey leads us to hold it possible that a chemistry of the future should include physics — rather than that such a physics should include chemistry.

The following considerations do not aim at a physical theory of chemical phenomena or a chemical theory of physical processes; they are intended merely to discuss, for the purpose of a provisional orientation, some questions in respect to the relation of the two domains.[1]

2. If we wish to know clearly the relation of the two domains, we must ask the question: In what do physical and chemical processes agree and in what do they differ? Older chemical writers see in chemical processes "*material*" changes. How is this to be understood? Lead and iron are "materially" different. Cold iron and hot iron are still "the same" body. But is not lead with a velocity of 500 m/s much more different in its behaviour from lead at rest, iron at 1700 °C from cold iron, sodium vapor from solid sodium, than iron and lead in the cold state from one another? Were we restricted to the sense of touch alone we certainly could not think lead at rest and lead in motion the same body. The sense

of heat alone would lead us to declare that cold and hot iron are different; the sense of sight alone would lead us to declare that solid sodium and sodium vapour are different bodies. What we call "a" body is just a *complex* of attributes which belong to different sense fields and "matter" is simply the representation to ourselves of the *connections* of this complex. In physical processes *one* property of the complex changes alone — or at least preeminently; in chemical processes, the *entire* complex changes.

The most stable characteristic of a body, which is not arrived at by direct sense-perception but by a system of motor sensory, and intellectual reactions, is its *mass* or if we do not wish to go so deeply into the question, its *weight*. Lavoisier first impressed upon us the fact that the sum of the masses remains unchanged in chemical processes, so that the gain in mass of one body is compensated by the loss in mass of the others. The idea that in chemical processes *given* bodies unite, penetrate one another or separate is strongly connected with what we have just mentioned, and is quite in conformity with the experiences of daily life about the behavior of physical combinations of bodies. Thus we believe that a *chemical* change occurs when a body absorbs or yields another with change of mass, and we are in the habit of regarding the so-called allotropic changes as *physical*. If, now, chemical processes are conceived as combinations and separations of radically different bodies in themselves *unchangeable* the tendency to purely mechanical explanation breaks out and spreads farther and farther until its supporters endeavor even to build up the "*qualitatively*" different elements out of one kind of fundamental matter. To this attempt the fact that there is in mechanical respects only one kind of mass (or matter) appears to furnish support.

3. Physical processes present numerous analogies with purely mechanical ones. Differences of temperature and electric differences equilibrate themselves in a similar way to the differences of position of masses. Laws which correspond to the Newtonian principle of reaction, to the law of conservation of the centre of gravity to the conservation of quantity of motion, the principle of least action and so on, may be set up in *all* physical domains. These analogies may be made to rest upon the assumption which the physicist is fond of making, namely, that all physical processes are in reality mechanical. But I have been for a long time rather of the opinion that we can discover general phenomenological laws under which the mechanical ones are to be classed as

special cases. Mechanics is not to serve for the explanation of these phenomenological laws, but as a model in form, and as an indicator in searching for them. The chief value of mechanics for all physical research seems to me to lie in this.

4. How is it with the relation of physics to chemistry? In physical processes, masses go over from one velocity level, thermal level, or electrical level, to another; or what amounts to the same thing, since at least two masses must take part in reaction, level values travel from one mass to another. Cold iron and hot iron equalize their temperatures; but cold iron and hot copper also behave in the same way. Here only *one* property is involved in the equalization. May we not imagine that in the domain of chemistry as well nothing further takes place than that masses — homogeneous throughout — of one *chemical* level go over to another chemical level?[2] But in order that such a *chemical potential* may not be a mere word, what it has in common and what not in common with a *physical* potential must be ascertained.

The chemical process $Na + Cl = NaCl$ is in fact, similar to the process of equilibration of temperatures between cold copper and hot iron. In the place of two masses with different properties the process yields the sum of the masses of the same nature throughout. But in the physical case only one property has to be equilibrated; in the chemical case on the contrary, this equilibration embraces all the properties. In the physical case, all intermediate stages between the initial temperatures and the temperature of equilibration are passed through; in the chemical case the equilibration takes place *discontinuously*.[3] In the physical case, any desired temperature of equilibration may be attained by variation of the reacting masses and the initial temperatures; in the chemical case the initial potentials can enter only in definite discrete values, and the possible potentials of equilibration likewise have only definite discrete values. If the reacting masses in the chemical case are not in a definite proportion, masses with unequilibrated potential remain over.

5. With a little forcing, the parallelism between physics and chemistry can be pushed somewhat farther. In the first place the apparent discontinuity of equilibration in the case of two conductors of different electric potential may be referred to. This equilibration is very similar to an explosion of detonating gas. Indeed, if a positively charged and a

negatively charged water bulb are respectively filled with hydrogen in excess and oxygen, and these charges are equilibrated by a spark when the bulbs come into contact, the process might be interpreted offhand as an explosion of detonating gas.

If we consider the chemical case:

$$\begin{aligned} &NaCl + AgNO_3 = AgCl + NaNo_3, \\ 228.5 = {}&58.5 + 170 = 143.5 + 85 = 228.5, \end{aligned}$$

we see formed out of two bodies with different properties two new bodies with new properties. If ice at the melting point is combined in suitable masses with say, liquid wax at its melting point, liquid water and solid wax result. But in the chemical case, a characteristic displacement of masses also takes place.

The fixed proportion of masses which is necessary in a chemical reaction inevitably begets the idea that the masses react upon one another part for part and not as wholes. A fictitious physical analogy may be easily produced. Let two masses m_1 and m_2 of the same specific heats, equilibrating their temperatures u_1, u_2, be in the fixed ratio $1:\mu$, that is to say we have $m_2 = \mu m_1$. Then the temperature of equilibrium U is a perfectly *definite* one:

$$U = \frac{m_1 u_1 + m_2 u_2}{m_1 + m_2} = \frac{u_1 + \mu u_2}{1 + \mu}$$

which lies μ times farther from u_1 than from u_2. If conversely between u_1 and u_2 only one definite level is possible, the reacting masses must likewise be in a definite ratio in order to give a pure reaction. The discrete level grades and the fixed mass ratios are thus interdependent. This fact obtrudes itself, so it seems to me, as such a striking difference from what takes place in physical processes that to close our eyes to it and to deceive ourselves by ingenious assumptions about its significance can hardly commend itself to us.

6. Thus, if we assume a chemical potential, we must admit that as distinguished from physical potentials it exhibits discrete grades which may again be passed through in reverse order by breaking up a so-called combination. The ratios of the reacting masses are definite ones related to those potential grades in such a way that, with the reverse displacement of the masses (resolution), the potential grades will be passed through in reverse order. We must admit that the atomic theory in

common use presents all this in a very simple and clear way. If we consider moreover that this view has also led to new discoveries by the analogy being further confirmed than was assumed at its first detection, the value the chemist attaches to it cannot surprise us. But all this cannot prevent us from stripping away the shell from the conceptual kernel of this theory, as we have done above, and from looking upon its chance and accessory additions as pictures which are not to be taken seriously. But we must demand of any new theory of chemical phenomena that it should accomplish at least as much as the atomic theory given up for it.

7. The discrete grades of the chemical potential could perhaps be explained by the instability of the intermediate states. But such a potential can hardly be conceived as a *simple linear manifold* like the familiar physical potentials. The fact that in chemical processes (changes of potential) a whole complex of properties varies makes this view difficult. The periodic properties of Mendelejeff's series also point to a multiple manifold: These properties cannot be represented in a straight line. If finally the chemical potential were a simple manifold, it would remain totally incomprehensible why from two widely distant elements in the potential series all the intermediate ones could not be represented. If however, we wish to regard the discrete grades of potential as merely apparent — as *mean values* of continuously graded potential values —, then we should have to ask why all elements lying between the component parts do not diffuse out of a compound.

We need not shrink from the supposition of a multiple manifold of level values. The familiar physical level values are it is true, level values of *work*; and, as such, simple manifolds. But though the square of velocity represents only a simple manifold, yet velocity, acceleration and so on, since they are directed magnitudes, exhibit a three fold manifoldness. These quantities are level values although not those which are alone taken into consideration by the modern principle of energy. But the principle of energy certainly cannot settle all physical questions. Electricity and magnetism stand to one another in much the same relation as real and imaginary quantities. If in this sense level values of both domains are regarded as belonging together they form a system of a six-fold manifoldness. Much the same might be the case with the chemical potential; but the fixed points would be wanting, to begin with, for determining the dimension number of the manifold.

8. From all the foregoing, it appears that chemical processes lie much deeper than physical ones. This also appears in the following way. Physical processes are subject to certain equations which present permanences of combination or relation of the elements entering into the equation. If a chemical change takes place, those equations are replaced by entirely new ones. The rules which would determine completely the transformation from one system of equations to the other would be the complete chemical laws, and compared with physics would represent permanences of a higher order.

Reference has been made elsewhere to the fact that sensations are the true elements of our world picture. Now, we cannot doubt the extremely close connection of sensations with chemical processes. If we have six fundamental color sensations, we assume that the albumen of our bodies may be transformed by optical excitation in a six-fold manner. All sensations, as also space sensations would admit of an analogous view.

And just as at present in stereochemistry we endeavor to explain chemical relations by spatial relations, it is quite possible that we may some day attain to the understanding of space, its dimension number and so on, by chemical paths. If bodies so different as sugar, permanganate of potassium, and arsenic taste sweet, it naturally does not imply homogeneity of these bodies but a similar transformation of the albumen brought into contact with them. There will be as many sensations of taste as there are kinds of transformation of albumen by immediate chemical action. For knowledge of the latter, rather than for characterising the compounds examined by taste, it would be worth while to work out a system of taste sensations similar to that of color sensations. Progress in this direction would considerably further the clearness of our world-picture. That chemical processes appear to be local and without action at a distance puts no serious difficulty in the way of a comparison with physical processes. The galvanic current with its electric and magnetic actions at a distance can, in fact, be conceived as a chemical process. Action at a distance cannot thus be looked upon as a characteristic difference of physical and chemical processes.[4]

CHAPTER XXII

THE OPPOSITION BETWEEN MECHANICAL AND PHENOMENOLOGICAL PHYSICS

1. The opposition indicated in the title to this chapter stood out more clearly and forcibly than ever before at the Congress of men of science (*Naturforscherversammlung*) at Lübeck in 1895. This opposition is, at bottom, the old opposition between Hooke and Newton. Yet it seems as though a reconciliation were quite attainable.

What impels us to a mechanical conception of phenomena and makes a mechanical explanation seem the natural one, has been brought forward in §§1—4 of Chapter XIII and §2 of Chapter XVIII. Anyone who has felt the value for research of a clear representation of a fact, will readily grant the usefulness, the means, of such ideas. Let us but reflect how very much the mere fact is enriched by what such an idea adds to it; how that fact gains, in imagination, new properties which incite us to experimental investigations, to inquiries as to whether the supposed analogy actually exists and how far it extends. Let us but recall the dynamical theory of gases, the progress which the study of the behavior of gases and solutions has made as a result of the conception of the processes as statistical mass phenomena, the investigations concerning the dependence of the velocity of diffusion, of friction and so on, upon temperature — to all of which this theory directly led. The liberty which we allow ourselves in assuming invisible, hidden motions is not greater, at bottom, than in Black's assumption of a latent heat.

2. While, now, I wish to emphasize on the one hand that every idea which can help, and does actually help, is admissible as a means of research, yet on the other hand it must be pointed out how necessary it is from time to time to purify the exposition of the results of research from the unessential ingredients which have become mixed with them by working with hypotheses. For analogy is not identity; and for complete understanding we must have, besides knowledge of the similarities and the agreements, knowledge of the differences as well.

When I try to do away with all metaphysical elements in the exposition of natural science it is not my opinion that all ideas which are meant to serve as images are to be put aside, if they are useful and are viewed merely as images. Still less is an anti-metaphysical critique to be regarded as directed against all fundamental principles which have hitherto show themselves to be valuable. We may, for example, quite well have strong objections to the metaphysical conception of "matter", and yet not necessarily have to eliminate the valuable conception "mass": we can retain this latter conception as I have done in my *Mechanics*, because we have seen that it signifies nothing more than the fulfilment of an important equation. I cannot agree, either, that the marvellous forces which people like to ascribe to the notions used in mechanical physics are now merely transferred to algebraic formulae, and that in place of the mechanical mythology we can simply substitute an algebraic one. The validity of a formula in like manner denotes an analogy between the operation of a calculus and a physical process. Whether this analogy holds or not, in each particular instance, has to be tested.

Sometimes the advocates of mechanical physics assert that they have never regarded their ideas as anything but images. This perhaps reveals a somewhat polemical disposition that is not altogether chivalrous. At some distant time, when the physicists now living have left the stage, a future historian will be able to show easily and without any chance of being contradicted by numerous quotations from eminent physicists and physiologists, with what fearful earnestness and naiveté these ideas were taken by the great majority of distinguished investigators of the present time; and how very few men of an individualistic tendency of thought are to be found on the opposite side.

3. Advantageous as the mechanical view of thermal processes has been in the past, there is, in a one-sided adherence to it, a certain prejudice which may be illustrated here by two examples. When Boltzmann[1] made the beautiful discovery that the second law of thermodynamics corresponds to the principle of least action, I was at first no less agreeably surprised than others. Yet there is no reason for being surprised. When once it has been found that quantity of heat behaves like *vis viva*, and thus an analogue of the theorem of *vis viva* is applicable to it, it is not to be wondered at that the remaining mechanical principles (which are not essentially different from this theorem) may also be applied in the theory of heat. The appearance of the expression $\delta \Sigma \int mv^2\, dt$ in Boltzmann's deduction ought not then to seem strange to us and certainly ought not to be regarded as a new proof of the mechanical nature of heat.

The mechanical conception of the second law of thermodynamics, by distinguishing between orderly and disorderly motions and equating increase of entropy with increase of disorderly motions at the expense of orderly ones, seems a very artificial expedient. If we consider that a true analogue of the increase of entropy in a purely mechanical system made up of absolutely elastic atoms does not exist, we can hardly help thinking that an escape from the second law (even without the help of Maxwell's "demons") would be possible if such a mechanical system should really be the basis of thermal processes. I agree entirely with F. Wald when he says: "In my opinion, the roots of this [entropy] theorem lie much deeper, and if the molecular hypothesis and entropy theorem be brought into harmony, it is a piece of good luck for the hypothesis but not for the entropy theorem."[2]

CHAPTER XXIII

THE EVOLUTION OF SCIENCE

1. Man is governed by the struggle for self-preservation: his whole activity is in its service and only achieves, with richer resources, what the reflexes accomplish in the lower organisms under simpler conditions of life. Every recollection, every idea, every piece of knowledge has a value originally only in so far as it directly furthers man in the direction indicated. The life of ideas reflects the actual facts, supplements partially observed facts according to the principle of similarity (by association), and makes it easier for man to place himself in more favorable relations to them. The more extensive the field of facts and the more truly that field reflected, the more exactly ideas are adapted to the facts, the more effectively helpful are those ideas in life. But only what most powerfully concerns the will, the interest (that is the useful), or what stands out strikingly from the frame of daily life (the new, the wonderful) will initially attract attention. Only gradually, from this point, are ideas able to adapt themselves to broader fields of facts. Here the continuous widening of experience, often resulting from chance circumstances, plays an essential part.

2. The enrichment and deepening of experience can be gained only by the division of labor which appears in organized society — a division which finally converts research itself into a special vocation. The temporal, spatial and professional narrowness of the individual circle of experience creates the necessity of *linguistic communication* for the widening of this circle. But the possibility of communication itself is based upon comparison of facts, which is effected naturally and spontaneously by memory. Communication is, essentially, a justification for the reproduction of facts in thought. The more extensive becomes the field of experience, knowledge of which we arrive at by communication, the more sparingly and economically must the means of exposition be used in order to subdue the material in hand with a moderate expenditure of memory and work. The methods of science are, therefore, of an economical nature. But, of course, we do not economize merely *in order to* economize; but in order to possess, and ultimately to

enjoy possession. The aim of scientific economy is to provide us with a picture of the world as complete as possible — connected, unitary, calm and not materially disturbed by new occurrences: in short a world-picture of the greatest possible stability.[1] The nearer science approaches this aim, the more capable will it be of controlling the disturbances of practical life, and thus of serving the purpose out of which its first germs were developed.

The significance of the motives here touched upon and their relations and connections is best shown by treating them separately in special chapters.

CHAPTER XXIV

THE SENSE OF THE MARVELLOUS

1. All incitement to inquiry is born of the novel, the uncommon, and the imperfectly understood. The ordinary, to which we are adapted, takes place almost unnoticed; novel events alone catch the eye and stimulate the attention. It happens thus that the sense of the wonderful, which is a universal attribute of mankind, is of immense import also for the development of science. It is the striking forms and colors of plants and animals, the startling chemical and physical phenomena, that arrest our notice in youth. Only when we compare these with the everyday run of things do we develop, gradually, the craving for explanation.

2. The beginnings of all physical science were intimately associated with magic. Hero of Alexandria made use of his knowledge of the expansion of air by heat to perform conjuring tricks; Porta described his beautiful optical discoveries in a work entitled *Magia naturalis*; Athanasius Kircher turned his physical knowledge to account in the construction of a "*magic* lantern"; and in the "Récréations mathématiques" or in Ensl's *Thaumaturgus*, the sole purpose to which the more phenomenal facts of physics were turned was that of dazzling the uninitiated. With the fascination exerted by remarkable events there was naturally associated, in the case of the person first discovering them, the temptation to acquire greater prestige by keeping them secret, to produce extraordinary effects by their assistance, to derive profit from their practice, and to gain increased power, or at least the semblance of such power. Some slight successful venture of this kind may then have kindled the imagination and awakened hopes of attaining an altogether extraordinary goal, resulting in the deception not only of others but perhaps also of the person himself. In this manner, for example, from the observation of some astonishing and not understood transformation of matter, there may have originated *alchemy*, with its desire to transmute metals into gold, to discover a panacea, and so on. The felicitous solution of some innocent geometrical problem is the probable foundation of the geomancy of the Arabian Nights, which divines futurity by means of numbers, as it was probably also of astrology, and

so forth. That "malefici et mathematici" were once mentioned in the same breath by a Roman law, is also intelligible on this theory.[1] Even in the dark days of mediaeval demonology with witchcraft, natural inquiry was not extinguished; on the contrary, it appears to have been invested, then, with a distinct charm of mystery and wondrousness, and to have become imbued with new life.

3. The mere happening of an extraordinary event is in itself not marvellous; the marvel is to be sought, not in the event, but in the person observing the event. A phenomenon appears marvellous when one's entire mode of thought is disturbed by it and forced out of its customary and familiar channels. The astonished spectator does not believe for a moment that *no* connection exists between the new event and other phenomena; but, not being able to discern a connection, and being invariably accustomed to such, he is led, in the nature of the case, to adopt extraordinary and fallacious conjectures. The character of these conjectures may be infinitely varied. But inasmuch as the psychical organisation of mankind, conformably to universal conditions of life, is everywhere pretty much the same; and since young individuals and races, whose psychical organisation is the simplest, are the most prone to surprise, almost the same psychological phenomena are repeated the world over.

4. Auguste Comte[2] examined the phenomena here referred to; and Tylor[3] subsequently studied them in the vast material which the ethnology of races of a lower culture afforded. The most striking and immediate occurrences in the natural environment of the savage are those of which he himself or his fellow creatures are the authors. He is conscious of will power and of muscular force in his own person, and is thus tempted to interpret every unusual phenomenon as the creation of the will of some creature like himself. His limited capacity to distinguish sharply his thoughts, moods, and even his dreams from his perceptions, leads him to regard the images of absent or deceased companions appearing in his dreams, or even those of lost or ruined objects, as real phantom entities, as *souls*. Out of the worship of the dead which here took its being there has sprung the worship of demons, of national deities, and so on. The conception of sacrifice, which is utterly unintelligible in modern religion, becomes comprehensible when we reflect on the continuous development from the funeral sacrifice. Men

were wont to bury with the dead the objects which their phantoms have most desired in their dreams, that the shades of the one may take pleasure in the company of the shades of the other. This disposition to consider all things as like ourselves, as animated and ensouled, is in the same manner transferred to useful or injurious objects generally, and leads to *fetishism.* There is a trace of fetishism even in the theories of physics. So long as we consider heat, electricity, and magnetism as mysterious and impalpable entities residing in bodies and imparting to them their known wonderful properties, we still stand on the level of fetishism. True, we invest these entities with a more stable character and do not attribute to them the capricious behavior which we deem possible in the case of living beings; but the point of view indicated is not entirely discarded until exact investigation by means of metrical concepts has taken the place of fetishistic views.

Failure to distinguish sharply between one's own thoughts and moods and the facts of perception, which is noticeable even in scientific theories to-day, plays a predominant part in the philosophy of youthful individuals and nations. Things that appear alike in the least respect are taken to be kindred in character and to be closely allied also in their nature. Plants that exhibit the slightest similarity with any part of the human body are held to be remedies for corresponding local disorders. The heart of the lion is supposed to augment courage, the penis of the ass to be a cure for impotence, and so on. Ample corroboration of these facts is afforded by the old Egyptian medical papyruses, the prescriptions of which are found in Pliny and even as late as Paulinus. Things that are desirable but difficult to obtain are sought after by the most fantastic possible combinations of ingredients which are difficult to obtain, as is amply demonstrated by the recipes of the alchemists. One need but recall one's childhood to appreciate, from personal experience, this manner of thinking.

The intellectual behavior of the savage is very similar to that of the child. The one strikes the fetish which, in his opinion, has deluded him; the other strikes the table that has hurt him. Both talk to trees as they would to persons; and both believe it possible to climb to heaven by high trees. The phantom world of fairy tales and the world of reality are not sharply distinguished for them. We know this condition from our own childhood. If we only reflect that the children of all ages are invariably disposed to harbor thoughts of this character, and that a goodly portion even of highly civilized peoples possesses no genuine intellectual culture but only the outward semblance of this culture; if furthermore we remember that there always exist men who derive profit from fostering the lingering relics of the views of primitive mankind, and that entire sciences of deception have even been created for their preservation, we shall clearly understand why these ideas have not yet died out. We may read, indeed, in Petronius's *Symposium of Trimalchio* and in Lucian's *Liars' Friend* the same blood-curdling stories that are told to-day; and the belief in witchcraft now prevalent in Central Africa is not a whit different from that which pestered our forefathers. The same ideas, slightly modified, are also found in modern spiritualism.

From manifestations of life in every respect similar to those of which we ourselves are the authors, the stupendous, the important, and the wonderfully convenient *inference* of another ego analogous to our own ego is drawn. But, as is the case with all thoroughly adaptive habits, this inference is likewise drawn where the premises do not justify it. True, the phenomena of the inorganic world do in a measure run parallel with the phenomena of the organic world; yet, owing to their simpler conditions, they are subject to laws of a far more elementary character. Something similar to will is doubtless existent there also, but the train of reasoning which invests trees and stones with all the attributes of human personality appears, at our stage of civilization, unfounded. Even the critically trained intellect also infers the agency of another ego in spiritualistic séances, but it is the ego of the conjuror and not that of a spirit.

Darwin, in his *Expression of the Emotions*, has abundantly shown that habits which were originally useful continue to exist even where they are useless or indifferent. And there can be no doubt that they also continue to exist where they are even injurious, provided they do not bring about the extinction of the species. The ideas above discussed are all based, in their elements, upon useful psychical functions, however monstrous they may have become in their subsequent development. Yet no one would think of saying that the human species has been preserved or even bettered by the human sacrifices of Dahomey, or by the rival persecutions of witches and heretics inaugurated by the Church. It simply is that civilisation has not yet perished from such baseless practices.

5. Should any one be prone to think that the foregoing discussions are no subject for a scientific public, he is mistaken; for science is never

severed from the life of the every-day world. It is a flowering of the latter, and permeated with its ideas. When a chemist who has achieved fame by fine discoveries in his science espouses spiritualism; when the same can be said of a physicist; when a renowned biologist, after expounding to us in cogent manner the grandeur of the Darwinian theory, closes with the statement that the doctrines he has set forth are applicable only to the organic world but not to the spiritual element in man and, again, openly professes spiritualism; when prominent psychiatrists show themselves perpetually to attribute extraordinary nerve power to every female quack — then it is certain that the intellectual malady of which I have here been speaking is very deeply seated, and not confined to the minds of the non-scientific public. The malady appears in the majority of cases to spring from a too one-sided intellectual culture, and from a lack of philosophical training. In this case it may be eradicated by a study of the works of Tylor, which exhibit the psychological *origin* of the views under consideration in a very lucid manner, and thus render them susceptible of critical scrutiny. But the situation is often different. A scientist may elevate his view of the fitful play of the atoms, which serves good purposes in limited domains, to the rank of a conception of the world. In that case it is not to be wondered at that sometime his conception may come to seem to him so barren, insipid and inadequate as to render it possible for spiritualism to satisfy his intellectual craving, or may even be a need of his heart. In such circumstances enlightenment will be difficult.

6. A few personal experiences, which are instructive enough to make public, will show how great the need for marvels is with some scientific men.

I was once in the university town of X, when several distinguished inquirers, whom we will call A, B, and C, were seized with this spiritualistic craze. The event was to me a psychological problem, and I resolved to take a nearer look at the situation. At the head of the group stood A, whom I had known for a long time. He received me kindly and showed me the wonderful results of his intercommunication with spirits, and also expatiated enthusiastically and picturesquely on the happenings at the séances. In reply to my question as to whether he had really observed closely all the things he described, he answered: "Well, the fact is that I did not myself see so very much, but you must remember that careful observers like C and D," etc. C in his turn said: "I should not have been so much convinced by what I saw myself, but you must remember that accurate observers like A and D were present, who subjected the performances to the most searching scrutiny," and so on. I believe we are justified in drawing no other inference from this vicious circle than that the miracle could count upon a friendly reception from all the members of this circle.

The chief curiosity which A showed me was an ivory ring which could be slipped on the centre leg of a round table by magic only; provided of course that the top of the table were not easily removable. That the top of this table could be readily removed I surmised from its appearance, and imparted my suspicion to another acquaintance of mine in the same town, remarking that A, with his pronounced predilection for the marvellous, had undoubtedly never once thought of investigating whether such was really the case. Years later, after A's death, I met a friend of his; the subject accidentally came up in our conversation. I was informed that while the celebrated table was being removed after A's death the leg fell off and the top remained in the hands of the movers.

Let the circle K of the annexed figure be pictured as performing a revolution in space about the axis GG, situated in the same plane with

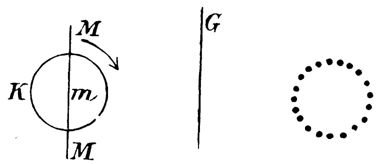


Fig. 105.

it, and conceive the ring thus described to be composed of vulcanized rubber. Then imagine a knife, MM, thrust through the ring, and conceive a point m of the blade to be carried in a circle round GG as axis, whilst at the same time the blade performs a complete rotation about m, say in the direction of the arrow. In this way, the ring will be cut into two component rings, locked within each other. Simony[4] described this beautiful geometric or really topological fact along with numerous others of kindred character. I once showed it to an acquaintance of mine, a professor of mechanics, who of course noticed at once that the two rings could not be separated without tearing them asunder. "But I am a medium", I said; and, concealing the two rings for a moment behind my back, I placed them separate and intact upon the table. I shall never forget my friend's amazement. All I had done was boldly and undisguisedly to exchange the locked rings for a pair of detached rings which I had in my pocket. The latter are readily obtained from the operation indicated above by first turning the blade of the knife one-half a revolution about *m* in one direction and then one-half a revolution in the opposite direction. The two pairs of rings are sufficiently alike to be easily confounded.

I wanted to show my friend how easy it was to be deceived, but his *penchant* for mysticism was not to be eradicated by my efforts. As a devotee of homeopathy, he found a corroboration of his views in the discovery that the merest vestiges of sulphuric acid were sufficient to effect the electrolysis of water, whereas pure water did not permit of electrolysis. He claimed to have been cured once of a serious affection of the lungs by *natrium muriaticum* (table salt) in minute doses, diluted in the ratio of 1 to 100,000. The remark that the accidental variations in the saline constituents of the food which he ate must have been many thousand times greater than the doses of his physician could not shake his opinion, which he doubtless carried with him to the grave.

There was once on exhibition in a certain city a girl "who had been struck by lightning, and who, in consequence of the stroke, ever afterwards gave forth electric sparks". She was not confined to one spot, but was free to move about at will. An old gentleman, Mr. S., an able professional man, was disposed to take the matter seriously, to the gratification of the proprietor of the show, who must have chuckled gleefully to himself and inwardly repeated the adage "difficile est satyram non scribere." Mr. S. persuaded me to go and see the curiosity. I recognized the sparks as those of a small Rühmkorff coil, but was unable to discover the connexions, despite the fact that I had brought along with me a cane covered with a strip of tinfoil. My laboratory mechanic, however, who was a versatile conjurer, lit upon the secret of the device after a brief inspection, and an hour later exhibited to the old gentleman his own little son as similarly lightning-struck. The old gentleman was delighted, but when shown the simple contrivance by which the trick had been effected, he cried out "No, that was not the way it was done!" and made off.

Of the common run of spiritualistic séances I will say nothing here. They afford abundant opportunity for observing the ingenuousness of the so-called "educated" public with its insatiable thirst for miracles; as well as the artfulness, cunning, and knowledge of human nature displayed by the conjuror. I have always felt on such occasions as if I had been transported among savages, though I was in the very heart of Europe.

7. The tricks of the spiritualists have been repeatedly imitated by prestidigitateurs and anti-spiritualists; and the methods have been revealed by which they can be performed. Many mediums have been exposed and have been found guilty of resorting to the tricks of the prestidigitateur. The *psychological* principles by which the prestidigitateur proceeds[5] are very simple. The psychological habit of regarding things which are at all alike as identical is turned to frequent account here, as in the rapid interchange of similar objects, or where the conjurer, assuming an expression of deepest sincerity, appears to perform movements which he does not perform, but which are then believed to have been performed. A second method is that of concentrating the attention upon a time or place where apparently the event of greatest importance is taking place, whilst in reality that event is being enacted at a different time and a different place. An excellent example of the effectiveness of this method is afforded by the well-known question: "Which is correct, 7 and 9 *are* 15, or 7 and 9 *is* 15?" The person addressed, having his attention diverted to the grammatical form of the sentence, seldom notices, at first, the arithmetical error.

But explanations of this character have no weight with devotees. The tricks which conjurers perform by natural methods are performed for them by spirits, by supernatural methods. Newton's rules of admitting only true causes for the explanation of phenomena, and of not assuming more causes than are necessary for explanation, of explaining like phenomena everywhere by like causes, appear to be unknown to these folk. On the other hand, many persons to whom spiritualism is instinctively repulsive or who stand in fear of its practical consequences, do not always assume the correct attitude. They frequently characterize spiritualism as a "superstitious belief" and recommend as a preventive against it "the true belief." But who is to decide which belief is the true one? If such a decision were possible, it would be wrong to speak of "belief"; we should then rather have to speak of "knowledge". History arouses our apprehension here. For compared with the atrocities with which the extravagant outbreaks of various "true beliefs" have in times past blessed us, the consequences of spiritualism are, by virtue of their private character, the merest pleasantries. It would be inadvisable, accordingly, to drive out the Devil by the hand of Beelzebub. The preferable course would seem to be to regard that alone as true and acceptable from a scientific point of view which admits of proof, and to entertain in practical life and in science only such suppositions as sound and sober criticism grants a high degree of probability.

8. The error of that widespread movement of modern thought which fosters spiritualism along with other aberrations does not consist of the undue attention which it devotes to extraordinary phenomena, for these the natural scientist may not neglect either. Indeed, it is almost invariably extraordinary phenomena, like the attraction of small particles by rubbed amber or the adherence of iron filings to certain ores, that direct us in their subsequent development, to results of the greatest importance. The error is also not to be looked for in the belief that our knowledge of nature is not exhaustive and definitive. No natural scientist will imagine for a moment that new discoveries of great import are impossible, that fresh and undreamed-of relations between the facts of nature may not still be revealed. The error of these people lies rather in their uncritical pursuit of miracles as such, and in the childish and unthinking delight which they take in contemplating them, which produces chronic insensibiity to what is genuinely marvellous and worthy of investigation.

Do not far greater marvels encompass us in reality than the pseudo-miracles that the spiritualists offer? They can lift themselves upon a chair in the dark; but we are able, in broad daylight, before the eyes of all and by means known to all, to raise ourselves thousands of yards into the air. We can speak with a friend many miles distant just as we can with a person at our side, and this by the aid of a spirit who does not capriciously conceal himself or act the miser with his powers, but who has freely revealed to us those powers and placed them at our disposal. A three-edged piece of glass enables us to determine the composition of objects millions of miles away. By means of a few magic formulae, which are concealed from no man, our engineers discover how a waterfall can be compelled to illuminate our town, by what means steam can be made to draw our burdens, how mountains can be tunneled and valleys bridged. A talisman of heavy metal in my pocket, which every man can acquire by labor, gains for me, by a remarkable understanding on the part of all spirits, a kindly reception everywhere in the world.

Even when alone in my own study, I am still not alone: spirits still stand ready at my back and call. A problem perplexes me; I reach out now for this and now for that volume, and suddenly I observe that I have taken counsel of the dead. Galileo, Newton, and Euler have helped me. I too can call up the dead. And when I bring to new life in myself some great thought of Newton, or develop that thought to remoter consequences, then I have called up the dead in a far different fashion from the spiritualists, who can extract nothing from their ghosts but the expression of nonsensical commonplaces.

Are not these far more stupendous miracles — miracles which have actually transformed the world? But they have their drawbacks. Their employment is fraught with far more toil than making one's hair stand on end in a darkened room; and it is certainly far less alluring, since by common belief, anyone has a chance of becoming such a medium.

9. But the mere taking note of what is extraordinary is not the sole factor by which our knowledge of nature is advanced. There is requisite, in addition, resolution of the extraordinary into the ordinary, the elimination of the miraculous. The two operations, however, need not be combined in any one person or in any one period. The alchemists, while proceeding altogether uncritically, made some remarkable observations which were subsequently put to good use. And the possibility is also not excluded that the modern inquiries into miracle-working may unearth some valuable results. Attention has again been called by this movement of thought to the almost forgotten arts of hypnosis and suggestion; why should not something more of that character and perhaps of greater moment be brought to light?

There can be no question of good observations and results of course so long as this domain, which requires the nicest critical discernment for its exploration, remains the resort of credulous and uncritical minds. One is confronted every day with the results that are forthcoming when people are determined to see only what is remarkable, and care naught for criticism. Once while a student, I visited Baron von Reichenbach, the famous investigator of "od". According to his frank confession, he himself saw absolutely nothing of the wonderful phenomena which he so minutely described, but obtained his information solely from the persons upon whom he was experimenting. One of these persons, Frau Ruf, confessed to Fechner after Reichenbach's death that the statements of her experiences had been wheedled from her by cross-examination. I gained an ineradicable impression of Reichenbach's method from the following experiment: Passing a ray of light through a piece of Iceland spar, he split it into two parts, each of which was directed into a glass of water; the water of one of these glasses became in this manner "od-positive" and that of the other "od-negative"; but it seems never to have occurred to him that the "od-positive" water would have been changed into "od-negative" by simple rotation through 90°.

We shall not judge the "method" of the spiritualists too severely if we compare it with the method employed by many psychopathologists and neuropathologists. When we are told by a physician that a person has been made by suggestion to see an elephant upon a piece of blank cardboard we may let it pass; but when we are told that the same person picked out the same piece of paper from a packet of similar empty sheets, and saw the elephant upon this sheet only, and saw it inverted when the sheet was accidentally inverted, saw it magnified through an opera glass, and reduced in size when the opera glass was turned about — then this "scientific" statement taxes our credulity rather too severely. Why not rather say everything is possible, and give up all further investigation as unnecessary?

Constant appeals to our ignorance and to the incompleteness of our knowledge, which is denied by no genuine inquirer, are characteristic of the methods of the professional miracle-seekers of "occultists". But the conjectures which may be built upon our ignorance are infinitely numerous, while those which are built upon our knowledge are as a rule but few. The latter are accordingly alone qualified to serve as starting points for further investigation. While the miracle seekers see in the incompleteness of our knowledge the possibility and necessity of an extraordinary and phenomenal extension of knowledge, the obscurantists both within and without science base upon this incompleteness their claims for casting doubt upon the actual results which have been already obtained. How often have we been obliged to hear that the Darwinian theory is still nothing more than a hypothesis, to the demonstration of which much is still lacking; and this from people who would fill up the gaps of science with the relics of fog which they have carried with them from their childhood days and which, for them it would seem, is no hypothesis. The result of this procedure is in both cases the same, the substitution of chimerical illusions for sound, productive knowledge.

The *observation* of singularities in nature does not alone constitute science; the *elimination* of them is also part of it. So long as a person sees something remarkable in the saving of power accomplished by the lever, so long as he regards it as an exception and, deceiving both himself and others, sets to work to construct a perpetual motion machine on its principles, — then that person still stands upon the level of the alchemist. Not until he has perceived with Stevinus that the "marvel is no marvel" has he made a real scientific advance. In the place of intellectual intoxication now comes the delight which springs from logical order and from the intellectual penetration of what is apparently heterogeneous and manifold. The propensity to mysticism often appears clearly enough even in the exact sciences. Many a bizarre theory owes its origin to this propensity. Even the principle of energy is not without a mystical coloring in some of its forms. And, to take a commoner instance, with what satisfaction are not people often heard to remark upon the marvels which we can accomplish with electricity, without ever knowing what electricity really is? What else, pray, can electricity *be* than the totality of the facts in question, which we know and of which, as Popper[6] has aptly said, we *hope* to know still more? This state of affairs may serve as an excuse for our having illustrated the propensity to mysticism with somewhat drastic examples.

CHAPTER XXV

TRANSFORMATION AND ADAPTATION IN SCIENTIFIC THOUGHT

1. This is not the first time I have referred to the fact that ideas, and especially scientific ideas, are transformed and adapted in the same manner as that which Darwin supposed to be the case for organisms. This view, suggested in a lecture[1] first published in 1867, runs throughout my *Mechanics* of 1883, and was discussed in detail in an address[2] delivered on the occasion of my assumption in 1883 of the Rectorate of the University of Prague. I have no doubt that others have noticed the same circumstances, though it happens that no such instance has come to my knowledge. Indeed, Darwin's idea is too significant and of too wide a range not to influence all domains of knowledge. On account of its connection with the present subject, I am obliged to discuss it once more; and here I will lay aside the unnecessary adornments of the address just referred to and amplify the more essential parts.

2. Darwin must be adjudged the same significance in the domain of biology that is conceded to Galileo in that of physics. The same fundamental significance that belongs to the elements "inertia" and "acceleration" perceived by Galileo must also be ascribed to Darwin's "heredity", and "adaptation". The disposition to heredity seems at first glance to contradict the possibility of adaptation, and in fact the stability of the species due to heredity has been repeatedly urged against the Darwinian theory. In my address mentioned above, I have denoted the property of organic nature discovered by Darwin as "plasticity", but since then it has been brought home to me that this expression is not quite suitable. For a long time, the dynamical equilibrium of an organism has been compared to a waterfall, which retains its *form*, while its *material* continually changes, there being an inflow and an outflow at every one of its points. In reality, organic substance is essentially distinguished from inorganic substance in that the latter simply yields to outer physical and chemical influences while the former seeks to preserve itself in a definite state. This striving after "stability" has been emphasized in modern times by Fechner,[3] Hering,[4] Avenarius,[5] and Petzoldt.[6]

Despite the struggle of organic substance for preservation of a definite state, this state is nevertheless certainly influenced and modified and altered by outer forces, and this fact perhaps overcomes the apparent contradiction in Darwin's assertions. It may be mentioned that Boltzmann,[7] without being acquainted with the views just mentioned, demonstrated that a physical system when left to itself, gradually goes over into "more probable states" and finally into the "most probable state". Closer consideration shows that this "most probable state" is at the same time the most stable. Thus, an organic body seems to be distinguished from the general physical case in that it represents from the very beginning a system of considerable dynamical stability whose species is only slightly changed by outer conditions, if these conditions are not too powerful. We might, with Petzoldt, call the transference from one state of equilibrium to another by outer circumstances "development" — and this is transformation or adaptation.

Without wishing to decide the question here, I should like to present a somewhat different view of my own. It seems to me that we fall into a kind of Aristotelian physics when we ascribe to organisms a struggle for "stability", "variability", and so on. What would we say if, for example, such struggle were attributed to a heavy body? Forces drive the heavy body *downwards;* according to outer circumstances, it will change its state or, if it is disturbed, *return* to the same state — and thus, in the latter case, show stability. So I believe that it is sufficient to assume that the forces of the organism impel it in a certain direction — towards a certain state — which is attained more or less according to outer circumstance. For this impulse are made the sacrifices of adaptation to change of circumstances. To me this is probable when I consider the slight differences of blood-temperature, chemical constitution, and so on, of the higher vertebrates in connection with the great variations of outward form which they have undergone for the sake of outer circumstances. Only when we have to do with a *conscious* struggle (which takes into consideration a succession of situations) can we correctly speak of a "tendency to stability".

3. Thoughts are not *separate* organisms. Yet thoughts are manifestations of organic life. And if Darwin's view is correct the *trait* of transformation and development must be perceptible in them. Indeed, Herbert Spencer applied the theory of development to psychology even before Darwin did; and regarded the whole psychic development as

a phenomenon of adaptation. We see scientific ideas become transformed, spread over wider fields, contend with rivals, and triumph over less capable ones. Every learner can observe such a process in his own mind.

A member of a savage tribe knows how to find his way perfectly in the small circle of his nearest wants, but falls into perplexity and danger of misinterpreting any natural phenomenon to which he is not used or any product of the technical arts. Such a danger exists in a less degree for civilized man. His ideas are adapted to a larger sphere of experience and this adaptation makes it possible for society to relieve him of a considerable portion of care about subsistence, and frees him from the necessity of looking only after his most immediate needs.

When we move in a definite circle of facts which recur with uniformity, our thoughts at once adapt themselves to the surroundings in such a way that they involuntarily reflect them. A stone pressing the hand falls to the ground when left to itself not only in reality but also in our thoughts; iron flies to a magnet in our notions as well as in the experiment, and is heated at the fire in our imagination as well as outside us.

The power that impels us to the completion in thought of partially observed facts, is *association.* It is greatly strengthened by repetition. Then it seems like a power outside our will and independent of particular facts, which impels thought *and* facts, and keeps both in harmony; like a law governing both.

That we consider ourselves able to prophesy by means of such laws proves only adequate uniformity in our surroundings, but by no means establishes the *necessity* of fulfilment of the prophecy. In fact, we must always wait for the fulfilment before we can be sure of it. And failures of prophecy can always befound, though they are small in fields of great stability like astronomy for instance. The simpler a domain of facts is and the more familiar it has become to us, the more strongly does the belief in *causality* force itself upon us. But in domains new to us our gift of prophecy forsakes us; and we are merely left with the hope of soon becoming intimately acquainted with this domain too. How far a subjective conviction is from being a guarantee of the truth of the conviction may be seen when we think of the constructors of a *perpetuum mobile.* These people sacrifice their fortunes and their whole lives to their conviction. We cannot be more strongly persuaded of the truth of the best verified laws of physics than these inventors are of their would-be inventions.

4. What happens when the sphere of observation to which our thoughts are adapted is enlarged? We often see a single heavy body fall when its support gives way. We see also that lighter bodies are forced upwards by heavier ones which are sinking. If at some time or other we suddenly become aware that a light body is lifting a heavier one — with a lever for example — a peculiar intellectual situation arises. The powerful habit of thought according to which the heavier body is the stronger asserts itself. The new fact likewise demands its rights. In this conflict of thoughts and facts lies that which we call a "problem". In order to solve the problem, the habit of thought must be so transformed that it is adapted to both the old and the new cases. In our case this was done by adopting the habit of thought of regarding not only the weights but also the possible depths of falling or the product of the two, the *work,* as determinative of the motion. When we consider this, we understand why neither the child, who still lacks fixed habits of thought, nor the old man, living as he does a life that is practically closed up, knows any problems: problems arise for those only whose experience is in process of expansion. If the problem comes to full consciousness, and the transformation of thought takes place voluntarily and intentionally we call the process "research". We now see why from what is new, or from the chance occurrence of what is unusual, problems arise which throw a new light on our daily experiences. Once again, we see why science is a natural enemy to the marvellous, for science can only solve its problems by destroying the miraculous through explanation.

Let us now consider in detail, a process of transformation of thought. The sinking of heavy bodies seems commonplace and self-evident. But if we notice that wood floats upon water and flame and smoke rise in the air a contradiction to these facts comes into action. A well-known and ancient theory tried to include these new facts by attributing the thing most familar to man, the will, to inanimate bodies, and said that each thing *seeks* its *place*, heavy bodies below and light ones above. But it soon appeared that smoke itself has weight that it too seeks its place below and that it is only forced upwards by the air striving to descend, just as the wood is forced upwards by the water because the water is stronger. Thus, all bodies seek their place below, and *specific gravity* becomes a co-determining, new and decisive characteristic on which we base our expectation of the behavior of several bodies.

We see, now, a body thrown upwards. It rises. Why does it not now seek its place? Why does the velocity of its "forced" motion decrease while that of the "natural" motion of falling increases? Galileo, following both facts attentively, saw in both instances the same increase of velocity towards the earth. By this perception the problem was solved. Thus, not a place, but an acceleration towards the earth is assigned to bodies.

By virtue of this idea, the motions of heavy bodies become perfectly familiar. Adhering to the new habit of thought, Newton saw the moon and planets moving like projected bodies but yet with peculiarities, which compelled him again to modify a little this habit of thought. The heavenly bodies or rather their parts do not maintain a constant acceleration towards one another, but they "attract" each other with forces varying inversely as the square of their distances from one another and directly as their masses.

This idea, which includes that of terrestrial heavy bodies as a special case, is very different from the original one. The original one was very restricted, and this new one contains a profusion of facts. Yet there still lurks in "attraction" which really only serves to express the direction or *sign* of the acceleration, something of the "search for place". It would indeed be unwise timidly to avoid this notion of attraction because it carries with it an intimation of its pedigree; for it leads our thoughts in paths long familiar, and clings to our thoughts as possibly the historical root of the Newtonian view. Thus the most significant ideas gradually arise by the transformation of those already existing. Newton's idea, which is the flower from the seeds sown by Copernicus, Kepler, Gilbert, Hooke, and others, is very suitable for illustrating this process. This process of transformation consists of two parts. On the one hand it consists in finding new identical characteristics in apparently different facts; for example, *all* bodies are heavy, ascending and descending bodies maintain the *same* acceleration, the moon is under the influence of gravity just like a stone. On the other hand it consists in noting distinguishing characteristics in facts which have not hitherto been held to differ; for example the acceleration of two bodies to one another is not constant but is determined by their masses and distances from one another. In this way it becomes possible on the one hand to comprehend a constantly enlarging field with the same kind of habit of thought; and, on the other hand, to make variations of the habit of thought correspond to our distinctions of facts in the field.

5. The development considered is only a special case of a universally distributed biological process. Lower animals, acting merely by reflexes

swallow whatever happens to be in their reach and exercises the appropriate stimulus. With more highly developed animals with individual memory, a small part of the optical or acoustic sensation associated with the sensation of taste is sufficient to release the motions for seizing their prey. But in just these animals the relations between the former sensations and the released motions must be variously modified in course of time. For instance, an animal snaps at anything that buzzes. It happens to be stung by a wasp. After that habit urges it again to snatch at a buzzing creature, while a particular memory warns against it. In this conflict of impulses lies a problem, a burdensome and useless physiological pressure, which only gives way when the distinguishing characteristic of stinging and non-stinging insects is firmly fixed in the memory, and, coming in as a determining factor in addition to the characteristic of buzzing, acts decisively in releasing the motion of snapping. The motion is then determined unambiguously in every case. The habit formed at first is preserved as far as possible and is modified only so far as necessary for assuaging and removing the conflict with new experiences.

An animal acting on an optical stimulus which was associated with the sense of taste, seizes something unpalatable. On repeating this procedure the habit is formed of giving heed to those sensations of taste or smell which are common to the most varied objects of food without getting confused by the disparity of optical sensations. Quite analogous are the processes which take place in the adaptation of thought.

Such processes of adaptation have no discoverable beginning, for any problem which furnished the stimulus to a new adaptation presupposes a habit of thought already fixed. But neither have they any visible end, so long as experience has none. Science stands thus in the midst of the natural process of evolution, and she can guide evolution in the proper direction and help it along, but never *replace* it.

If we glance over the history of a thought which has grown familiar to us, we are no longer able, as a rule, to estimate correctly the whole value of its growth. What essential organic transformations in the intellectual reflex have come about we know from the scarcely conceivable narrow-mindednes with which great contemporary investigators sometimes oppose one another. Huygens's wave-theory of light was incomprehensible to Newton, and Newton's gravitational theory was incomprehensible to Huygens. The mighty and spontaneous transformations of habits of thought which are effected by pioneers presuppose, on the one hand, the naiveté of the child and, on the other, the mental energy of the mature man; the latter being unamenable to outside influences.

6. The modes of thought established by older habit, seeking to maintain themselves against new occurrences, push themselves into the reading of every new experience, and are thus caught in the inevitable transformation. The method of explaining by theoretical ideas or hypotheses new phenomena which are still not understood is based essentially upon this process. When, instead of forming entirely new notions about the motion of the heavenly bodies or the phenomena of the tides, we imagine the parts composing bodies to be heavy with respect to one another; when we imagine electric bodies to be charged with attracting and repelling fluids or the insulated space between them to be in elastic tension — we replace, as far as possible, the new ideas by obvious ones which have long been familiar to us, and which either run their courses without further trouble on our part or must be recast. Just so, an animal cannot form new organs for every new function its destiny confers upon it: it must utilise those it has already. A vertebrate, when it wants to learn to fly or swim, does not grow a third pair of extremities for this purpose; it reorganizes one of those pairs it has already. Only an unfortunate imagination could picture flying men with six extremities.

The rise of theory and hypothesis is thus not the result of an artificial scientific method but it reaches back into the childhood of science and proceeds quite unconsciouly. However, these constructions become dangerous to science as soon as they are held more dear and their content more real than the facts themselves.

7. The widening of our intellectual horizon, whether due to nature actually changing her face and presenting us with new facts or only to an intentional or involuntary change of view on our part, induces a transformation of our thoughts. In fact, the many methods of scientific enquiry, of intentional adaptation of thought, enumerated by John Stuart Mill — of observation as well as of experiment — may be recognized as forms of one fundamental method, the *method of variation*. The investigator of nature learns by accidental or intentional change of circumstances. But the method is by no means confined to the investigator of nature. The historian, the philosopher, the jurist, the

mathematician, the aesthetician and the artist clarify and develop their ideas by bringing forth from the rich treasures of memory different and yet similar cases by observing and experimenting in thought. Even if all sense experience should suddenly end, the events of earlier days would come together in our consciousness in various combinations; and that process would continue which, in contrast to the adaptation of thoughts to facts, belongs to theory proper, the adaptation of thoughts to one another.

8. Often enough thoughts are only incompletely adapted to facts. In that case, if the thoughts are in conflict, there arises an opportunity for adapting them to one another. In this way, Ohm's not altogether opposite notions of a stationary electric current were adapted by Kirchhoff to electrostatic ideas by substituting electric potential for electric density. Galileo, in his mental picture of uniformly accelerated or retarded motion ascending or descending on an inclined plane, changed the inclination of this plane, and thus, by experimenting in thought, attained to the insight that the notion of a decreasing velocity of a body moving upon a smooth horizontal path is inconsistent with the former idea. To restore harmony, he substituted for the traditional notion the idea of *inertia.* In every deduction, an idea is first found so as to adapt itself to — that is, agree with — those already given, or the agreement of two ideas already given is recognized. Processes of discovery almost always move by a series of changes between adaptations of thoughts to facts and of adaptations of thoughts to one another.

A very remarkable process takes place when the idea of a clear and simple case is consciously and intentionally adapted to the general impression, involuntarily and instinctively acquired, concerning the behavior of a large domain of facts. Stevinus's reasoning on the chain, Galileo's and Huygens's considerations on the connection between velocity and space fallen through, Fourier's bringing of the special laws of radiation into agreement with the notions on mobile thermal equilibrium, and the deductions from the principle of the excluded *perpetuum mobile,* are examples of this process.

The method of variation introduces us to instances of the same kind of facts, which contain both common and different parts. Only by *comparison* of different cases of refraction of light with varying angles of incidence can that which is common to all these cases, — the constancy of the index of refraction — emerge; and only by *comparison* of the refractions of light of different colors can the inequality of the indices of refraction attract attention. Comparison occasioned by variation leads the attention to the highest abstractions and to the most delicate distinctions.

9. The English investigator Whewell rightly affirmed that in the development of science two factors must work together: ideas and observations. Ideas alone lead to fruitless speculation; observations alone produce no organic knowledge. We see, in fact, that all depends upon the ability to adapt notions already existing to new observations. Too great pliancy towards every new fact permits the formation of no fixed habit of thought whatever. Too unyielding habits of thought become hindrances to free observation. From the conflict or compromise of our judgment with prejudice, if we may say so, in this startling way, our knowledge grows. Our entire psychical life, and especially our scientific life consists in a continual revision of our notions.

To those who are sceptical about the Darwinian theory, the observation of their own mental development cannot be sufficiently recommended. Thoughts are organic processes. The alteration of our mode of thinking is the most delicate reagent for our organic development that there is, and this development, when we regard it from this point of view, is immediately certain to us. Whoever considers the conduct of two individuals of different experience under the same circumstances will no longer doubt that every individual experience and every recollection leave their *physical* trace in the organism. Thus our whole scientific life appears to us as one side merely of our organic development.

CHAPTER XXVI

THE ECONOMY OF SCIENCE

1. I have shown at length in other writings[1] that the scientific and methodical presentation of a domain of facts has the advantage over an accidental and unsystematic view of the same domain in a more sparing or more economical utilisation of our mental powers. I would not return to this subject here if various objections which have been made against this thesis did not drive me to give some further explanations.

Without concerning myself with the details of such objections, which would never have been made if the trouble had been taken actually to follow my exposition and which would vanish of themselves if this had been done, I will here begin by premising a general remark: obviously we do not economize in order to economize, but in order to possess or to enjoy. The *methods* by which knowledge is built up are *economical* in nature. What use is made of the knowledge acquired, whether it serves merely for dispelling intellectual discomfort or for aesthetic gratification, whether it is turned to further account in science or engineering, or whether it is misused — has nothing whatever to do with the nature of scientific methods. It is expressly with regard to these methods that I made my assertion, and to that assertion I still hold.

2. Objections based upon a serious consideration of the subject, and which are thus helpful even though we cannot wholly agree with them we owe to Petzoldt.[2] Petzoldt finds the idea of economy inappropriate to physical as well as to intellectual domains, and is of the opinion that, in both domains, merely a tendency to *stability* is manifested. In the physical domain, I have myself rejected[3] the idea of economy, but I cannot agree with Petzoldt with regard to the mental domain.

In the first place let us indicate Petzoldt's point of view by some sentences of his:

Economic phenomena present two sides to us. Either we fix our eyes upon the end in view and note that it is attained with the most limited means or with the smallest expenditure of force; or we set out from the consideration of means or forces as the case may be, and observe that they accomplish as much as possible .... Thus, given forces cannot perform more at one time than another .... Leaving out of account the differences of our own influences, given forces or tendencies can attain only *one* stationary final state and consequently can be used perfectly conveniently or economically in only *one* way .... From the principle of continuity we cannot get minimal aspect. We can as little combine the notion of economy with the variation of an idea or the retention of a thought in the face of new impressions as we can speak of a minimum in the alteration of the magnitude and direction of one force by another in connection with the parallelogram of forces .... Thought does not need to "mirror" the world at all, it is not intended to do this, and there is no need for it to do so. Its *purpose* is to enter into a stable relation with things and processes. But its mere existence is, like all existence, *purposeless* and merely a matter of fact ... convenience, and the economy associated with it, is intelligible only as the result of a development.

The conclusion of Petzoldt's work reads: "Not maxima, minima and economy, but uniqueness and stability are brought into relief by those aspects of reality which must stand in the foreground of our interest."

I will not speak here of the principle of stability but only of economy. I, as well as Petzoldt, am convinced of course that in nature only that and so much happens as can happen, and that this can happen in only *one* way.[4] In this sense, then, economy cannot be predicated of physical processes, since there is no choice between the actual happening and another. For this very reason I have not used the notion of economy in any way in this domain. In so far as a mental event is a physical event, it also can take place, in the circumstances operating at that moment, in only one way.

But even in a technical — and thus physical — domain the way of looking at things changes radically, as soon as there is a definite *purpose* in view. A particular steam engine can, it is true, work, under given conditions, in only one way. But it will the better serve its purpose the more completely it realizes Carnot's reversible cyclic process. And of different steam engines, that particular one will be more economical which attains this ideal more nearly. Here the question is not about the absolute consideration of one definite event, but about the relative consideration of different events with respect to a purpose. It is not about what happens, but about what is *intended* to happen.

Science presents an analogous case. If, in the widening of experience, the corresponding adaptation of thought had at once to take place in the only possible and most perfect way, just as in a physical case all forces present work together in one definite way, then there could be no question of economy here either. But different men, side by side and following one another, will effect adaptation in different ways. One will overlook this, another that. Perhaps a hundred years elapses before wrong tracks are avoided and the right ideas appear. Then we can compare these different scientific attempts, much as we compare the steam engines spoken of above and find *one* more economical than the *other*. Economy will give us a most valuable point of orientation from which to direct our scientific actions, just as it does to technicians; and we shall be better off if we use it than if we simply abandon ourselves unconsciously to the psychical forces of the moment. It was for *this* reason I advanced my point of view.

Kepler, in his approximate law of refraction ($\alpha/\beta = n$) had everything in hand for setting up the dioptrics of Gauss. Nevertheless, even after Kepler's time, many did not do this. We can trace the path of every ray separately through all the refracting surfaces and so find everything necessary. The homocentric law makes matters very much easier to work with. But Gauss set up the two principal planes and the two principal foci once for all, and did not concern himself any more about the separate refracting surfaces however numerous they might be. Thus it is not correct here to say that with the given means only one final result can be attained in one way. Mental work may, as regards the particular, be wasted just as heat can be lost so far as mechanical work is concerned in the steam engine. Let this one example be sufficient.

I cannot agree with Petzoldt when he says that thought does not wish to mirror the world. In order to place ourselves in any relation to our surroundings we need a picture of the world; and to obtain this in an economical way, we cultivate science. I hope that Petzoldt will not withhold his assent from these explanations.

3. My assertion that the economy of science has the advantage over every other economy, in that no one suffers a loss by it, has also given occasion to a discussion. Dr Paul Carus,[5] maintains in opposition to me, that *no* systematic economic enterprise entails loss for others. I willingly admit that, for example, an industrial enterprise in an unfruitful region creates values which previously were not there, and that others besides the man who undertakes the enterprise benefit; moreover methods are discovered in this way which are useful to *all*. But with regard to material goods the amount of which cannot be increased by any industry beyond a certain measure (land and its proceeds) it must be granted that what one obtains possession of what others must necessarily lack. But new thoughts have the fortunate peculiarity that they are

not bound down in this way; and so anyone can possess them without others being deprived of them on that account. Of course science can also be misapplied in harmful enterprises. But there, the question is no longer of the economy of science but of an application of it.

CHAPTER XXVII

COMPARISON AS A SCIENTIFIC PRINCIPLE

1. The foregoing considerations have shown that every new notion is in a kind of opposition to those already present, and that those already present are not uninfluenced by the new one. This leads to a comparison of the old with the new, and this comparison makes its appearance quite spontaneously even in isolated observers and thinkers. But comparison gains greatly in significance from another circumstance.

The only immediate source of scientific knowledge about nature is sense-perception. But the result of this, in view of the spatial and temporal limitations to any individual's sphere of experience, would remain only very meagre if each one had to begin anew. Science can grow vigorously only by the fusion of the experience of many men, by language. Linguistic communication arises when sounds — at first involuntary sounds — become associated with commonly observed facts, phenomena in the outer or inner world. Afterwards these sounds become changed to voluntary signs of these facts. By means of these sounds it is possible to evoke in the person addressed the ideas of facts not observed at the time but previously experienced. Without the sense perception of the person spoken to, speech is impotent; but it can, when based upon this experience, vastly widen the individual's domain of experience.

For the purpose of communication, every new perception which is at first passively received must be, as far as possible, spontaneously resolved into generally known elements or built up from them, as the case may be. This process is spontaneously effected by association and memory; in it there comes into play, even in the simplest observations, a speculative element which is not only justifiable but necessary and inevitable. Both the striving for adaptation in the thought of the individual, the striving for communication, and the necessity of economy in the thought of the individual and of the person communicating — who is compelled to make a limited number of ideational and linguistic elements suffice for his purpose — all necessitate comparison. But comparison is also the most powerful element of the inner life of science; for all connection and all conceptual unity comes into science by means of comparison. The zoologist sees fingers in the bones of the wing-membranes of the bat, compares the skull-bones with vertebrae, compares the embryos of different organisms with one another, and compares the stages of development of the same organism with each other, and obtains, instead of a conglomerate of disconnected facts, an orderly picture which consists of elements of the same kind and whose execution is guided by unification motives. The geographer sees a fjord in Lake Garda and a lake in the process of drying up in the sea of Aral. The philologist compares different languages and forms of the same language. If it is not customary to speak of "comparative physics" as we speak of "comparative anatomy", this is due merely to the fact that in an experimental science the attention is too much diverted from the contemplative element. Physics lives and grows by comparison like every other science.

2. There are different ways in which the result of comparison finds expression in communication. We say that the colors of the spectrum are red, yellow, green, blue, and violet; and these words may have been derived from the art of tattooing or they may later have acquired the meaning, that the colors are those of the rose, the lemon, the leaf, the cornflower and the violet. But by the frequent use of such comparisons in various circumstances, the common characteristics have so obliterated the varying ones that the former have acquired an independent significance and, as we say, an "abstract" or "ideal" one. Nobody thinks when he hears the word "red" of any other agreement with the rose than that of color, or when he hears the word "straight" of any other property of a stretched string than sameness of direction throughout. So also numbers, originally the names of our fingers, hands, and feet, which were used as ordinal marks of the most various objects, have become abstract concepts. Only the naïveté of the Pythagoreans could imagine that relations of number gave the whole essence of things and not merely one property. A linguistic communication of a fact, which employs only these purely conceptual means, we call a "direct description".

The direct description of a somewhat comprehensive fact is an arduous task, even when the necessary ideas are already completely developed. What a relief it must then be when we can say that a fact A under consideration behaves, not in a single characteristic only but in many or all particulars, like a fact B already known. The moon behaves like a body heavy towards the earth; light behaves like a wave-motion or electric vibration; a magnet behaves as though charged with gravitating fluids; and so on. We naturally call such a description, in which we make use to a certain extent of one already given elsewhere or even of one that has yet to be worked out accurately, an "indirect description". We are at liberty to complete gradually, to correct, or to replace entirely, this indirect description by a direct one. We see without difficulty that what we call a "theory" or a "theoretical idea" which is the starting point of a theory, falls into the category of indirect description.

3. What is a theoretical idea? What does it do for us? Why does it seem to us to stand higher than the mere adherence to a fact or an observation? Here again it is just that memory and comparison come into play. Only here our memory brings us, instead of a single feature of resemblance, a whole system of features, a well-known physiognomy in fact, by which the new fact is suddenly converted into one familiar to us. Indeed, the idea can, and is intended to, present more than we see at the moment in the new fact; it can widen and enrich this fact with new features for which we are stimulated to seek — and which are often found. It is this rapidity of extension of knowledge which gives theory a quantitative advantage over mere observation, while theory does not essentially differ qualitatively from observation either in mode of origin or in final result.

The acceptance of a theory always brings with it a danger. For theory substitutes for a fact A another simpler or more familiar one B which can represent in our thoughts the former in some respect or other, but just because it is another certainly cannot represent it in some other respect. If, now, as easily happens, enough attention is not paid to this point, the most fruitful theory may become a hindrance to research. The emission theory accustomed physicists to think of the projectile path of "light particles" as an undifferentiated straight line, and demonstrably made the knowledge of the periodicity of light more difficult to reach. When Huygens substituted the idea of sound — with which he was more familiar — for light, light in many ways appeared to him a known thing; but it seemed doubly strange in its polarization, for this is absent in the longitudinal waves of sound which were alone known to him. So he was not able conceptually to grasp the fact of polarization which lay before his eyes; while Newton, who adapted his thoughts to his observations, simply put the question: "An non radiorum luminis diversa sunt latera?" In this question polarization was conceptually grasped or directly described a hundred years before Malus. But if the correspondence between a fact and its theoretical representation reaches farther than the theorist originally supposed, he may be led thereby to unexpected discoveries, of which conical refraction, circular polarization by total reflexion and Hertzian waves are obvious examples and form a contrast to those given above.

4. We reach more of an insight into these things, if we follow the development of some theory or other more in detail. Let us consider a piece of magnetized steel beside an unmagnetized piece which is otherwise in the same condition. While iron filings are indifferent to the unmagnetized steel, the magnetized steel attracts them. Even if the iron filings are not present we must regard the magnetized piece as being in a different state from the unmagnetized one. For the unmagnetized piece shows that the mere proximity of the iron filings does not cause the phenomenon of attraction. The naive man, who takes for comparison his own will as the most familiar source of power, imagines in the magnet a kind of spirit. The behavior of a hot or an electric body suggests similar ideas. This is the point of view of the oldest theory, fetishism, which the investigations of the earlier middle ages had not yet overcome and of which the last traces still persist in our physics of to-day in the notion of forces. Thus the dramatic element is, as we can see, not always lacking in a scientific description.

If upon further observation it should be noticed, say, that a cold body, when placed near a hot one, becomes heated at the expense so to speak, of the hot one; and that moreover, in bodies of the same kind, the colder, say of double the mass of the hotter one, gains only half as many degrees of temperature as the hotter one loses, then an entirely new impression arises. The demoniacal character of the fact vanishes, for the supposed spirit does not act arbitrarily but according to fixed laws. But instead of it the impression of a substance instinctively comes to the fore; part of this substance flowing from one body to the other but the total amount of it, which can be represented by the sum of the products of the masses and their respective changes of temperature, remaining constant. Black was the first to be powerfully struck by this resemblance of thermal processes to motion of a substance, and under the influence of the notion of heat's being a substance, to which this resemblance gave rise, discovered specific heat and the latent heats of fusion and vaporisation. But strengthened by these successes, the notion of heat's being a substance then became an obstacle to further progress. It blinded Black's successors and prevented them from seeing the long known and obvious fact shown by the fire-drill, that heat is generated by friction. Fruitful as Black's notion was and helpful as it still is to learners in Black's special domain, it cannot claim permanent and universal validity as a theory. But what is essential to the conception, the constancy of the sum of the products mentioned, retains its value and may be regarded as a direct description of Black's facts.

It is natural for those theories which present themselves unsought, instinctively as it were, to act most powerfully; to bear our thoughts with them, and evince the greatest powers of self-preservation. On the other hand it may also be observed how much they lose in force as soon as they are examined critically. With matter we have to do continually; its behavior has firmly impressed itself upon our thought, our liveliest and most intuitive memories are knit to it. So it need not greatly surprise us that Robert Mayer and Joule, who finally demolished Black's notion of heat as a substance, again introduced the same notion in a more abstract and modified form as a principle of energy in a much more extensive domain, as was discussed more fully in an earlier chapter.

We know that in the development of the principle of energy yet another theoretical notion was active, from which, it is true, Mayer kept entirely free; namely, that heat and the other physical processes are due to motion. But when the principle of energy has been found, these auxiliary theories and transitional theories no longer play essential parts and we may regard this principle as well as Black's as a contribution to the direct description of an extensive domain of facts.

5. After these considerations it may seem not only expedient but even imperative to substitute gradually, as the new facts become familiar, direct for indirect description. For direct description contains nothing unessential and is limited exclusively to the conceptual comprehension of facts. Of course in recommending this substitution we do not spurn the help which theoretical ideas give us in research. We must acknowledge that we are not able, on the instant, to describe every fact directly. We should collapse disheartened if the whole multiplicity of facts which we come to know little by little were presented to us all at once.

Fortunately, only the isolated and unusual strike us at first and we bring them nearer to our understanding by comparing them with daily occurrences. In this way, the ideas of our ordinary speech develop. Afterwards the comparisons become more diversified and more numerous, the domains of facts compared become more extensive, and the resulting conceptions which render direct description possible correspondingly more general and more abstract.

The free fall of bodies first becomes familiar to us. The conceptions of force, mass, and work are transferred, with proper modifications, to electric and magnetic phenomena. A stream of water is said to have furnished to Fourier his first intuitive picture of a thermal stream. A particular case of the vibration of strings investigated by Brook Taylor explained to Fourier a particular case of the conduction of heat. Just as Daniel Bernoulli and Euler compounded the most various vibrations of a string out of Taylor's cases, so Fourier compounded the most various distributions of temperature out of simple ones analogous to Taylor's particular solution; and this method has spread over the whole of physics. Ohm modelled his notion of electric currents after Fourier's of thermal currents; then came Fick's theory of diffusion; and in an analogous way a notion of magnetic currents was developed. All kinds of stationary currents display in fact, common features; and even the state of complete equilibrium in an extensive medium shares these features with the dynamical state of equilibrium. Things so far apart as magnetic lines of force of an electric current and the stream-lines of a frictionless fluid vortex enter thus into a relation of similarity. The conception of potential, which was originally set up in a narrowly bounded domain, assumes a wide applicability. Things so unlike as pressure, temperature, and electromotive force show agreement in their relations to the conceptions derived from them in a definite way: slope of pressure, slope of temperature, slope of potential and likewise intensity of a fluid, thermal or electric current. Such a relation of systems of conceptions in which both the dissimilarity of any two homologous conceptions and the agreement in logical respects of any homologous pair of ideas comes to clear consciousness, we are accustomed to call an "analogy". It is an efficient means of subduing heterogeneous domains of facts by taking a single view. Here is clearly indicated the way in which we shall at last get a general physical phenomenology which embraces all domains and would be an exposition of physics which is free from hypotheses.

The theorem of Carnot and Clausius, which was originally borrowed from a resemblance in the behavior of heat to that of a heavy liquid, may be transferred by paying attention to such analogies, to all domains of physics: this was discussed in detail in a previous chapter.

6. By the process specified we develop comprehensive and abstract conceptions. These conceptions are not to be confounded with the more or less definite perceptual notions which accompany the conceptions.

The strict definition of a conception and, in case it is familiar, even the name of the conception is a stimulus to a precisely determined though often complicated, testing, comparing or constructing *activity* whose result, in most cases perceptible by the senses, is a term in the extension of the concept, as will be detailed in one of the following chapters. It is of no consequence whether the conception only directs the attention to a definite sense (sight), to aspects of a sense (color, form), or releases some elaborate action; nor does it matter whether the activity (chemical, anatomical, mathematical) is performed muscularly or with instruments or only in imagination or even merely indicated. The conception is to the worker in natural science what the note is to the pianist, the prescription to the apothecary, or the cookery book to the cook. It releases definite reactions but not ready-made perceptions. A practised mathematician or physicist reads a memoir as a musician reads a score. But just as the pianist must first learn, in succession, to move his fingers singly and in combination, so that afterwards he may respond almost unconsciously to a note that he hears or sees written down; so must the physicist and mathematician go through a long period of training before he masters the various fine innervations of his muscles and his imagination, if we may be allowed to speak in this way. How often the beginner in mathematics or physics does something more or less different from that which he ought to do, or thinks out something different. But if, after necessary practice, he meets with the concept "potential", for instance, he knows immediately what the word demands of him. Well-exercised activities which have resulted from the necessity of comparison and representation of facts by one another are thus the kernel of conceptions. Indeed both positive and philosophical philology claim to have found that all roots signify concepts, and originally only muscular activities.

7. Imagine that the ideal of complete, direct, conceptual description has been attained for a domain of facts: we can, I think, say truly that this description achieves everything the investigator can require. Description is a construction of facts in thought, and such a construction in the experimental sciences often makes possible an actual production. For the physicist the units of measurement are the building-stones, conceptions are instructions for building, and facts are the results of building. The unit of measurement is a conventionally established fact of comparison by means of which we build up other facts in thought. By this means we put other people to whom this fact which serves for comparison is accessible, in a position to reconstruct our own thoughts. We need the unit of measurement because we cannot directly transfer our notion of magnitude — indeed, no thought can be so transferred — but only with the help of facts which are open to common observation. Our thought construction is for us an almost complete equivalent of the fact, all of whose properties we can ascertain from the construction. It is well known that Kirchhoff in modern times conceived the function of the natural scientist to be a purely descriptive one; and that this view met with many objections. It is not improbable that the opinion of Kirchhoff, who had no time for a detailed critical discussion of it from the point of view of the theory of knowledge, was the result of a mere cursory glance; for in a conversation with Franz Neumann he did not defend it energetically. But still this view is not on that account false. The chief objection, that the need for causality and explanation is not satisfied by mere description, will, I hope, have some light thrown upon it in a later chapter.

CHAPTER XXVIII

LANGUAGE

1. In a preceding chapter, communication by language has been described not only as the necessary condition of the origin of science, but also as the means by which the motive of *comparison* is produced in scientific presentation and investigation. I may be permitted, therefore, without making the slightest pretension to authority on questions in which I have not made special studies of my own to state my views concerning the origin and development of language and its significance for scientific thought.

We find ourselves in the possession of speech as soon as our consciousness shines out with full clearness; to a child this is so much a matter of course that it is frequently much astonished at hearing that babies are obliged to learn to talk. But as soon as the facts have wrung from us this admission we naturally inquire: Who *first* taught language? Who *invented* it? If we have outlived the ingenuous period which looks upon language as a gift of the gods, the first explanations that naturally present themselves are the rationalistic theories which regard language as an ingenious invention and convention, and which attribute to men not yet in possession of language a far higher degree of intelligence than they now exhibit. We learn from positive linguistic science that one and the same language exhibits different stages of development, that different languages exist which are related to one another and which are therefore presumably of common origin; and, finally, that there are languages which show widely varying degrees of complexity in their structure. The weightier and more promising question with respect to the development of language is thus forced into the foreground, that of the origin is relegated to the rear, and the resolution of the latter is found with that of the former. In addition, we can readily observe the development of speech and thought in our own persons. And from the fact of our all having so abundant material for observation immediately at hand, both philosophical and psychological science have fortunately been placed in a position to compete successfully with positive research in this domain.

Traces of the ancient ingenuousness still linger in the question which is still so frequently put as to the *origin of human speech,* as if human speech ever had at any definite place or time a precisely determinable beginning! From the modern scientific point of view a totally different view of the problem must be entertained. Whence, pray, should human language have been developed, if not from the animal language of our ancestors? And no unbiassed person can entertain the slightest doubt that animal language actually exists. Every species of animals, particularly such as have social habits, has its accurately distinguishable cries of warning, allurement, attack and so on. The origin of the purely reflex sounds which are given by man's organisation accordingly require no explanation whatever; for sounds of this character were possessed by our animal ancestors.

2. The undeniable and stupendous differences between animal language and human language are as follows. Animal language has at its command only a small number of sounds, and these are employed in situations and emotions (fear, joy, anger) which, while different, are extremely general in character and are accompanied by corresponding activities which in their turn also are extremely indeterminate (flight, the search for food, attack). These activities are then more precisely determined by perception of the actual situation. Animal language, further, is largely innate and is learned only in a minute degree by imitation. The very reverse is true of human language. The belief that animal language is absolutely invariable is not borne out by the facts; the belief is refuted alone by the circumstance that related animal species employ systems of sounds of which any one is easily recognizable as a variation of the other.

The cries of the house dove, the wild dove, and the turtle dove may be cited as examples.[1] But the power of producing the phonic elements of language is also inborn in man, being part of the heredity of his organs of speech; and it is even permissible to assume a difference of races in this particular.[2] Only the combinations of sounds are learned. And the situation here is precisely what it is in the case of movements, which are innate in animals in far more enduring combinations than in man.[3] Man is born "younger," so to speak, and consequently with more capacity for adaptation.

It is customary to say that the language of animals is unarticulated. I am curious to know what ground there may be for such a contention. Many of the sounds uttered by animals and repeated by them on similar occasions, and in the same order, admit quite easily of being reproduced by our letters; and in the case of the other sounds for which this is impossible, owing to the fact that we possess no characters for sounds that do not accord with our organs, an acoustic or phonographic transcription could quite well be resorted to. If we examine the facts closely, we are constrained to admit that we are situated with respect to the language of animals precisely as we are with respect to any human language that is unintelligible to us; and that the word "unarticulated" only means not-English, not-German, and not-French. We might with equal reason call the movements of animals unarticulated because they do not correspond precisely to ours.

3. Animals are not credited with sufficient intellectual capacity to form a language; that power is supposed to be wanting to all creatures except to man. But is it found in man as the result of a sudden miracle, or has it been produced in him by gradual development? If the latter assumption is true, and it will be the one most likely to be accepted to-day, then the germs of human intelligence must have existed in some form in animals also. Let it be remembered that a mere difference of degree will account for everything. A man whose capacity for work produces but a trifle more than is necessary to supply his wants has a good prospect of a constant improvement in his condition; whereas, if there is the slightest difference in the opposite direction, he is almost certain to come to grief. Similarly, a species of animals or race of men, the range of whose intellectual variations is so narrow that they can never rise above a certain level, will be incapable of development, whereas a very slight average but constant excess of intelligence entailing effects not entirely effaced in the following generations, is a certain guarantee of continued evolution.

The underestimation of the intelligence of animals was for centuries the conventional thing. On the other hand, we now not infrequently meet with instances of new overestimation of the intelligence of animals which are quite as unfounded. I myself deprecated this overestimation.[4] Any great development of the intelligence of animals is unlikely for the reason that it would be both unnecessary and useless in their simple sphere of life. Long ago I observed the mechanical regularity with which beetles always creep *upwards* on a stalk, no matter how often they are turned round, just as other insects fly mechanically towards the *light*, etc. Since that time, the marvellous and instructive experiments of

Jacques Loeb on "heliotropism", "geotropism", and so on of animals have appeared, which throw a flood of light upon the mechanics of the lower organisms. But Sir John Lubbock, who annihilated in so exact and praiseworthy a manner the illusions respecting the intelligence of bees and ants, appears to me to claim altogether too much intellectual power for dogs.[5]

I am accordingly of the opinion that the view which assumes a *qualitative* difference between animal and human intelligence is a relic of an old superstition; I am able to see a *quantitative* difference, a difference of degree only, in the animal scale including man — a difference that assumes enormous proportions with the distance apart of the single members. The lower we descend, the weaker the individual memory becomes, and the shorter the series of associations at the command of the animal. A similar difference exists between children and grown people. In like manner, I see a quantitative difference only between the language of man and the language of animals. The same difference exists even between human languages of different degrees of development. Even in the most highly developed human languages, it often happens that the full meaning of some utterance can only be determined by the context; while it is well known that languages in a low stage of development very frequently have to have recourse to gestures to be understood, so that, when spoken in the dark, they are partly unintelligible.

4. As I take it, then, the right course to follow is to let the question as to the origin of language rest for the time being and in its place to propound the question of how animal language has been developed into the greater wealth and greater precision of human language. In this manner, the discontinuity between speaking and not-speaking, which forms the main difficulty of the problem, will be removed, and it will be discovered that the discontinuity never existed in the manner which has been assumed. Lazar Geiger,[6] to whom owe the most luminous of the contributions to this subject, actually pursued his investigations along these lines, although reversions to the old form of the inquiry are not wanting in his works. And when these reversions do occur, the most wonderful and unhappy solutions make their appearance. I agree with Noiré[7] that the manner in which Geiger conceived the origin of the first language cry is absolutely incomprehensible in the case of a man of Geiger's eminence. I am further of the opinion that Noiré has made

most important advances over Geiger. Great merit is to be accorded to Noiré's book even if one does not share his Kantian-Schopenhauerian point of view and even if one cannot assume with him the abrupt difference between animal and human intelligence. And although Noiré, in consequence of this latter circumstance, sometimes reverts to the old form of the inquiry, his results nevertheless remain valid for the question under discussion.

It will be admitted by every one that sounds uttered involuntarily could never have acquired meaning and significance as phonic symbols save in the event that things which are observable and have been observed by men in common are designated by them. It will, furthermore, not be doubted that, in the beginning of civilization, the employment of a symbol, or even anything like an appreciation of it, could not have been possible save where extremely strong common interests required some common activity which readily lent itself to the apprehension of all. The symbol under such circumstances will associate itself with the activity, with the sensory result of the activity, and with the sensorily perceptible medium or instrument. I think that this will be immediately accepted by every one, no matter what his philosophical or scientific position may be. The results of my own speculations upon the import of language, of conceptions, and of theories, in my own special department of physics, which I undertook without a knowledge of either Geiger or Noiré, point to the same results.[8]

The evolution of language, accordingly, is associated step for step with the various forms of activities involved in labor in common. In the precise measure in which the pursuits and industries of men are perfected is the sphere and power of language augmented. It is not to be denied that, in higher stages of development, events and objects of lesser importance form the occasion for the invention of new terms, just as in family life we frequently observe some chance word uttered in jest acquiring the office of a permanent symbol. But for this to be accomplished the value and import of speech must have already been known from use; there are requisite to it a certain freedom and disburdenment which are certainly wanting in the beginnings of civilization.[9]

5. The principal value of language is as a medium for the communication of thoughts; and the very circumstance that language compels us to describe the new in terms of the known, and therefore to analyze the new by comparison with the old, is a gain, not only for the person

addressed, but also for the speaker. A thought is frequently rendered much more clear by our imagining ourselves called upon to communicate it to others. Language has also a great value for solitary thinking. The sensory elements enter into the most manifold combinations and in these different combinations possess the most varied interests. The word embraces everything that is of importance for some single sphere of interest, and draws forth all the images connected with this sphere, as if they were beaded upon a thread. It is remarkable that we can employ word symbols correctly without having full consciousness of all the images which are symbolized by them, just as we can read correctly without scrutinizing each single letter closely. In like manner, we never suspect the existence of a portrait in a portfolio bearing the inscription "Landscapes", even though the contents of the portfolio be not familiar to us.

The ever recurring view that language is indispensable for every species of thought I must regard as an exaggeration. This did not escape the notice even of Locke, who declared that, inasmuch as language scarcely ever corresponded exactly with the thought, it may on occasions even be a drawback. Visualistic thought, which is concerned exclusively with the association and comparison of images, and with the recognition of their agreement or their differences, can be carried on without the intervention of language. For example, I observe a fruit on a tree too high for me to reach; I remember that on a former occasion by some good chance I got such a fruit by means of a forked twig broken from a tree; I notice a twig of this kind on the ground near me, but see at once that it is too short. This process may be gone through without a single word occurring to me. I am accordingly unable to believe that monkeys, for example, never employ sticks to accomplish certain ends, and never construct bridges by throwing trunks of trees across brooks *for the mere reason* that they are not in possession of language and consequently of any conception of *form*, or of any conception of sticks and trees as of isolated movable things which may be separated from their environment. On the contrary, it may be shown that the inability to make inventions rests upon an entirely different foundation. In saying this, I am far from denying that images also are invested with greater clearness by descriptions in language, and by the accompanying decomposition of their parts into simple and more familiar elements. In abstract *conceptual* thought language is of course indispensable. For example, Carnot distinctly emphasized those variations of temperature which are alone permissible in reversible processes as those which are consequences of variations of volume. Without the means of language thought would here be impotent.

6. Thinking without words is at least partly realized in every instance where a newly invented conception appears as the result of thinking, that is, wherever there is any scientific development.

The importance of language for conceptual thought is most clearly observed when we consider the formation of words or symbols such as goes on in full consciousness in the course of the development of science.

The conception of *exponent* originated in Descartes's having written a multiplied by itself n times as a^n; at any rate, the conception received for the first time by this act of Descartes's an independent standing, and was made capable of further development. Here was the starting point from which the conceptions of negative, fractional and continuously varying exponents and of logarithms were reached. The entire system of algebraic symbols, which is a product of conscious and designed invention throughout, is instructive in other respects also. We learn to operate mechanically with the system without having constantly before our minds the full significance of the operations involved. In like manner words also are joined associatively with one another without our possessing in consciousness all the precise images that correspond to them. Like algebra, language takes the load off thought for a time. In the measure in which our scientific terminology is carried nearer to Leibniz's ideal of a Conceptual Notation, which is a process actually taking place, the high advantages of such a system will be vividly felt.[10]

CHAPTER XXIX

THE CONCEPT

1. The first movements of newly born animals are responses to outward or inward excitations, and these excitations are effected mechanically without the intervention of the intellect (the memory), and have their foundation in inherited organisation. They are reflex movements. Under this head belong the pecking of young chickens, the opening of the bills of young birds on the return of their parents with food, the swallowing of the food placed in their gullets, the suckling of young mammals, and so on. It may be shown that the interference of the intellect not only does not enhance these movements, but frequently has a tendency even to disturb them.[1]

It will happen that a great variety of pleasurable and unpleasurable sensations will be produced during this process, and these sensations, which are peculiarly adapted to disengaging reflex movements, will become associated with others, which in themselves may be indifferent, and will be stored up in the memory, which is gradually developing. Any small portion of the original complex of stimuli may then evoke the memory of the entire complex, and this memory in turn may evoke the entire movement. The young sparrow described by me in another place affords a good instance of this,[2] and young mammals, prompted by the sight of their mother to seek nourishment, furnish a second example. The movements which thus take place are the final term of a series of associations; they are no longer reflex movements, they are now called voluntary movements. The question whether the innervation as such makes its appearance in consciousness not only by its results but also immediately, we shall forego, since it is a debated one and since the answer to it is not absolutely necessary to our purpose.[3]

As soon as a movement B which follows ordinarily as a reflex upon a stimulus R is induced voluntarily by some stimulus S associated with R, the most varied complications may arise, as a result of which entirely new complexes of stimuli — and, in consequence, entirely new motor complexes — may come into play. A young animal which has reached maturity is observed to seize an object which appears edible, sniff at it, nibble at it, and finally to bolt it or cast it away. Young anthropoid apes, so R. Franceschini informs me, are in the habit of biting forthwith into everything offered them, whereas old apes will toss aside objects for which they have no use after merely a cursory inspection. Infants, too, are wont to thrust into their mouths every object they can lay hold of. A colleague of mine once observed a child grasp repeatedly at a burnt spot on a table, and immediately convey the supposed object with comical eagerness to its mouth.

2. Accordingly under different circumstances with something in common the same activities, the same movements, are produced (such as grasping, sniffing, licking, and biting). These are productive of new sensory characteristics (odors, tastes. etc.), which become in their turn decisive and determine the subsequent behavior of the animal (as swallowing, laying aside, etc.) Now it is these responses, together with the sensory characteristics evoked by them, both of which will come somehow into consciousness, that constitute (as I take it) the physiological foundation of the concept. Whatever induces like reactions comes under the same concept; as many reactions as there are, just so many concepts will there be. No one will feel disposed to deny to an animal that has acted in the manner described, the possession of the germs of the concepts "food", "non-food," etc., even though the words denoting these concepts be wanting. But even designation by speech, in the form of calls, may under certain circumstances accompany the acts we are considering, notwithstanding the fact that the calls are provoked involuntarily and never clearly appear in consciousness as deliberate signals. The concepts which originate in this manner will be exceedingly comprehensive and vague in character; but they are none the less the most important for the animal. But primitive man too will find himself in a similar situation. The consequences of activities employed in explorations and in the attaining of ends may be already considerably complicated. Take, for instance, the stopping and listening on hearing a noise; pursuit and capture of prey; picking, cracking, and opening of nuts; etc. The behavior of civilized man is distinguished from that of the animal and primitive man merely by the fact that he possesses more varied and more powerful facilities for investigation and for the attainment of his ends; that he is able, owing to his richer memory, to make use of more circuitous methods and of a greater number of intermediary agencies (instruments); that his senses are capable of making more refined and more comprehensive observations;

and finally, that he is enabled by the richer store of language at his command to define with greater minuteness and with greater precision the elements of his activity and sensory perception, to represent these same elements clearly in his memory, and to bring them within range of the observation of others. The behavior of the natural scientist offers merely a further difference of degree as compared with the preceding case.

3. A chemist *can* recognize a piece of sodium at sight, but does so really on the presupposition that a definite number of tests which he has in mind would give the results which he expects. He can apply the conception "sodium" to the body in question with certainty only provided he actually finds the body to be soft as wax and easily cut, to have a silver sheen on the cut surface, to tarnish readily, to have the capacity to float and to rapidly decompose in water, to have the specific gravity 0.972, to burn with a yellow flame when ignited, to have the atomic weight 23, and so on. The concept "sodium", accordingly, is made up of a certain series of sensory characteristics which make their appearance upon the performance of certain definite manual, instrumental, and technical operations which may be very complicated in character. Under the concept "whale" we subsume an animal which has outwardly the form of a fish, but which, on careful anatomical examination, is found to have a double circulation, to breathe by means of lungs, and to posses all the other classificatory marks of the mammals. The physicist subsumes under the conception "electro-magnetic current of unit intensity" ($cm^{\frac{1}{2}}\, g^{\frac{1}{2}}\, sec^{-1}$) that galvanic current which acting with a magnetic horizontal component of $H = 0.2$ ($gr^{\frac{1}{2}}\, cm^{-\frac{1}{2}}\, sec^{-1}$) on a magnetic needle suspended in the center of a circular wire of radius 31.41 cm through which the current has been made to pass, turns that needle 45 degrees out of the meridian. This presupposes an additional set of operations for determining *H*.

The behavior of the mathematician is similar. A circle is thought of as a line in a plane every point of which can be shown by measurement or otherwise to be equidistant from a certain point in the plane. The sum of 7 + 5 is that number, 12, which is reached by counting onward 5 numbers from 7 in the natural series. In these cases also we are required to perform certain well-defined operations — the measurement of lengths, counting — as the result of which certain sensory attributes (namely, the equality of the lengths in the one case, and the number 12 in the other) make their appearance. The well-defined activities in question, whether simple or complicated, are analogous in every respect to the operations by which an animal tests his food; and the sensory attributes referred to are analogous to the odor or taste which is determinative of the further behavior of the animal.

Many years ago, I made the observation that two objects appear alike only in case the sensation complexes corresponding to the two objects contain common components. This observation has been abundantly illustrated elsewhere in my works, and I have given numerous examples (symmetrical and similar figures, melodies of the same rhythm, etc.[4]) Attention was also drawn to the aesthetic value of the repetition of the same *motif*.[5] The idea was then naturally suggested that there lay at the basis of every abstraction certain common real psychical elements, representative of the components of the conception,[6] be those elements ever so recondite. And it was found that the elements in question were commonly brought to consciousness by some special and definite activity — a fact which has been sufficiently discussed in connection with the examples given above.

4. The concept is puzzling for the reason that on the one hand it appears in a logical aspect as the most definite of psychical constructs; while on the other hand, in a psychological aspect, when we seek for its real visualizable contents, we only discover a very hazy picture.[7] Now the latter, whatever its composition, must necessarily be an individual picture. The concept, however, is not a finished idea,[8] but body of directions for testing some actually existing idea with respect to certain properties, or of constructing some idea from given properties. The definition of the concept, or the name of the concept, releases a definite activity, a definite reaction, which has a definite result. The manner of the reaction,[9] as well as the result, must find its expression in consciousness, and both are characteristic of the concept. A body is electric when it exhibits certain sensible properties in certain reactions. Copper is a body of which the bluish-green solution in dilute sulphuric acid exhibits a certain behavior when subjected to a certain treatment, and so on.

Since the group of operations which is involved in the employment of a concept is often complicated, it is no wonder that the result appears as a visual picture only in the simplest cases. It is, furthermore, clear that the group of operations in question, like the movements of our body, must be thoroughly practised if we are really to possess the concept. A concept cannot be passively assimilated; it can be acquired only by doing, only by concrete experience in the domain to which it belongs. One does not become a piano player, a mathematician, or a chemist, by looking on; one becomes such only after constant practice of the operations involved. When practice has been acquired, however, the word which stands for the concept has a different sound for us. The impulses to activity, which are latent in it, even when they do not come to expression or do not appear in consciousness, still play the part of secret advisers who induce the right associations and assure the correct use of the word.[10]

5. Just as a technical operation may serve for testing a given object (testing by weights, dynamometric tests, the record of an indicator diagram) or for constructing a new object (the building of a machine) so also a concept may be used in a testing or constructive sense. The concepts of mathematics are mostly of the latter character, whereas the concepts of physics, which cannot create its objects but finds them already present in nature, are ordinarily of the first-mentioned kind. But, even in mathematics, figures arise independently of the inquirer which furnish material for subsequent investigation; and in physics also concepts are constructed for economical reasons. But the fact that mathematics operates in the main with constructions of its own creation, containing only that which it itself has put into them, whilst physics must wait before it finds out how far the objects of nature answer to its concepts — this fact is the foundation of the *logical* superiority of mathematics.

6. Many of the concepts of mathematics show still another peculiarity. Let us consider the simple concept of the sum $a + b$, where a and b may first be supposed to be whole numbers. This concept contains the impulse to count onward for b numbers from a in the natural series, when the last number is $a + b$. This act of counting forward may be regarded as a muscular activity which is always the same in all cases, however different, and the beginning of which is determined by a and the end by b. Through variation of the values of a and b, an infinite number of cognate conceptions is created. If a and b be conceived as members of a number-continuum, there results a continuum of related concepts for which the reaction-activity is throughout the same, but where the beginning and the end are determined by properties repre-

senting members of the same continuum. Analogous considerations hold with respect to the concept of product, etc. The existence of such concept-continua offers great advantages in those sciences to which mathematics is applicable.

A reference here to the old controversy of the nominalists and realists will be in place. There seems to be a germ of truth in both views. The "universals" possess no physical reality, but they do possess a physiological reality. The physiological reactions are of less complexity than the physical stimuli.

CHAPTER XXX

THE CONCEPT OF SUBSTANCE

1. That which is unconditionally permanent — or at least that which we think to be so — we call *substance.* The naive man and the child think that everything is unconditionally permanent for the perception of which the senses alone are necessary. Thus every *body* appears to be substantial, because we need only to grasp it or to look at it in order to perceive it. That this supposed unconditionally permanent thing is by no means really unconditionally permanent, since a definite activity of the senses (looking or touching) is the condition of the supposed permanent perception, does not strike the naive man; who, paying no further heed to the condition so easily complied with, regards it as always complied with or at least as capable of being complied with.[1]

But greater attention shows that the question here is not of an *absolute* permanency, but of a *permanency of combination.* It shows, further, that a definite activity of the organs of sense is not the *only* condition of a definite permanency. In order that something definite may be seen in a definite place, a definite tangible something must also be there; and thus a condition lying outside (foreign to) the sense of sight must be fulfilled. Moreover, illumination is a condition of visibility — for a definite sight a definite illumination. *Tangibility,* as bound up with the mere *capability of reaching* what is nearest at hand, seems relatively independent and permanent and even (erroneously) absolutely permanent. The tangible *seems* to present an absolutely permanent (substantial) *nucleus,* to which cling the other more variable elements depending on very many conditions. Since, from the complex of sensuous elements that form a whole, any single one can fall away without producing a noticeable derangement, there arises the idea of a supersensuous and substantial nucleus which holds those elements together — a supersensuous condition of perception. Judicious and open-minded consideration, however, represents the state of things differently.

A body looks different in every light, presents a different optical picture in every position, gives a different tactile picture at every temperature, and so on. But all these sensuous elements are so connected with one another that, for the same position, light, temperature and so on, the same pictures recur. Thus what we have to do with here is, in every case, a permanency of *combination* of the sensuous elements. If we could measure all the sensuous elements, we would say that the body consists in the fulfilment of certain equations between the sensuous elements. Even where we cannot measure, the expression may be retained as a *symbolic* one. These *equations* or relations are thus really the permanent things.

2. It may be urged for the existence of a *supersensuous substantial* condition of perception that a body which I perceive in a certain way must be perceived by *others* in a similar way. Certainly no one will deny this. But it means nothing more than that equations, similar to those which subsist between the more closely connected elements which represent my ego (J), hold also between the elements of other egos (J', J'', J''', . . .), a picturing that facilitates my understanding of the world; and that, furthermore, such *equations*, embracing the elements of all egos, J, J′, J″, J‴, . . . , subsist. More than this no investigator, who is conscious of the purely descriptive nature of his problem, and who seeks to avoid pseudo-problems, wishes to see in the circumstances just mentioned. Terms derived from old and one-sided views that are rooted in traditions would hardly describe the state of things better. If, now, anybody wants to regard the said equations, in contradistinction to sensuous elements, as *noumena*; or, on account of their importance in our knowledge of the actual world, as the expression of *realities*, such a quarrel about words is of little consequence.[2]

3. The state of things is not analyzed so accurately by the naive man, nor as a rule by the physicist. The physicist is in the habit of immediately linking up with the naive notion. The body appears as a *persistent* and given complex of attributes. Its finer variations, as well as the fact that the terms of the complex emerge only from certain sensuous, muscular, or technical reactions, are usually not considered. To the sensuous complex, which represents the body, belongs also the fact that it is perceived at a definite *time* in a definite *place* and thus also *sensations* of time and place.[3] The fact of the *mobility* of a body signifies *variability* of the two last-named elements of the complex, combined with relative *stability* of the remaining terms. A body '*moves*' from one place to another. A body leaves one place, and we find the

'*same body*' in another place. Naive consciousness imagines that the body is something *permanent*. The body is the basis of the first and most naive *notion of substance*. This notion develops quite *instinctively*, and for that very reason is very powerful. The animal seeks an appetizing body which has just disappeared from sight everywhere in the neighbourhood, in the unmistakable conviction that it must be there; and the child behaves in the same way. With his limited critical faculty, the child freely transfers the notion of substance to *everything* perceptible, seeks the vanished shadow, the extinguished light, strives after an image or illusion, and so on.[4] The error appears natural, because of perceptions the preponderating number are allied to bodies.

4. Suppose now a body to be *fluid*, or at least freely *divisible, quasi-fluid*, so that a part may be poured from one vessel to another. Every part of the body will then present a certain permanent complex of properties, and since the quantity of particles is susceptible of increase and diminution, those properties which manifest themselves in certain reactions are also represented as *quantities*. We arrive in this way at the notion of something *permanent* and *substantial* which may differ in different bodies with respect to *quantity*: and this we call '*matter*'. The parts of a body are again (permanent) bodies. If we take away a quantity of parts from a body, they appear elsewhere. The '*quantity of matter*' appears *constant*. The essential thing about this developed *notion of substance* consists in the fact that we regard the *quantity* of *substance* as invariable; in such a way that the quantity which vanishes somewhere appears again elsewhere, so that the *sum of the quantities remains constant*. A simple movable body forms a *special case* of this general notion. The *conceptual reaction*, by which the question, as to whether or not something is to be subsumed under the concept *substance*, is answered, will thus consist in this, that what appears as a quantitative loss somewhere should be sought elsewhere (whether by sensuous, muscular, technical or intellectual, mathematical operations is indifferent). If that which appeared as a loss is found, then the problematic something corresponds to the concept *substance*. We may notice that the simple looking about for a missing body represents the fundamental types of conceptual reactions which reach up into the most abstract domains of science.

The parts of a body, that is to say properties of it appearing upon different reactions, are additive quantities. Matter, or a body, will thus appear just as multiply *substantial* as there are properties producible; and thus with respect to weight, capacity for heat, heat of combustion, mass, and so on. For homogeneous bodies, these quantities are proportional to one another, since they are connected with one another in every particle, and we can therefore make use of any one of them as a measure of the others. Newton[5] designated *mass* as *quantity of matter*, and this (scholastic) expression has been critically examined elsewhere.[6] Here, reference is to be made only to the fact that each of the properties cited by way of example represents a *substantial quantity*; so that, for the concept *matter*, actually no other function remains than that which represents the *subsistent relation* of the separate properties. Of great practical significance was the experimental proof carried out by Newton, that *mass* and *weight* (at the same place on the earth) are proportional to another for *any* different bodies whatever. But mass is not on that account *the* 'quantity of matter', but only *one* (mechanical property of the complex designated as matter; in no way superior to any of the others cited as examples.

Were we confined to our senses alone, in our consideration of the subsistence of material properties, our judgment would be subject to many vacillations, even if we disregarded the fact that our observations were not exactly communicable. The Newtonian demonstration provides us, in the balance and the weight theory, with a *measure* of *substantiality*. These devices assist our direct sense observation in a manner analogous to that in which the thermometer assists observation by mere heat sensation. To anyone, in possession of a balance and a gravitation theory, there is accessible a datum of comparison, to which we can refer, when *communicating* our observations and exact representations of facts in thought. Herein lies, as already noted, the significance of all *measures*.

5. The history of this science shows the manner in which the substance-concept enters into physical theories and how it develops in them. An electrical or magnetic body differs in no way, to outward appearance, from a non-electric or non-magnetic body. But certain bodies move towards the former, while they behave indifferently toward the latter. Just as we are accustomed to observe that a tangibility is the basis of perceptibility, even though we do not touch the object at the moment; so we assume a *permanent difference* between electric and magnetic bodies on the one hand and indifferent bodies on the other

hand, which is not indeed perceptible at the moment, but which perhaps might become manifest sometime later. This *permanent* difference is, in the most natural and simple way, conceived as an imperceptible *substance*. This idea has also its (economical) *advantage*; for he who conceives the electric body to be *charged with* this substance, although it is not distinguishable from the non-electric body by direct sensibility, does not at each recurrence experience a fresh surprise at its behavior.

The living body of man or animal is distinguished from the dead body in a manner analogous to that in which the electric body is distinguished from the non-electric. No wonder, then, that the "*soul*" likewise was conceived as a substance; especially when one considers that in dreams, etc., it is imagined to be perceived in an isolated condition. Wherever *animistic* concepts intrude into physical theories, these belong, as has been already remarked, to the domain of fetishism.

The physical substance-concept undergoes a development, as soon as it is noted that one body is heated at the expense of another, that one body is electrified at the expense of another; that, furthermore, in the former case a certain product-sum (heat capacity × temperature change), and in the latter case the sum of the electric forces compared with that of the unit charge at unit distance, remains constant. Thereupon, the substance-conception enters the domain of quantity concepts.

The transition of physical concepts from the previous phases to the one finally designated was completed at the end of the eighteenth century. A further development consists only in the fact that the original, naive substance concepts were recognized as unnecessary; and that a value, at the most, as *illustrative pictures*, is attributed to them, while the discovered *quantitative relations*, which are manifested in the fact that the equations mentioned above hold good, are recognized *as what is really subsistent or substantial.*

6. The formation of substance notions may be further encouraged by various circumstances. For instance, consider the *sparks* obtained by contact with an electric body, the sparks which flash out between two bodies in electrifying one from the other. What is more natural than that one should believe the electric substance, its very self, to be seen there: that one should, like Franklin, speak of the "*electric fire*", of the distinction between electric fire and *ordinary* fire, which certainly presents analogous appearances and seems to break out obviously

enough in the glow and flash from the body. Naturally, Franklin was confirmed in his ideas from the fact that, led by them, he succeeded in carrying out fresh experiments; and, by means of his kite, was able to charge Leyden jars with electricity from the clouds, or, so to speak, fill flasks with the electric fire of lightning. The substance notion certainly represents, although not adequately, a *part* of the actual relations; and hence it had the power to lead, as it has done, to important discoveries.

Clausius, in an academic address,[8] discussed the substance-notions of physics. According to his view, the fact that the *number* of accepted substances has gradually *diminished*, whereas formerly a *special* substance, seem to be the basis of the phenomena of light as well as of experience, constitutes an essential advance for physics. Special substances seem to be the basis of the phenomena of light as well as of heat. By the knowledge of the wave nature of light, and the identity of light and radiant heat, these *two* agencies are reduced to *one*. Ampère's theory of magnetism reduced this to electricity, and the relations between light and electricity finally caused the *electric* nature of light to be recognized. In this way, Clausius believes, it has become clear that besides *ponderable* mass only *one* substance subsists, which up till now has been called *aether*, this being nothing else but *electricity*. Although we esteem Clausius as a chief originator and promoter of thermodynamics, it cannot be denied that his standpoint with regard to the substance theory is very close to that of Franklin. With his ideas of *natural* philosophy, he essentially belongs to the eighteenth century.

7. The modern *atomical philosophy* is an attempt to make the substance-notion, in its most naive and crudest form, the fundamental concept of physics; for it is the conception that holds the *body* to be absolutely subsistent. The *heuristic* and *didactic* value of the atomical philosophy – which lies in its *intuitiveness*, whereby it sets in motion the simplest, most familiar, most concrete, elementary and instinctive functions of imagination and intellect – is not in the least to be denied. Indeed, it is significant that Dalton, a man who was a teacher by profession, restored the atomistic philosophy to life. The atomistic philosophy, with its childish and superfluous subsidiary conceptions, is in peculiar contrast with the remaining philosophical development of the physics of today. It is doubtless possible, from the atomistic philosophy, as is from Black's substance-notions to pick out the *essential, fact-representative, conceptual kernel* and to discard the husk of

superfluous subsidiary notions. To this reality belongs the representation of definite atomic weights and multiple proportions. But the simple volume relations of combinations are represented only with an effort. Before everything else, the atomistic philosophy represents the fact that the elements come out of their combinations *unchanged*. How little this "unchangeableness" of a body corresponds with the original crude substance notion has been already explained. The atomistic philosophy has recently gained ground again owing to the advances made in stereochemistry.[9]

8. The more the conditions of a phenomenon become known, the further the impression of materiality passes into the background. One recognizes the *relations* between *condition* and *conditioned*, the equations which cover greater or less domains, as the *inherent permanency, substantiality*, as that whose ascertainment makes possible a *stable world picture*.[10]

But the natural philosopher is not only a theorist, but also a practician. In the latter capacity, he has operations to perform which must proceed instinctively, readily, almost unconsciously, without intellectual effort. In order to grasp a body, to lay it upon the scales, in short, for *hand-use*, the natural philosopher cannot dispense with the crudest substance-conceptions, such as are familiar to the naive man and even to the animal. For the higher biological step, which represents the scientific intellect, rests upon the lower, which ought not to give way under the former.

CHAPTER XXXI

CAUSALITY AND EXPLANATION

1. It is one thing, so it is said, to *describe* a process, and another thing to tell the *cause* of the process. In order to become clear on this point, we will inquire how the concept of cause arises.

In general, we need to inquire about causes only when an (unusual) change appears; because, in general, such a case alone attracts attention and gives occasion for questions; and further, it is only where *different* cases for the changes appear that the question in regard to the condition for the one or the other has any significance at all. The changes most familiar to us in our surroundings are those brought about by our *wills*, which lead to the conceptions of animism and fetishism. Hume yields for a moment to the opinion that our concept of cause might owe its origin to this fact; but immediately finds that the connection or succession between will and motion is of exactly the same kind as any other connection or succession given in experience. We do not obtain an insight into the connection between will and motion that is *greater* than that obtained in any other case of connection, thinks Hume; and he finally admits only the *expectation engendered by the habitual.* Hume's analysis, his illustration of the case by means of the palsied man who cannot move his arm, in spite of his will, is admirable as a standpoint for *higher criticism.* But, the whole history of civilization, with its powerful phenomena, speaks loudly in opposition to him, and testifies that to the *common* consciousness the connection between will and motion is far *more familiar* than any other. The idea alluded to is really ineradicable and is constantly turning up. Thus, S. Stricker, in his time, *drastically* explained the difference between an exact experimental and a historical (sociological) science, by saying that in the former one can arbitrarily insert or reject facts, and with them their results, by the mere will; in the latter one cannot do so. The correctness of this will be acknowledged by every natural philosopher.

2. The Hume critique, notwithstanding, maintains its ground. One should not, however, overlook the fact that there are connections of *different degrees* of familiarity, and that the most noteworthy psychic

phenomena are conditioned by this circumstance: indeed, that all problems relating to causality have their origin therein.

It is known that whenever animism or fetishism is in vogue almost *every* connection is considered possible. Still, at the same time popular belief prefers the interconnection of such things as have a certain similarity, even though this should lie only in the imagination of the believer. Thus fruits of plants are regarded as remedies for the head, the roots as remedies for the feet, and so on. For unusual effects, fantastic causes are sought: the witches' brew in Shakespeare's Macbeth affords a drastic example. We understand these things when we return to the mode of thinking of our childhood. But the essential traits of popular thinking are still expressed by thinkers of the Ionian philosophic school, and sporadically appear even today.

To the modern researcher, scarcely anything seems more strange than the system of occasionalism initiated by Descartes, or than the pre-established harmony of Leibniz. One recognizes both theories, however, as an almost necessary result of the intellectual situation in which those thinkers found themselves. One easily follows the connection of one psychic state with the succeeding by the clues afforded by association and logic; it must also have seemed comparatively easy, during the development of mechanical science, to discern, in any state of the mechanical world, signs of the one to follow. But this facility is totally lacking for the connection of the psychical world with the mechanical. Spirit and matter seemed absolute strangers, all the more different the further mechanics progressed; and the theological opinion of the time was scarcely necessary in order to produce the systems mentioned. We still see even today, in the Duboisian "Ignorabimus", the expression of a similar intellectual situation.[1]

Exact analysis shows, it is true, that we know as little of *why* a striking body sets in motion the one struck as we know why our psychical states have physical effects. Both connections are simply given in experience; only, the former is more simple, more familiar to the experienced mechanician. He has, in the direction, velocity and mass of the striking body, many more essential facts for the separate characteristics of the effect-states; he can move, in the former case, in more certain and familiar, more determinate and particularized thought-constructions. But it is only a difference of *degree* which creates the illusion of a *qualitative* distinction of the two cases.

3. It cannot be sufficiently emphasized that we judge concerning the connection of two facts in very different ways according to circumstances. In many cases we scarcely think of the possibility of a connection, while in other cases we are straightaway under a psychical compulsion and the connection seems a *necessary* one to us. Thus, for example, the definite trajectory seems, to the skilled artilleryman, connected necessarily with the initial velocity and direction of the projectile. In fact, *where* the process conforms to well-known, simple and clear geometric (kinematic) laws, it lies before us just as clearly as they do; initial velocity and initial direction are transformed for us into the *criterion*, from which the trajectory elements arise as a logically necessary *result*. At the moment when we feel this *logical necessity*, we do not *simultaneously* think that the existence of that condition is merely given by experience without being due in the least to necessity.

The discrepant force of such causality judgments impel therefore to investigation of their nature and engender the self-same Hume-Kantian problem: How can the existence of a thing, A, become in any way the necessary condition of the existence of another thing, B? The two thinkers solve this problem in altogether different ways — Hume, in the manner already mentioned, to which we assent. Kant, on the contrary, imposes upon the mind the *actual* power with which causality judgments make their appearance. The relation of the criterion (to which knowledge is due) and effect demonstrably hovers before him as ideal. The "innate concept of the understanding" appears to him, so to speak, as a postulate — necessary to understand psychologically the actual subsistence of causality judgments. Nevertheless, that the concept involved is not innate, but one developed by experience *itself*, is shown by the simple reflection that the experienced physicist behaves, for the first time, toward a newly observed fact altogether differently from the inexperienced child toward the same thing. A fact derived from experience does not operate just by itself alone, but is placed in psychical connection with all such facts that have preceded it. Thus, it is true, the impression may arise, as though we could experience by a single fact something which is not comprised in the fact *itself*. This something, which we add, is certainly comprised in the totality of preceding experience.

Where we assign a cause, we only express a relation of connection, an existing of fact; that is to say, we *describe*. When we speak of "attraction of masses", it might seem as though this expression implies *more* than the actual matter of fact. But what we add beyond this is certainly barren and useless. If we put the common acceleration $\phi = k(m + m')/r^2$, this formula describes the fact much more exactly than the above expression; and, at the same time, eliminates every superfluous addition to the fact.

If one endeavors to remove the traces of fetishism which still cling to the concept of cause; if one reflects that *one* cause, as a rule, is not assignable, but that a fact is generally determined by a whole system of conditions — then this leads to the total abandonment of the concept cause. It is far better to regard the conceptual determinative elements of a fact *as dependent upon one another* in exactly the same sense as the mathematician, for example the geometer, does.

4. *Explanation* is even supposed, according to widespread opinion, to be essentially different from description. Description gives the fact, it is considered, but explanation gives a *new insight.* Although the question has really been answered already by what has preceded, I should like here to throw light upon it from still another side.

Imagine a hot and a cold piece of iron; the two pieces may be exactly alike in other respects. Upon the former a drop of water sizzles and evaporates, a piece of wax melts and smokes; while upon the latter a drop of water freezes, a drop of wax falling on it suddenly becomes hard. I must therefore suppose the two pieces to be in a different state, which I call the *heat state*, because my sensation of heat gives me an *indication* of it. But, by this heat state I understand absolutely nothing else beyond the totality of *behavior* of this piece of iron toward other bodies; which, taught by experience, I have to *expect* so long as it is able to excite the characteristic sensation as indication. I can designate the *state* somehow (by a *name* or by a *picture*) to represent for me some imaginary thing in the iron outside of the *representation* of the known processes; but I obtain no advantage whatever by doing so. I can deduce nothing from it. I can infer nothing that experience has not taught me. In this case, from the heat sensation, I already obtain an indication of what I have to expect, even though the drop of water or wax is not yet there. A still better indication is that given by the thermometer.

I imagine now two equal steel rods, the one magnetized, the other unmagnetized, which I cannot distinguish from one another either by sight or touch. I have, suppose, just tested them; and I am aware, for instance, that the rod lying on the right is magnetized, and that the one on the left is not. I can, also, label the one rod. The magnetic fluid which I may imagine in the one rod (as a mental label) profits me nothing. If fresh rods are submitted to me, I am quite unable, with and without fluid-conceptions, to decide which state I have to conceive. When I suspend the rod freely, or move it towards a coil of wire, I gain for the first time (by the directive force or the induced current respectively) an *indication* of the behavior, and of the state, similar to that afforded in the above case by the heat sensation or the thermometer. The *relation of the actual to the actual* alone has any value; and this is exhausted by description.

The fluids, mentally supplied, have indeed only those properties that must be ascribed to them for the purpose of representation of the actual. Is it possible for them to contain more than the actual?

5. How can the impression arise that an explanation is more effective than a description? When I show that a process, A, behaves like another, B, which is *better* known to me, A thereby becomes more *familiar* to me; just as if I show that A consists of the succession or the juxtaposition of B, C, D, . . . , which are already well-known to me. But, in this, one actual fact is merely replaced by another actual fact, one description by another description, that is perhaps better known to me. The subject may thereby become more familiar to me, a simplification may result therefrom; but no change in essentials can take place.

It is said that, in the exposition of the physicist, facts stand in the relation of necessity; and that mere description does not express this at all. If I have ascertained that a fact, A, has certain (for example, geometrical) properties, B, and hold to this in my mind, it is obvious that I cannot *at the same time* disregard it. This is a *logical* necessity. But, it does not follow from that, that the property, B is due *necessarily* to A. This connection is given solely by experience. Any other than a *logical* necessity — a physical one, say — does not even exist.

If we ask the question, when is a fact clear to us, the reply must be that we are able to reproduce it in thought-operations that are perfectly simple and familiar to us; they may partake of the nature of a combination of accelerations, or a geometric summation, and so on. This demand for *simplicity* is obviously a different thing for the expert and the beginner. Description by means of a system of differential equations suffices for the former, whereas the latter requires gradual construction from elementary laws. The former instantly sees the connection of both representations. Naturally it will not be denied that the *artistic* value of descriptions of essentially the same things may be very different.

6. Outsiders are the most difficult to convince that the general *laws* of physics for any mass systems, electric, magnetic, and so on, are not essentially different from *descriptions*. Physics really has a great advantage over many sciences. Where, for example an anatomist, seeking for the similar and dissimilar characteristics of animals, attains an ever finer and finer classification, the separate facts which the terms at the end of the series represent are so *different* that they must be *separately* characterized. One think, for example, of the common characteristics of vertebrate animals, the class characters of the mammals and birds on the one hand, of the fishes on the other; of double circulation of the blood on the one hand, of single on the other. *Isolated* facts always remain over finally, which exhibit merely a *slight* similarity among themselves.

Chemistry, a science much allied to physics, is often in a similar position. The abrupt change of qualitative properties, the slight similarity of the coordinated facts of chemistry, make treatment difficult. Body pairs of different qualitative properties combine in different mass relations; but a connection between the former and the latter is at first not perceptible.

Physics, on the contrary, displays whole immense fields of *qualitatively homogeneous* facts, which differ merely in the number of equal parts into which their characteristics are resoluble, and thus differing merely quantitatively. Wherever we have to deal with qualities (colors and sounds) *quantitative* characteristics are at our disposal. Here the *classification* is so simple a task that, in general, it is not consciously such at all; and, even in infinitely fine gradations, in a *continuum of facts*, the number system is prepared ready in advance, to be followed as far as we please. The coordinated facts are here *very similar* and related; and so are their descriptions, which consist of a determination of the measure numbers of certain characteristics by those other characteristics by means of familiar operations of calculation, that is to say, derivative processes. Here also, everything that is *common* to all descriptions can be found; so that a *comprehensive* description or a *rule of construction* for all simple descriptions will be given; and this is what we call, precisely, the *law*. Well-known examples are the formulae for a free fall, for projection, for central motion, and so on. Even if physics apparently accomplishes with its methods so much more than other sciences, yet we must, on the other hand, consider that in a sense it meets with *far simpler ones.*

Chemistry has, moreover, known how to avail itself, in its way, of the methods of physics. Apart from older investigations, the periodic series of L. Meyer and Mendelejeff are ingenious and fruitful means towards setting up a lucid system of facts which, completing itself by degrees, almost reduces to a *continuum* of facts. Through the study of solution, dissociation, and generally of processes presenting an effective continuum of cases, the methods of thermodynamics have found entry into chemistry.

CHAPTER XXXII

REVISION OF SCIENTIFIC VIEWS CAUSED BY CHANCE CIRCUMSTANCES

1. Reference has already been made to the fact that, owing to the inexhaustibility of experience, a certain incongruence between thoughts and facts always remains to be taken account of. Although our conceptions are adapted to a complex of circumstances, other circumstances outside of this, nevertheless, come into play; these we are not acquainted with, and do not perceive; these we have not in our power, and accordingly can neither introduce nor exclude them arbitrarily. The totality of these circumstances, which become operative without our expectation, and without our cooperation either mental or practical, we may call *chance*. Now it is the nature of the case that the defective adaptation of the psychical life to the physical makes itself sensible by such chance circumstances, and that further adaptation is even promoted by them. In fact, chance plays a principal part, not only in the development of knowledge, but also in the transformation of practical life. This has been discussed in detail elsewhere, and only a few supplementary remarks are to follow here.[1]

2. The manner in which *entirely new* domains of facts are opened up by chance to the observant investigator is typically exemplified by the discovery of phenomena now familiar by Galvani, of the polarization of light by Malus, of visual purple by Boll, of X-rays by Röntgen, and so on. A *refined* adaptation of thought is involved in Newton's discovery of dispersion by observing the incompatible length of the spectrum with circumstances known up till then, in Gay-Lussac's overflow experiment, in Laplace's correction of the theory of the velocity of sound, in Hertz's experiments, in the discovery of argon, and so on.

3. Analogous processes occur in technical life, and may be exemplified by the invention of the telescope, of the steam-engine, of lithography, of the daguerrotype process, and so on. Analogous developments may be traced back right to the beginnings of human culture. It is in the highest degree probable that the most important advances of civilization (as, say, the transition from the hunting to the nomadic life)

have been brought about, not by plan and purpose, but by fortuitous circumstances, as the following quotation from Dr P. Carus exemplifies.

A very important progress is marked in the transition from the hunting stage to the nomadic era of mankind; and several hypotheses can be made as to how it was effected. It is generally assumed that the hunters, having killed a cow or a sheep, might have easily caught their young ones and taken them to the camp of the tribe. This is not probable when we consider the temper and intelligence of the men at that period. We might almost expect that a cat would spare and feel the young birds in the nest, after having caught and eaten the mother.

There is another and more probable solution of the problem.

The Deer Park Cañon, in La Salle County, Illinois, received its name from its being used by the Indians to keep deer in it, which in times of great need could easily be killed. It is a big natural enclosure, from which the deer, if the exists were well-guarded, could not escape, and where they found sufficient food, water, and shelter. It must have been more difficult to hunt an animal than to chase it into the cañon, where herds of deer could be kept without trouble.

The Indians who lived on this continent when the white man came had been taught the lesson, but had not yet learned it. Nature had shown the red man that he could keep herds; he actually kept herds of deer in the natural enclosure of Deer Park; and yet he had not as yet become a shepherd or a nomad. He still remained a hunter.[2]

4. It has already been detailed[3] how essential, in the processes mentioned, psychical cooperation is, whether it be applied to purely intellectual or to practical developments. It was also shown that, in respect to *psychical capacity for adaptation*, the animal world forms a continuous series, from the moth which, simply from a necessary response to stimulus, flies into the flame, from the ant, which, according to Lubbock, does not know how to make use of an obvious and purposely offered advantage, up to the anthropoid ape which learns to open a cigar-case consisting of two parts, but not to close it,[4] and so on up to man with his great variation of intellectual individuality. There is, in this series, a vast difference in the ability to acquire new experiences.

5. The effect of the reception of new experiences is shown in the fact that an established practical procedure or an established mode of thinking becomes revised or modified. Instead of fording a stream, a man, after adequate experience, will lay the trunk of a tree across it resting upon the banks. Quicksilver, he comes to the conclusion, is not sucked into the vacuum, but is pressed into the barometer-tube by the air; the moon is not set going about the earth by a rotation, but is hurled like a stone. Hereby, it usually happens that *one* somewhat

generalized conception is now made to comprehend several cases which, previously, were regarded as essentially different; and it is in this that the extended adaptation consists. Through such developments of experience, the fall of a stone, for example, appears as a special case of *planetary* motion, refraction and dispersion are included under one concept, fluorescence and phosphorescence become analogous, the differences between gases and vapors vanish, and so on.

The history of art of all times shows how, in this domain also, forms presenting themselves by chance are used in the creations of art; and Leonardo da Vinci has given instructions to the artist to notice in the chance forms of clouds, of smoke-stained walls, what is suitable for his designs and moods. This is a proceeding which has a certain relationship with those considered above. The musician also occasionally gets inspiration from irregular sounds, and occasionally one may also hear of a renowned musician who has been led by chance mistakes upon the piano to new motifs of great value, of a tuneful or harmonious nature.

6. Special rules for bringing about a favorable chance, be it in a physical or in a mental occurrence, cannot from the nature of the case be given. The only thing that one can commend, a thing commended by all eminent investigators, is profound study, often repeated and varied, of the field of research; this creates, so to speak, the opportunity for favorable chance. Characteristic, in this connection, is a pronouncement by Helmholtz:

> As I have been fairly often in the uncomfortable position of being obliged to wait for favorable ideas, I have gained some experiences with respect to them, as to when and where they came to me, which may probably be useful to others as well. They often steal quietly into the circle of one's thoughts, without their significance being perceived at the very beginning; then later, a mere chance circumstance helps one at times to discern when and under what circumstances they have come; except for this they are there, whence one knows not.
>
> But, in other cases, they enter suddenly, without effort, like an inspiration. So far as my experience goes, they never came to the brain when tired, nor at the study table. I was always bound first to have turned my problem this way and that, considering it from all sides, to such a degree that I had a synoptical view of all its twists and complications in my head, and could run through them easily without writing. To accomplish this is, indeed, generally impossible except after prolonged previous work. Then, after the fatigue resulting from this has passed away, an hour of complete bodily vigor and peaceful well-being must have occurred before the fortunate ideas came. Often in the morning, *on awakening*, they were there, as Gauss has also noted.[5] But,

they came with special facility on convenient easily climbed wooded hills in sunny weather. However, the least amount of alcoholic beverage appears to frighten them away.[6]

It is obvious that by varied, exhaustive study of a domain, the *known* relations continually become more familiar, and make continually less demand on the attention: which, in consequence, returns all the more easily to *new* relations. It is indeed wonderful how much that is new one still perceives in an often considered subject.

CHAPTER XXXIII

THE PATHS OF INVESTIGATION

1. Whoever has busied himself with research or with the history of research cannot surely believe that discoveries are made according to the Aristotelian or Baconian schema of "induction" — by the enumeration of cases which agree with one another. If they were so made, discovery would be, indeed, a very easy affair. The discoverer, rather, *sees* the facts of which his discovery introduces the knowledge. Liebig[1] has expressed this, though in a different form; and, at the same time, has emphasized the close relationship that there is between the work of the artist and the work of the investigator. Liebig's exposition seems correct in essentials, though many objections may be raised against his manner of expression.

By this *seeing* we do not mean any mystical process. Any fact which possesses the charm of novelty in itself, or which is connected with an intellectual or practical interest, detaches itself from its surroundings, enters with greater brightness into our consciousnesses, and soon also becomes the condition to which its appearance is tied. In an objective respect, it is always the associative connections with the contents of the memory which bring this about, and, in a subjective respect, it is by fine sensitiveness to traces of these connections that this process is made possible.

All natural science begins with such intuitive knowledge. The expansion of heated air, electrification by friction, the periodicity and the polarization of light are examples. Schönbein's discovery of ozone by considering the association of a strong oxidizing power with a certain smell is a typical case, and was mentioned by Liebig.

2. The *seeing* of a fact must by no means be confined to what is immediately perceptible by the senses. Very abstract relations may also be *seen*. Consider that science has arisen from practical life, in which we are not only passively perceptive but are in active intercourse with nature, accumulating the useful and repelling the harmful. Often a fact is first expressed directly as a reaction to such an activity. What is seen may be the relation of different reactions. Thus we find, for example,

that to a volume, v, and a temperature, t, of a unit mass of gas corresponds an expansive force, p.

The senses are not so well adapted to the furtherance of knowledge as to the perception of the most important conditions of life. It is soon noticed in practical or technical life that, on account of the influencing of the organs by numerous indefinite circumstances, immediate sense perception does not always furnish a sufficiently trustworthy characteristic for the actual physical behavior of our surroundings; this was discussed in detail with regard to the concept of temperature. Something may look like gold, but chemical test alone is decisive. A carpenter, for example in building a hut, may think, on glancing at it, that a tree-trunk is long enough, but may find it actually too short. I myself cannot even maintain the accuracy of my own conception of magnitude with sufficient confidence, and much less could I offer the results of it to another without actual corroboration by the measuring rod. Accordingly we arrive at comparing facts with facts, and not with recollections of facts. Measuring with fingers, hands, feet, and steps, and the application of measuring rods of all kinds arise in this way. But what is still left to direct sense perception and is regarded as sufficiently reliable is *equality* or *inequality* with a measuring rod or with a multiple or fractional part of it. Measuring and counting belong to the most important and finest reaction operations. By means of them, homogeneous cases are distinguished, while the grosser qualitative reaction comes into play in cases of non-homogeneity.

3. Mathematical and geometrical knowledge too can be a matter of *seeing*. This agrees very well with the view which a distinguished French mathematician expressed in conversation with Liebig. Historical investigations leave no doubt whatever that the properties of similar figures, the theorem of Pythagoras, and the like, have been discovered by empirical methods. When we see that the numbers (a, b, and c) which correspond to the measurements of the sides of a right-angled triangle satisfy the equation $a^2 + b^2 = c^2$, we see a relation between two reactions, just as when we find that sodium which decomposes water, gives, when combined with chlorine, common salt.

If, in some domain, we see some relation of reactions which wins our interest, then the question will arise as to how far it is valid. Only then are different cases compared according to the Aristotelian schema and their agreement and difference examined. It sounds quite probable that, as a Greek legend has reported, it was first observed — in Egypt — that the shadows of all vertical objects, such as staffs, for a certain position of the sun, were simultaneously of the same lengths as the objects. Afterwards, Thales is supposed first to have discovered that, at any other position of the sun, staffs and shadows were no longer of the *same* length, but were in all cases simultaneously in the same *ratio*.

There will have been a similar procedure in the case of the Pythagorean theorem. The ancient Egyptians were practical surveyors, and must soon have noticed that, for the determination of the area of a rectangular field, it is not necessary actually to apply the unit square and to count, but that the result is attained more quickly and more easily by multiplication of the numerical measures of the sides. Simple examples such as rectangles with sides of measures 3 and 4, may have been used, and the right angled triangle with sides of measures 3 and 4, which is formed by diagonal section of such a rectangle, would have been conceived as half of the rectangle in question. This diagonal was now found — by actual measurement to be of the exact length 5.[3] Ropes of the lengths 3, 4, and 5 must thenceforth have served for the simple and practical marking of right angles.

If Pythagoras — incited by the Babylonians — occupied himself by experimenting with the properties of square numbers, he would notice that $5^2 - 4^2 = 3^2$. Now the question must have inevitably arisen as to whether these two distinct reactions, the geometrical and the arithmetical, which first appeared as connected in Pythagoras's mind, are associated in only *one* triangle. Pythagoras knew that the series of odd numbers represents the differences of successive square numbers, and that among the odd numbers or sums of consecutive odd numbers, square numbers are found. In this way other cases of arithmetical equalities and triangles corresponding to it could be found, and the triangles always proved to be right-angled. Finally, it was seen that, in the simplest case of an isosceles right-angled triangle — one in which the two sides including the right angle are of the same length — the geometrical equivalent of the arithmetical equation comes out clearly but is itself not representable by (rational) *numbers*. This would finally have led to a general *geometrical* proof of the theorem.[2] The theorem is thus true of *all* right-angled triangles; but, on the contrary, not for obtuse-angled or acute-angled ones. For the later kinds, as is well known, another analogous theorem was found later.

Both geometrical examples show clearly how the insight gained in a special case becomes widened and generalized, and how, by efforts towards extension, a restriction and specialization to definite conditions comes to light. In the domain of physics we see the same process. A special observation is extended to Richmann's rule of mixture, and this again to Black's rule of mixture; Galileo's law of falling or projection is extended to Newton's law, and is at the same time specialized by this last law, and so on.

4. The process of discovery by "induction" is frequently regarded as essentially different from the process of discovery by "deduction". Yet the process of deduction is based upon single acts of *seeing*, which, however, are combined together into a more extensive act only in the case of deduction. An example may elucidate this. I find that the span of the compass from the center of a circle to the circumference (reaction A) is contained six times in the circumference (reaction B). I may further observe that the hexagon formed may be resolved into six equilateral triangles by straight lines from the angles to the center, and that any side of these triangles is at the same time equal to the radius of the circle and to a side of the hexagon (reaction C). The act of seeing: "A is connected with B" is resolved into smaller steps by interpolation of C.

The same complex of ideas may assume different forms. In the first place, it may be new to me, surprising perhaps, that the radius is contained six times in the circumference. By splitting up the hexagon, I *see* that the side of the hexagon and the radius of the circle are the same. Whether I have seen, now for the first time, the properties of the hexagon and of the equilateral triangle, or whether they were already known to me, the interpolation of C between A and B will, especially in the latter case, appear as an elucidation. Secondly, I may be already familiar with the properties of the equilateral triangle and, on joining six of them together into a hexagon, I may come upon the allied notion of describing, in thought or actually, a circle from the angular point which is common to all as center, through the other angles. Then I have "discovered by inductive paths", as we are accustomed to say, that the radius is equal to the side of the hexagon. If, thirdly, the whole complex of thoughts is already familiar to me, then, if I begin with the latter theorem and perform one of the two above processes for the conviction or instruction of another person, I have carried out a "deductive proof" — and a "synthetic" one if I choose that last mentioned, an "analytic" one if I choose the previously mentioned process. In the first case the theorem is found as result of a condition; in the second case the condition for the theorem is found.

5. In an analogous manner, any other geometrical theorem — that, for example, which states that the sum of the opposite angles of a quadrilateral inscribed in a circle is equal to two right angles — may be used for the elucidation of this state of things.

In the domain of physics, we meet with the same phenomena, only in a less simple form. Arago found that a rotating copper disk (A) moves a magnetic needle (B). By Faraday's later discovery, new elements (C) are interpolated between A and B. This discovery was that, in parts of the conductor moved relatively to the magnet, currents are generated which (according to Oersted) exert forces upon the magnet, and these forces (according to Lenz) act in the opposite direction to the motion. The connection of A and B is explained by C, which, however, involves constructions of the same kind. If C had been known, previously not only partially but wholly, deduction would have led to the discovery of the connection of A and B. The processes of *seeking* are always the same in essentials, whether they occur singly or in combination; whether the connection of A and B is immediately seen or whether intermediary terms between A and B are seen.

Remembrance of the above geometrical examples favors the conviction that in many cases the same discovery can be made both by experiment and by theory; that is to say, between these two methods of investigation the great gulf customarily assumed does not exist. Wherever my thoughts are sufficiently adapted to the facts, I shall meet with middle terms interpolated between A and B, both in my thoughts and in experiment, in so far as the terms C, first to be found in experience, are not entirely new. The theorist experiments, as Liebig said in the address mentioned above, just as freely with his thoughts as the experimenter with facts. He has occasion, when comparing his results with the facts, to test or to rectify the concepts with which he started; Galileo, Newton, and Carnot furnish numerous examples of this. On the other hand, Gauss incidentally said that experimenting is so interesting because we are, properly speaking, continually experimenting with our own thoughts.

If an experiment is to be made in any way, the experimenter must be guided by certain ideas, however incomplete, in regard to the behavior of the facts which he verifies, refutes, or corrects; while even the most abstract investigation of the mathematician cannot wholly dispense with observation and experiment.[3] The intimate relation between thought and observation, which is especially characteristic of modern research, has already been referred to.

6. The character and course of development of science becomes more intelligible if we keep in mind the fact that science has sprung from the needs of practical life, from provision for the future, from techniques. From surveying there developed geometry; from observations of the stars for domestic and nautical purposes astronomy; from metallurgy alchemy and chemistry. The intellect, strengthened by work in an alien service, soon asserts its own needs. Thus, purely intellectual interest gradually makes the knowledge of large domains of facts accessible; and often this knowledge suddenly and quite unexpectedly acquires technical value. Think of the paths which have led, through many centuries, from the phenomena of rubbed amber to the dynamo and to the transmission of power; think of the uses of liquefied carbonic aid gas, which was liquefied from purely intellectual interests; and so on. On the other hand, if technical arts or industry have taken possession of a field of facts, they set up experiments of such vastness and precision that they cannot be carried out in another way, and thus supply science with new facts and abundantly repay science for its help.

The investigator strives for knowledge of a field of facts; it is all the same to him *what* he finds. The technician strives for a definite purpose; he leaves everything alone which does not seem subservient to his ends. Because of this, the thought of the technician is more one-sided and narrower. It is similar to that of a geometrician who seeks the solution of a problem of construction. Yet the technician, when examining his resources, is often an investigator; an investigator, when pursuing definite aims, is often a technician. The investigator strives for the removal of an intellectual discomfort; he seeks a *releasing thought.* The technician wishes to overcome a practical discomfort; he seeks a *releasing construction.* Any other distinction between discovery and invention can scarcely be made.

The fragmentariness of all our knowledge is explained by the fact that all science had in the beginning, a practical aim. To the facts which were in the center of practical interest, the nearest knowledge attached itself.

As a rule, a special side or property of the facts is of practical interest. To this property investigation is confined. Facts which agree in possessing this property are treated as the same or homogeneous; those which differ in this property are treated as dissimilar. If that property is first expressed by a special reaction, this reaction serves, so to speak, for the enrichment of the facts immediately at hand, and also, on the other hand, for the simplification of them, by allowing more attention to be given to the result of this reaction only. Thus practical needs impel us to abstraction.

7. A property of a fact, which is very important to us, may not be accessible to direct examination by the reaction R. Practical interest then demands that we look for other reactions A, B, C, by whose combination R is determined — and, moreover, *uniquely* determined. Thus we determine the difference of the heights of two places not directly measurable by their barometric heights, the length (b) of the inaccessible side of a triangle by that of an accessible side (c) and its adjacent angles (α, β), the perhaps impracticable manometric measurement of pressure of a gas by its mass, volume and temperature, and so on. The desire to determine properties of facts in advance or to complete in thought partially given facts thus impels us to look for groups of reactions (A, B, C, D) which are so connected that, when a certain number of them are given, the remaining ones are uniquely determined. Trigonometrical formulas, the law of Mariotte and Gay-Lussac, and so on, are examples of this.

Two systems of properties or reactions which uniquely determine one another, whether qualitatively or quantitatively, we shall designate as equivalent. Thus, for a rectilinear motion under force, where m, s, t, v, and p denote mass, length of path, time, final velocity, and force, respectively, the determination of any three of these quantities, among which is m or p, is equivalent to the determination of any other three; for the two still remaining are always codetermined with the three specified above. For a plane triangle with sides A, B, and C, and opposite angles α, β, and γ, the determination of A, β, and γ, is equivalent to that of B, C, and α, the knowledge of such systems of equivalent properties is of the highest value for deductive thought. Mann[4] has shown this in the case of geometry and his exposition may be applied to physics or any other domain of science. Every deduction, every correct explanation, every proof, is based upon the substitution (step by step) of properties by equivalent ones. By this, finally, certain properties are shown to be equivalent where this equivalence was not directly manifest.

It is self-evident that such an equivalence is always *mutual*. If, for example, we lay down the proposition: "In a quadrilateral (A) whose angles lie on a circle (B), the sum of the opposite angles is equal to two right angles (C)"; then conversely it may be said that: "In a quadrilateral (A) in which the sum of the opposite angles is equal to two right angles (C), the angles lie on a circle (B)". An incorrect conversion merely arises from the omission of a portion of a determination. Thus, the pressure (p) of a given mass of gas is determined by the volume (v) and the temperature (t), and v is also determined by p and t, but not v and t by p alone. The example of false conversion of a geometrical proposition which was given by Mann may also be construed in the same way.

Mann's discrimination of properties having more and less weight, contradictory properties, and properties independent of one another, may be at once applied to physics, and we will not go into further details. The relation of superordinate, subordinate, disparate, and exclusive concepts is sufficiently familiar from logical disquisitions. Only with respect to the independence of properties, stress may be laid upon the importance for clear understanding. A succession of most significant discoveries establishes such *independence*, and thus clears our vision from prejudicial and disturbing inessentials. Think, for example, of the parallelogram of forces, the principle of the mutual independence of forces. The whole of medieval research is obscured by the assumption of dependences where none exist.

8. The unique determination of certain properties $M, N, O, \ldots,$ of facts, which properties are important to us, by others $A, B, C, \ldots,$ which are more easily accessible, is, therefore, what is aimed at in science. With respect to variable $A, B, C, \ldots,$ an analogous determination may become necessary, and this determination is incapable of completion if we are not finally led to invariable properties. But invariable properties would preclude variations in our surroundings. In practice, we either have $A, B, C, \ldots,$ completely in our power — so that problems with respect to these fall away — or $A, B, C, \ldots,$ come and go in ways independent of us or even beyond our ken. The fall of a

meteorite, for instance, so that every point of attack of the problem is missing. Thus the practical limitations of science are everywhere conspicuous.[5]

9. Practical needs require a familiar and sure application of science. This application is brought about by tracing back new relations to those already known. A further means consists in the simplifying and schematizing of facts; that is to say, in the representation of them by images which contain only the essential features, and in which everything taking away the attention — everything superfluous — is left out. Thus, we think of a planet as a point and the path of an electric current as a line.

If it is directly shown that a fact, *A*, behaves in practice like one more familiar to us, *B*, then *B* may be a personal activity, an operation of calculation, or a geometrical construction. The spaces traversed by a falling body behave like the numbers obtained by squaring the numbers which measure the time; the temperatures of mixtures behave like arithmetical means, and so on. The more familiar such operations are to us and the simpler they are, the better are we satisfied, the less is the need for further explanation, and the better do we understand the situation.

10. The whole peculiarity, certainty, and familiarity of arithmetical operations is transferred to the knowledge of facts represented by them. This peculiarity was well known to Kant, but the investigation about its origin may be carried further than was done by him.

In the first place, it is clear that counting is our *own* ordering activity, and that arithmetical principles simply contain experiences concerning this activity — since they only express the equivalence of one ordering activity with another. That we are concerned here, as in every other case, with *experiences*, is indubitable. The experiences, however, are wholly independent of the physical aspect of things, and which, consequently, affirm nothing whatever with respect to physics. Therefore, it is scarcely understandable how people have, at times, come to believe the extraordinary notion that an "*a priori* developed" arithmetic prescribes laws to the world. Of the objects to which we apply arithmetic, we *only* assume that they remain the same.

Yet the simplest arithmetical proposition — and all more complicated propositions may be traced back to such simple ones — have, it must be admitted, a peculiar property. If I think of the equation $2 \times 2 = 4$, the idea of 2×2 is one psychic act, 4 another, and the equating of the two a third. But I notice that I have already conceived, simultaneously with the sensible or conceptual image of 2.2 points, 4 points also. Any other relation — the opposite, so to speak — is unthinkable.[6] Likewise, I may imagine an angle of a triangle to be increasing and, by a special act of attention, notice that the opposite side simultaneously increases. But I find that, in the mental picture of the increasing angle, the increasing side is already contained. Physical experiences behave differently. A glowing (luminous) body is also hot. But I am not obliged to perceive or conceive both properties in one sense-act, as in the above cases. I can also find bodies which are hot without being luminous, and conversely. I may perceive two material points, but that they attract each other is first taught me by a special act of perception. The inseparability and simplicity of sensible acts of experience, which lie at the basis of certain mathematical experience, along with the ease of repeating the experiences, creates a special feeling of security.

The case with respect to geometry is somewhat more complicated than that with respect to arithmetic. Our visual space is not identical with geometrical space. Yet, the former corresponds to the latter in such a way that every point of the one is coordinated to a point of the other, and to a continuous displacement in the one corresponds a continuous displacement in the other. All questions of order — or topological questions — thus become capable of settlement in the imagination, without the help of physical experience. But a good deal of our geometry is an actual *physics* of space. Without assuming the use of a rigid measuring rod, propositions of congruence cannot be proved. It is often strangely enough urged against the conception of geometry as a physics of space that the geometrical concepts are never exactly represented in the physical world but that geometrical propositions are, nevertheless, absolutely accurate. On the contrary, it is to be remarked that geometry idealizes its objects in just the same way as does physics, and that the conclusions are valid precisely in the same degree of approximation as the assumptions. If I rotate a thin, curved, rigid wire about two fixed points of it, the remaining points leave their place. The slighter the curvature, the less do the remaining points referred to change their position. In so far as I will or can completely disregard the curvature, I will or can disregard the change of position by rotation. The straight wire and the straight line are ideals just as much as is the perfect gas. In so far as I regard the ideal as attained, is the straight line determined by two points. With the same approximation as I can or will look upon the angles at the base of a triangle as equal, can or will I look upon the opposite sides as equal. Another meaning than this is not possessed by geometrical propositions. In applications, I allow myself the same liberty that I allow myself in the applications of arithmetic, by regarding the physical units as equal.

11. On account of the differences previously considered, it may be questioned whether the setting up of axioms in the domain of physics has the same legitimacy as in that of geometry.[7] But if we consider the quantitative side of the matter and reflect that without the empirical acts of counting and measuring nothing is begun even in geometry, then the foundations of the two domains appear as related in so far that, in both of them there is no such thing as an axiom properly so called.

If I find that a physical fact behaves like my calculation or my construction, I may not at the same time assume the opposite. Thus, I must expect the physical result with the same certainty with which I regard the result of the calculation or construction as correct. But this logical necessity is obviously to be distinguished from the necessity of the assumption of the parallelism between the physical fact and the calculation, this assumption being invariably founded upon a common experience of our senses. The strong expectation of a known result, which appears to the natural philosopher as a necessity, is based upon the practice of firmly associating the conception of facts with that of their behavior. The relation which, in the geometrical intuition, subsist of itself, is here gradually created artificially. In this way, that develops which we are accustomed to describe as a "feeling for causality".

12. It has already been mentioned that quantitative scientific determinations are to be regarded as simpler and at the same time more comprehensive special cases of qualitative ones. Zinc gives, when acted upon by dilute sulphuric acid, a colorless solution; iron gives a pale bluish green one; copper gives a blue one; platinum gives none at all. In every case I require a special determination. If a gas is enclosed in a vessel provided with manometer and thermometer, I find, for different thermometric indications, different positions on the manometric column. I have here again a series of different cases which, however, have great similarity among themselves and differ only in the number of the

thermometric degrees and the number of units of length of the manometric column. If I enter in a tabular form the position of the manometric column for every position of the thermometric one, I am then attending only to the schema of the aforesaid chemical arrangement. But I am at an advantage in that the thermometric and manometric positions each form a series between whose terms I can discriminate as finely as I please by a mere application of the number system, and without any further discovery. A further glance shows me that the separate cases represented in the table exhibit great similarity among themselves, that every position of the manometric column can be obtained from the thermometric position by a simple numerical operation, that this operation agrees in kind for all cases, and that, therefore, the whole table may be replaced and rendered unnecessary by the comprehensive rule for its construction:

$$p = p_0 (1 + t/273).$$

If a ray from the air passes into gas, it is refracted. Another ray of greater angle of incidence is refracted more. I may place together, in a table, the angles of incidence α with the corresponding angles of refraction β for many cases; and this table may be refined and extended to any degree I please. But if I imitate the behavior of the ray with Snell's or Descartes's formula, $\sin\alpha/\sin\beta = n$, I replace the table in a very simple manner. If I am accustomed to conceive the formula instantly applied to the incident ray, the refracted ray is given with a readiness and certainty altogether different from that met with if I should first look for the refracted ray in the table or even in the picture in my memory. The certainty is still greater and the need for a wider explanation of the process is still better satisfied if I succeed in grasping the formula even more simply as the mere expression of the ratio of the velocities of light, $v/v' = n$, in the two mediums. The refinement to any degree wished, the easy survey and manipulation of a whole continuum of cases, as to whose completeness we are at the same time satisfied,[8] constitutes the advantage of such quantitative determinations. It is natural that this should be striven for as an ideal wherever we can approximate to it, and that a science which has reached it is regarded as completed. There is no other difference between quantitative and qualitative determinations than is described here.

13. The processes by which science develops are very different in kind,

and heterogeneous also are the contributions of investigators towards its structure. This may be illustrated by some striking examples. In Newton, we find capabilities which are usually unequally apportioned to different investigators united in a superlative degree. In optics, he *saw* at once a series of remarkable facts: the unequal refrangibility of differently colored light, its periodicity, its polarization. In his ability to represent, free from disturbing conditions, facts once known, he proved himself a pre-eminent technician. His deductive power was predominantly manifested in his astronomical and mechanical work. For Kepler's motions he found the condition, the interpreting conception, in the modified assumption of gravity, and could reconcile with the facts the most remote consequences of this conception and then return and verify these facts. Though he, at one time, schematized the planet as a point, he did not hesitate to drop the schema for the phenomena of tides, where particulars for which that schema cannot provide are in question, just as he dropped the usual schema of one light ray in order, by finer discrimination of details, to see into the phenomenon of dispersion.

14. We meet, perhaps, a still richer manifestation of inductive power in Faraday than in Newton. Only think of the multitude of facts into which he first *saw*, and how he could designate the current by a single striking phrase — "a line of force". And if the capacity for deduction was less marked in him, this may be due to the method that he acquired by education. For Maxwell, who understood best how to read him, did not fail to recognise this capacity.

But even a far more limited endownment can achieve very fruitful results. Thus we have, in the cases of Lambert and Dalton, learned to know two men whose gift for observation was greatly inferior to their ability to deduce, construct, and schematize. In Fraunhofer and Foucault, we meet with a peculiar idiosyncrasy: these men were predominantly ingenious technicians. In addition to retaining noteworthy and representative facts, they could also utilize them, but preferably for the pursuance of aims, clearly defined to them, which were in part purely practical.

CHAPTER XXXIV

THE AIM OF INVESTIGATION

1. If we wish to say that *a complete theory* is the final aim of research, the word "theory" must not be used in the sense of §2 of Chapter XXVII, which is that in which it is generally used and which is that of bringing into parallelism a domain of facts with another more familiar domain. Rather must we understand by this word a complete and systematic representation of the facts. So long, however, as this final aim is not yet attained, "theory" in the former sense always signifies an approximation towards a "theory" in the latter sense; and it marks a progress in so far as it gives a more complete picture of the facts than would be possible without its help. So long as a representation is not yet complete, a "theory" in the former sense will therefore have a certain justification as an automatic arranging, constructive, *speculative* element.

2. But the ideal to which every scientific representation tends (even though we may only speak asymptotically) is to embracing a complete description of the facts, more than all speculations can give, and containing one of the adventitiousness, superfluity, and deceptiveness which enters into every speculation. This ideal is, *a complete and clear inventory of the facts of a domain.* It must be simply, handily and economically aranged for use, and so plain in construction that, where possible, it can be retained in the memory without further help. The things which we at present can contribute to such elucidation are only essays towards, and fragments of, a future representation — like the questions of d'Alembert (or Lagrange) for instance, which comprise all possible dynamical facts, or the equations of Fourier which comprise all conceivable facts of thermal conduction. Whoever has mastered the easily remembered equations of Fourier has a survey of the facts of conduction and the certainty that the domain is as exhaustively represented as (say) chemical facts by a complete analytical table. But this table may be deranged by a single newly discovered fact, whereas Fourier's equations contain no explicit description but the rule for making one for every possible case, that is for an infinite number — a continuum in fact — of cases. By the resolution into elements of volume

and time and the knowledge of the simple processes going on in them (from which these equations start), we are enabled to compound with sufficient accuracy, from such elementary processes, any fact that occurs; and then to build up its course, step by step in thought, by simply calculating and constructing. Certainly this is what Riemann meant by saying that true (everywhere applicable) natural laws are only to be expected in the infinitely small (not perchance in the too special and individual, integral case). Equations are thus much more simple to manipulate than the table spoken of; and the repetition of the same few and simple motifs in every application creates a logical-aesthetic impression akin to that which the multifarious employment of the same motif in a work of art produces.[1]

NOTES

INTRODUCTION BY MARTIN J. KLEIN (pp. vii–xxi)

The reader who wants to consult more recent historical studies of the material treated by Mach in this book might well begin with the discussions of individual scientists in the *Dictionary of Scientific Biography*, ed. Charles C. Gillispie, Sixteen Volumes (New York, 1970–1980).

Some additional suggestions are listed below.

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[1] A. Einstein, 'Autobiographical Notes', in *Albert Einstein: Philosopher-Scientist*, ed. P. A. Schilpp (Evanston, Illinois, 1949), p. 21.

[2] See, for example, John T. Blackmore, *Ernst Mach. His Life, Work, and Influence* (Berkeley, 1972).

[3] See especially Erwin Hiebert's valuable discussions of Mach's work.

(a) 'Mach's Philosophical Use of the History of Science', in *Historical and Philosophical Perspectives of Science*, ed. R. H. Stuewer; Minnesota Studies in the Philosophy of Science, Vol. V (Minneapolis, 1970), pp. 184–203.

(b) 'Mach's Conception of Thought Experiments in the Natural Sciences', in *The Interaction between Science and Philosophy*, ed. Y. Elkana (Atlantic Highlands, New Jersey, 1974), pp. 339–348.

(c) 'An Appraisal of the Work of Ernst Mach: Scientist-Historian-Philosopher', in *Motion and Time, Space and Matter*, eds. P. K. Machamer and R. G. Turnbull (Columbus, Ohio, 1976), pp. 360–389.

[4] E. Mach, *Knowledge and Error*, transl. T. J. McCormack and P. Foulkes (Dordrecht, 1976), pp. xxxii–xxxiii.

[5] E. Mach, *The Principles of Physical Optics. An Historical and Philosophical Treatment*, transl. J. S. Anderson and A. F. A. Young (Reprinted New York, n.d.), p. vii. This preface is dated July, 1913.

[6] See below, p. 1.

[7] E. Mach, *The Science of Mechanics: A Critical and Historical Account of Its Development*, transl. T. J. McCormack. Sixth American Edition (New York, 1960), p. xxii. This book will be referred to briefly as *Mechanics.*
[8] See below, p. 1.
[9] H. Hertz, *The Principles of Mechanics*, transl. D. E. Jones and J. T. Walley (Reprinted New York, 1956), pp. 6—7.
[10] *Ibid.*, p. 35.
[11] E. Mach, *History and Root of the Principle of the Conservation of Energy*, transl. P. E. B. Jourdain (Chicago, 1911), pp. 17—18. This work appeared first in 1872. It will be referred to briefly as *Energy*.
[12] W. James quoted in Blackmore, *op. cit.* note 2, pp. 76—77.
[13] See Mach's descriptions of his demonstration apparatus in his *Mechanics.* Also, see the descriptions in his *Popular Scientific Lectures*, transl. T. J. McCormack (Chicago, 1895).
[14] See below, p. 60.
[15] See below, pp. 53—57. See also, *Mechanics*, pp. 271—289.

For a recent critical analysis of Mach's arguments against absolute motion see Howard Stein, 'Some Philosophical Prehistory of General Relativity', in *Foundations of Space-Time Theories*, ed. J. Earman, C. Glymour, and J. Stachel; Minnesota Studies in the Philosophy of Science, Vol. VIII (Minneapolis, 1977), pp. 14—21.
[16] See below, Chapters IV, V. Note that on p. 75 Mach refers to 1/3 as "a relative irrational number, as compared with the decimal system." (!)
[17] See below.
[18] For an introduction to modern Newtonian scholarship see I. B. Cohen, 'Isaac Newton', in *Dictionary of Scientific Biography*, ed. C. C. Gillispie (16 Volumes, New York, 1970—1980), Vol. X, pp. 42—101.
[19] E. Mach, *Mechanics*, pp. 236—237.
[20] See below, p. 150.
[21] A. Einstein, 'Ernst Mach', *Physikalische Zeitschrift* **17** (1916), 101—104. Reprinted in K. D. Heller, *Ernst Mach. Wegbereiter der modernen Physik* (Vienna, 1964), pp. 151—157.
[22] E. Mach, *Energy*, p. 74.
[23] "To criticize is to appreciate, to appropriate, to take intellectual possession, to establish in fine a relation with the criticized thing and make it one's own." Henry James, *The Art of the Novel. Critical Prefaces* (New York, 1934), p. 155.
[24] J. Willard Gibbs, 'Rudolf Julius Emanuel Clausius', in *The Scientific Papers of J. Willard Gibbs* (Reprinted New York, 1961), Vol. II, p. 262.
[25] J. E. Trevor, Review of E. Mach, *Die Prinzipien der Wärmelehre, Journal of Physical Chemistry* **1** (1896), 430—431.
[26] E. Mach, *Mechanics*, pp. xxvi—xxvii.
[27] A. Einstein to M. Besso, 6 January 1948 in A. Einstein, M. Besso, *Correspondance 1903—1955*, ed. P. Speziali (Paris, 1972), p. 391.
[28] A. Einstein, 'Autobiographische Skizze' in *Helle Zeit — Dunkle Zeit*, ed. C. Seelig (Zürich, 1956), p. 10.
[29] A. Einstein, *op. cit.* note 1, p. 19.
[30] H. Hertz, *op. cit.* note 9, Author's Preface.
[31] E. Mach, *Mechanics*, pp. 596—597.

[32] E. Mach, *Energy*, p. 41.
[33] A. Einstein, *op. cit.* note 1, p. 21.
[34] A. Einstein, *op. cit.* note 27, p. 390.
[35] E. Mach, *Energy*, p. 49.
[36] *Ibid.*, p. 49.
[37] *Ibid.*, p. 50.
[38] See below, p. 389.
[39] See below, p. 335.
[40] A. Einstein, *Investigations on the Theory of the Brownian Movement*, ed. R. Fürth, transl. A. D. Cowper (Reprinted New York, 1956), p. 2.
[41] A. Einstein to E. Mach, 9 August 1909. This letter, as well as all other letters from Einstein to Mach, is reproduced in Friedrich Herneck, '*Die Beziehungen zwischen Einstein und Mach, dokumentarisch dargestellt*', in his book *Einstein und sein Weltbild* (Berlin, 1976), pp. 109—155. See, in particular, p. 132.
[42] *Ibid.*, p. 134. The Mach-Einstein relationship is also discussed by Gerald Holton, 'Mach, Einstein, and the Search for Reality', in his book *Thematic Origins of Scientific Thought. Kepler to Einstein* (Cambridge, Massachusetts, 1973), pp. 219—259.
[43] See Blackmore, *op. cit.* note 2, pp. 319—323.
[44] Mach, *Mechanics*, pp. 305—317.
[45] Mach, *Energy*, pp. 41, 59.
[46] See below, p. 296.
[47] A. Einstein, 'Zur allgemeinen molekularen Theorie der Wärme', *Annalen der Physik* **14** (1904), 354—362.
[48] A. Einstein in *La théorie du rayonnement et les quanta*, ed. P. Langevin and M. de Broglie (Paris, 1912), p. 436.
[49] A. Einstein, *op. cit.* note 1, pp. 44—45.
[50] H. Stein, *op. cit.* note 15, pp. 3—14, 39—49. See also his 'Newtonian Space-time', *Texas Quarterly* **10** (Autumn 1967), 174—200. Huygens used the relativity principle to derive some of his results on collisions. See 'Christiaan Huygens' *The Motion of Colliding Bodies*', transl. R. J. Blackwell, *Isis* **68** (1977), 574—597. It is worth noting that Mach discusses Huygens' analysis of collisions and points out his use of the relativity principle in his *Mechanics*, pp. 403—417. Einstein never refers to this discussion, but one cannot help wondering if he was influenced by it. So far as I know Huygens was the only one who used the relativity principle constructively before 1905. (Einstein does, as is well known, refer explicitly to Mach's analysis of Newton's ideas on absolute motion. See note 15.)
[51] They were not consistent when the classical (or Galilean) transformation was used to relate the measurements in the two frames of reference.
[52] A. Einstein, 'Zur Elektrodynamik bewegter Körper', *Annalen der Physik* **17** (1905), 891—921. See p. 891.
[53] R. S. Shankland, 'Conversations with Albert Einstein', *American Journal of Physics* **31** (1963), 47—57. See p. 48.
[54] A. Einstein, 'Über das Relativitätsprinzip und die aus demselben gezogenen Folgerungen', *Jahrbuch der Radioaktivität und Elektronik* **4** (1907), 411—462. See p. 413.
[55] See below, pp. 251—268.
[56] M. J. Klein, 'Thermodynamics in Einstein's Thought', *Science* **157** (1967), 509—516.

AUTHOR'S PREFACE TO THE FIRST EDITION (pp. 1–2)

[1] [First German edition 1883. 6th English edition 1960. *Ed.*]
[2] [*History and Root of the Principle of the Conservation of Energy,* 1911 (first German edition 1872), 'Ueber den Unterricht in der Wärmelehre', *Zeitschr. f. phys. u. chem. Unterricht,* I (1887), *Ed.*]
[3] [First German edition 1896. 3rd English edition reprinted 1910. *Ed.*]

AUTHOR'S PREFACE TO THE SECOND EDITION (pp. 3)

[1] [Russell's work was published in 1898, Stallo's in 1862. A German translation of the latter with a preface by Mach appeared in 1901. *Ed.*]

CHAPTER I (pp. 7–44)

[1] Similar considerations were early advanced by Sagredo (Letter to Galileo, February 7th, 1615). Cf. F. Burckhardt, *Die Erfindung des Thermometers,* Basel, 1867.
[2] *Heronis Alexandrini spiritalium liber,* Amsterdam, 1680.
[3] *Ibid.,* p. 53.
[4] According to Burckhardt (*op. cit.*) there is a German edition of this book bearing the date of 1608.
[5] *I tre libri de spiritali di Giambattista della Porta,* Naples, 1606.
[6] *Thaumaturgus mathematicus,* Cologne, 1651, p. 132.
[7] First edition, 1624.
[8] 'Zur Geschichte der Erfindung und Verbreitung des Thermometers', *Ann. der Phys. und Chem.,* Vol. CXXIV, pp. 163–178.
[9] Cf. Burckhardt, *op. cit.*
[10] [According to J. Rosenthal (*Ostwald's Klassiker,* No. 40, p. 71), Galileo invented the thermometer between 1592 and 1597, and to Ferdinand II, Grand Duke of Tuscany (see below), is due the modern form of the instrument.]
[11] *Experimenta Magdeburgica,* 1672.
[12] *Collegium experimentale sive curiosum,* Nuremberg, 1676.
[13] Cf. Reyher, *Pneumatica,* 1725, p. 193.
[14] *Traité des baromètres et thermomètres,* 1688.
[15] *Mémoires de l'Académie des Sciences, Paris,* 1699, 1702, 1703.
[16] *Historie de l'Académie des Sciences, Paris,* 1703, p. 50 *sqq.*
[17] Lambert, *Pyrometrie,* Berlin, 1779, pp. 29, 40, 74.
[18] *Op. cit.,* p. 47.
[19] *Journal de physique,* Vol. LXXXIX, 1819, pp. 321–346, 428–455.
[20] Burckhardt, *op. cit.,* p. 19.
[21] *Op. cit.,* p. 37.
[22] *New Experiments and Observations touching Cold,* London, 1665.
[23] *Philosophical Transactions,* Vol. XVIII, 1693, pp. 650–656.
[24] [Halley also drew attention to the fact that the temperature of the mercury in the barometer must be observed.]

[25] *Philosophia naturalis*, Padua, 1694.
[26] *Acta Eruditorum*, 1714.
[27] *Philosophical Transactions*, Vol. XXX, 1724, pp. 1–3; Vol. XXXIII, 1724, pp. 78–84, 114–118, 179–180.
[28] *Histoire de l'Académie des Sciences de Paris*, 1730, pp. 452–507; 1731, pp. 250–296; 1733, pp. 165–185.
[29] *Saggi di naturali experienze fatte nell' Accademia del Cimento*, Florence, 1667.
[30] Lambert, *op. cit.*, p. 121.
[31] *Mémoires de l'Académie de Paris*, Vol. XXI, 1847.
[32] *Annales de chimie et de physique* (5), Vol. XIX, 1880, pp. 345–385.
[33] *Annales de Chimie* (1), Vol. XLIII, 1802, pp. 137–175.
[34] *Nicholson's Journal*, Vol. V, 1802, pp. 241–244.
[35] Biot, *Traité de physique*, Vol. I, Paris, 1816, p. 182.
[36] *Ann. der Phys. und Chem.*, Vol. XLI, 1837, pp. 271–293, 558–559, Vol. XLIV, 1838, pp. 119–123. [Translated in Taylor's *Scientific Memoirs*, Vol. II, London, 1841, pp. 507–516.]
[37] *Ann. der Phys. und Chem.*, Vol. LV, 1842, pp. 1–27.
[38] *Mémoires de l'Acad.*, Vol. XXI.
[39] *Annalen der Phys. und Chem.*, Jubelband: . . . J. C. Poggendorff . . . gewidmet, 1874, pp. 82–101.
[40] [In America and England, sometimes called Charles's law or the law of Dalton and Gay-Lussac. In this translation, Mach's terminology is always followed.]
[41] *Op. cit.*
[42] *Nouvelles idées sur la météorologie*, Paris, 1787.
[43] *Essai sur l'hygrometrie*, Neuchatel, 1783.
[44] 'On the Constitution of Mixed Gases, etc.', *Mem. Manchester Lit. and Phil. Soc.*, Vol. V, 1801.
[45] *Manchester Memoirs*, Vol. V., 1801, p. 535. Compare Henry, *op. cit.*, p. 32. Dalton says: "and consequently [the particles] arrange themselves just the same as in a void space."
[46] The passage reads: "When two elastic fluids, denoted by *A* and *B* are mixed together, there is no mutual repulsion amongst their particles; that is, the particles of *A* do not repel those of *B*, as they do one another. Consequently, the pressure or whole weight upon any one particle arises solely from those of its own kind."
[47] *Ann. de Chim. et de Phys.*, Vol. XLIII, 1802, p. 172.
[48] Account in Brewster's *Edinburgh Encyclopaedia*, 18 vols., 1830.
[49] *Mem. Manchester Soc..*, Vol. V, 1801.
[50] Biot, *op. cit.*
[51] *Ann. der Phys. und Chem.*, Vol. XI, pp. 225–247.
[52] *Memoires de l'Acad.*, Vol. XXI.
[53] *Ann. de chim.* (2), Vol. XXI 1822, pp. 127–132, 178–182; *ibid.*, Vol. XXII, 1823, pp. 410–415.
[54] *Phil. Trans.*, 1823, pp. 160–189; *Ann. de chim.* (2), Vol. XXII, 1823, pp. 323–330, Vol. XXIV, 1823, pp. 397–403.
[55] *Ann. der Phys. und Chem.* Vol. XCIV, 1855, pp. 436–444.
[56] *Phil. Trans.*, 1869, part II, pp. 574–589.
[57] I have been unable to satisfy myself of the correctness of this report.

[58] *Sur les modifications de l'atmosphère*, Paris, 1772.
[59] *Annalen der Phys.* (ed. by Gilbert), Vol. XIV, 1803, pp. 297—305.
[60] *Ann. de chim.*, Vol. LXX, 1839, pp. 5—81; 'Le maximum de densité des liquides', *ibid.*, Vol. LXXIII, 1840, pp. 296—310.
[61] *Sitzungsberichte der Math.-Naturw. Classe der K. Akad. der Wiss.* (Wien), Vol. LXVIII, 1874. Abt. 2, pp. 463—475.
[62] Biot, *op. cit.*
[63] *Phil. Trans.*, 1785.
[64] *Ann. de chim.*, Vol. VII, 1817, pp. 113—154.
[65] We are obliged by the context to anticipate here the definitions of quantity of heat, specific heat, and caloric content, which will be critically discussed in a later chapter.
[66] Cf. Chapter I, §8.
[67] *A New System of Chemical Philosophy*, London, 1808, Part I, p. 13. Compare also Henry, *op. cit.*, p. 66.
[68] *A New System of Chemical Philosophy*, 1808, p. 126.
[69] *Ann. de chim.*, Vol. VII, 1817, pp. 150 *et seq.*
[70] Henry, *op. cit.*, p. 67.

CHAPTER II (pp. 45—61)

[1] Maxwell, *Theory of Heat*, 9th edition, London, 1888. I surmise that the remarks cited were contained in the first edition of 1871 [they were: pp. 32—33]; but I am unable to verify my conjecture, as I have had access only to Auerbach's translation of the fourth edition (1877). My considerations on the concept of mass were published in 1868 in the fourth volume of Carl's *Repertorium*, again in 1872 in my tract *Erhaltung der Arbeit* (Eng. transl. under the title *History and Root of the Principle of the Conservation of Energy*, Chicago, 1911), and finally in 1883 in my *Mechanics* (Eng. transl., Chicago, 5th edition 1942).
[2] Cf. Pfaundler, *Lehrbuch der Physik*, Vol. II, p. 2. See also §11 of Chapter I.
[3] See §6 of Chapter I.
[4] Lambert, *Pyrometrie*, p. 52.
[5] *Op. cit.*, p. 9.
[6] *Ann. de Chim.*, Vol. XLIII (1802), p. 139.
[7] *Ibid.*, Vol. VII, 1817, p. 139.
[8] *Ibid.*, p. 153.
[9] *Mechanische Wärmetheorie*, 1864, Vol. I, p. 248.
[10] Bolz, *Die Pyrometer*, Berlin, 1888, p. 38.
[11] Compare Mach, *Analysis of the Sensations*, Eng. transl., Chicago, 1897, pp. 18 *et seq.* Also Popper, *Die physikalischen Grundsätze der elektrischen Kraftübertragung*, Vienna, 1884, p. 16.
[12] *Science of Mechanics*, Eng. transl., 5th edn., La Salle, 1942, pp. 271—298 and 598.
[13] *Analysis of the Sensations*, Eng. transl., pp. 109 *et seq.*
[14] See §12 of Chapter II above.
[15] *Op. cit.*
[16] Cited by Dalton in the same work.
[17] See §18 of Chapter I above.

CHAPTER III (pp. 62—66)

[1] 'Scala graduum caloris et frigoris,' *Phil. Trans.*, No. 270, March and April, 1701, Vol. XXII, p. 824; *Newtoni Opuscula*, Lausanne and Geneva, 1744, Vol. II, p. 419.
[2] The original of the passage in question reads: "Locavi autem ferrum, non in aere tranquillo, sed in vento uniformiter spirante, ut aer a ferro calefactus semper abriperetur a vento, et aer frigidus in locum ejus uniformi cum motu succederet. Sic enim aeris partes aequalibus temporibus calefactae sunt, et calorem conceperunt calori proportionalem. Calores autem sic inventi eandem habuerunt rationem inter se, cum caloribus per thermometrum inventis; et propterea rarefactiones olei ipsius caloribus proportionales esse recte assumpsimus."
[3] *Histoire de l'Académie*, 1703, p. 6.
[4] *Journal des Mines*, 1804, Vol. XVII, p. 203.
[5] Cf. the Chapter (VI) on the Conduction of Heat.
[6] *Pyrometrie*, pp. 184—187.
[7] Black, *Lectures on the Elements of Chemistry*, 2 Vols., Edinburgh, 1803.
[8] A. Weinhold, 'Pyrometrische Versuche', *Annalen der Phys. und Chem.*, Vol. CXLIX, 1873, pp. 186—235.
[9] C. H. Bolz, *Die Pyrometer*, Berlin, Springer, 1888.
[10] L. Holborn and W. Wien, 'Ueber die Messung hoher Temperaturen', *Annalen der Phys. und Chem.*, Vol. XLVII, 1892, pp. 107—134.
[11] C. Barus, *Die physikalische Behandlung und die Messung hoher Temperaturen*, Leipzig, 1892.
[12] See the works cited above.
[13] *On the Conservation of Solar Energy*, German translation, Berlin, 1885, p. 144.

CHAPTER IV (pp. 67—72)

[1] M. Cantor, *Mathematische Beiträge zum Kulturleben der Völker*, Halle, 1863; Cantor, *Vorlesungen über Geschichte der Mathematik*, E. B. Tylor, *Primitive Culture*, 1st edn., London, 1871; 4th edn., London, 1903; Tylor, *Researches into the Early History of Mankind and the Development of Civilization*, London, 1865.
[2] Mach, *Mechanics*, pp. 583, 584, Fifth edn. 1942.
[3] L. Kronecker, 'Ueber den Zahlbegriff', *Philosophische Aufsätze Eduard Zeller zu seinem fünfzigjahrigen Doctorjubilaum gewidmet*, Leipzig, 1887; *Werke*, Vol. III, pp. 249—274.
[4] Cf. 'Ueber die ökonomische Natur der physikalischen Forschung, *Almanach der Wiener Akademie*, 1882, p. 167 (Eng. transl. in *Popular Scientific Lectures*, Chicago, 1898, p. 186). Also, *Mechanik* (1883), p. 458 (Eng. transl., Chicago, 3rd edn., 1907, p. 486). Also, *Analyse der Empfindungen*, 1886, p. 165 (Eng. transl., Chicago, 1897, p. 178).
[5] Helmholtz, 'Zählen und Messen', in *Philosophische Aufsätze, Eduard Zeller . . . gewidmet*, Leipzig, 1887, pp. 17—52. Compare especially pp. 17 and 20. Reprinted in Helmholtz's *Wissenschaftliche Abhandlungen*, Vol. III, pp. 356—391; cf. especially, pp. 357 and 359.
[6] *Lehrbuch der Arithmetik und Algebra*, Leipzig, 1873, p. 14. I became acquainted

with Schröder's book, which is based upon Grassmann's work, through a quotation in the aforementioned paper of Helmholtz.
[7] *Loc. cit.*, pp. 30—32; *Wiss. Abh.*, pp. 369—371. Cf. also Kronecker, *loc. cit.*, p. 268.
[8] Helmholtz, *loc. cit.*, p. 37; *Wiss. Abh.*, p. 376.

CHAPTER V (pp. 73—78)

[1] Euclids ingenious proof of this proposition is found in his *Elements*, Vol. X, p. 117. Compare Moritz Cantor's views in his *Vorlesungen über Geschichte der Mathematik*, Vol. I, 3rd edn, Leipzig, 1907, pp. 181—184.
[2] The irrational number $\sqrt{p}$ is the limit between all rational numbers (1) the squares of which are less and (2) the squares of which are greater than p. In the first class no greatest, and in the second no least, number can be assigned. If $\sqrt{p}$ is rational, the number in question is the greatest of the first and the least of the second class. [Or should here take the place of and.] Compare Tannery, *Théorie des Fonctions*, Paris, 1886.
[3] Dedekind, *Stetigkeit und irrationale Zahlen*, 2nd edn., Brunswick, 1892. [English translation by W. W. Beman in *Essays on the Theory of Numbers*, Chicago, 1901. The passage referred to is on p. 9.]
[4] Hankel, *Geschichte der Mathematik*, Leipzig, 1874, p. 149.
[5] It is well known that differentials may be avoided by operating with differential coefficients which are the limiting values of the difference quotients. Timid minds which find solace in this mode of conception will be content to put up with the cumbrousness sometimes involved.

CHAPTER VI (pp. 79—112)

[1] 'Sur le nouveau thermomètre', *Histoire de l'Academie Royale des Sciences*, Paris for 1703, pp. 6—10.
[2] *Pyrometrie oder vom Maasse des Feuers und der Wärme*, Berlin, 1779, p. 184.
[3] According to Albert Riggenbach, *Historische Studie über die Grundbegriffe der Wärmefortpflanzung*, Basel, 1884, p. 17.
[4] *Nouvelles expériences*, Paris, 1785. [Cf. Jean Ingen-Housz; 'Sur les métaux comme conducteurs de la chaleur', *Journ. de Phys.*, Vol. XXXIV, 1789, pp. 68, 380.]
[5] *Gesetze und Modificationen des Wärmestoffes*, Erlangen, 1791.
[6] *Journal des Mines*, An. XIII, (1804), Vol. XVII, pp. 203—224.
[7] "Mémoire sur la propagation de la chaleur et sur un moyen simple et exact de mesurer les hautes temperatures," *Ibid.*, p. 209.
[8] *Traité de physique*, Paris, 1816 [Vol. I, p. 257.].
[9] *Théorie analytique de la chaleur*, Paris, 1822, pp. 63—64. [English translation by A. Freeman under the title *The Analytical Theory of heat* (Cambridge and London, 1878). The passage referred to occurs on pp. 58—59.]
[10] *Théorie analytique de la chaleur*, pp. xvii—xviii, 282 *et seqq*. [Freeman, pp. 255 *et seqq*.]
[11] [*Théorie*, Chap. I, Sect. IV; *Oeuvres*, Vol. I, p. 38; Freeman, pp. 45 *et seqq*.]

[12] These are the first two terms of the development of $kq(\partial u/\partial x)$ by Taylor's series.
[13] *Théorie*, pp. 152—158 [Freeman, pp. 126—130].
[14] *Mechanics*, pp. 278—280.
[15] *Ann. de chim. et de phys.* (2), Vol. XXXVII, 1828, pp. 291—315 [*Oeuvres*, Vol. II, pp. 453—477].
[16] *Ann. de chim. et de phys.* (3), Vol. II, 1841, pp. 107—115.
[17] *Ann. de chim. et de phys.* (2), Vol. XIX, 1821, pp. 97—109.
[18] *Trans. Roy. Soc, Edinburgh*, Vol. XXIII, 1864, pp. 133—146; Vol. XXIV, 1867, pp. 73—110.
[19] *Théorie*, p. 61 [Freeman, p. 57].
[20] *Théorie*, p. 599 [Freeman, p. 465].
[21] *Ann. de chim.* (3), Vol. LXVI, 1862, pp. 183—187.
[22] Taylor, *Methodus incrementorum*, London, 1717 [really 1715], p. 89.
[23] The cumbersome presentation of Taylor is here somewhat modernized.
[24] Mach, *Mechanics*, p. 168.
[25] Taylor (*op. cit.*, p. 92) gave the expression in another form.
[26] *Ibid.*, pp. 90, 91.
[27] *Hist. de l'Acad. de Berlin*, 1747, pp. 214—219.
[28] Euler, *Hist. de l'Acad. de Berlin*, 1753, p. 208.
[29] D'Alembert concluded in the following manner:

$$du = \frac{\partial u}{\partial t}\,dt + \frac{\partial u}{\partial x}\,dx = p \cdot dt + q \cdot dx;$$

further,

$$dp = \frac{\partial p}{\partial t}\,dt + \frac{\partial p}{\partial x}\,dx = \alpha \cdot dt + \beta \cdot dx,$$

$$dq = \frac{\partial q}{\partial t}\,dt + \frac{\partial q}{\partial x}\,dx = \beta \cdot dt + \alpha \cdot dx.$$

for

$$\beta = \frac{\partial^2 u}{dx\,dt} = \frac{\partial^2 u}{dt\,dx}$$

and

$$\alpha = \frac{\partial^2 u}{dt^2} = \frac{\partial^2 u}{dx^2}$$

by his first equation. Therefore

$$dp + dq = (\alpha + \beta)(dt + dx)$$

is a function of $t + x$, and

$$dp - dq = (\alpha - \beta)(dt - dx)$$

is a function of $t - x$. Hence we get for u itself

$$u = \phi(x + t) + \psi(x - t).$$

[30] Euler, from the equation

$$\frac{\partial^2 u}{\partial t^2} = c^2 \frac{\partial^2 u}{\partial x^2}$$

made the inadmissible inference, which accidentally leads to a true result, that we have

$$\frac{\partial u}{\partial t} = k \frac{\partial u}{\partial x}$$

By differentiation

$$\frac{\partial^2 u}{\partial t^2} = k \frac{\partial^2 u}{\partial t\,\partial x}; \quad \text{and} \quad \frac{\partial^2 u}{\partial x\,\partial t} = k \frac{\partial^2 u}{\partial x^2};$$

and thus

$$\frac{\partial^2 u}{\partial t^1} = k^2 \frac{\partial^2 u}{\partial x^2}, \quad \text{and} \quad k = \pm c.$$

Therefore an integral of the principal equation satisfies also one of the two equations.

$$\frac{\partial u}{\partial t} = +c \frac{\partial u}{\partial x} \quad \text{and} \quad \frac{\partial u}{\partial t} = -c \frac{\partial u}{\partial x}$$

Since, now,

$$du = \frac{\partial u}{\partial t}\, dt + \frac{\partial u}{\partial x}\, dx \quad \text{we have} \quad du = \frac{\partial x}{\partial x}\,(dx + k \cdot dt);$$

therefore u is a function of $x + kt$, and analogously of $x - kt$, etc. On p. 209 (*loc. cit.*), Euler stated that, if $P, Q, R, \ldots$ are particular integrals of a linear differential equation, then $u = \alpha P + \beta Q + \gamma R + \ldots$, where $\alpha, \beta, \gamma, \ldots$ are arbitrary constants, also satisfies the equation. Cf. also Euler, 'Sur la vibration des cordes', *Hist. de l'Acad. de Berlin*, 1748, pp. 69—85.

[31] *Hist. de l'Acad. Roy. des Sciences*, Paris, 1701, pp. 297—364; *ibid.*, 1702, pp. 308—328.

[32] D. Bernoulli, 'Réflexions et éclairissements sur les nouvelles vibrations des cordes exposées dans les Mémoires de l'Académie de 1747 et de 1748', *Hist. de l'Acad. de Berlin*, 1753, pp. 147—172.

[33] Euler, *ibid.*, pp. 196—222.

[34] Euler, *Hist de l'Acad. de Berlin*, 1735, p. 209. [This reference must be wrong. According to M. Cantor (*Vorlesungen über Geschichte der Mathematik*, Vol. III, 2nd edn., Leipzig, 1901, p. 893), Euler gave the method of constructing the complete integral (of an ordinary linear differential equation of the nth order) by adding together n particular integrals of the equation, each multiplied by an arbitrary constant, on p. 200 of his paper 'De integratione aequationum differentialium altiorum graduum' in *Miscellanea Berolinensia*, Vol. VII (printed in 1743), pp. 193—242. Cf. also Cantor, *op. cit.*, p. 906, where a remark from pp. 208—209 of Euler's paper 'Remarques sur les mémoires précedent de M. Bernoulli' in *Histoire de l'Académie de Berlin*, 1753, vol. IX, pp. 196—222, is quoted.]

[35] *Théorie*, pp. 167 *et seqq.* [Freeman, pp. 137 *et seqq.*].

[36] Section 24 of this chapter.

[37] *Théorie*, p. 580 [Freeman, p. 450].
[38] In his memoirs on the nature and propagation of sound, the first being in Vol. I (1759) of the *Miscellanea Taurinensia*, and the fourth in Vol. III (1766). [These memoirs are printed in Vol. I of Lagrange's *Oeuvres* (pp. 39–148) and Vol. II respectively.]
[39] See for example Riemann-Hattendorff, *op. cit.*, pp. 46 *sqq.*
[40] Cf., e.g., *Théorie*, p. 248 [Freeman, p. 198].
[41] Fourier, *Théorie*, p. 260 [Freeman, p. 207].
[42] Fourier, *Théorie*, pp. 445 *sqq.* [Freeman, pp. 348 *sqq.*]; Dirichlet's memoirs of 1829 and 1837 [see *Ostwald's Klassiker*, No. 116].

CHAPTER VII (pp. 113–120)

[1] Cf. Mach, *Mechanics*, Fifth English edn., p. 176.
[2] [Cf. *ibid.*, pp. 415–416.]
[3] Cf. Chapter VI, §9.
[4] Cf. Chapter VI, §17. We see in what way, in accordance with the standpoint explained, *molecular-theoretical* investigations concerning the significance of *m* and concerning its connection with the powers of *absorption* and *emission* of heat-conducting bodies came to be introduced.
[5] Mach 'Ueber Guébhard's Darstellung der Aequipotential-Curven', *Sitzungsber. d. Wiener Akademie, Math.-naturw. Classe II. Abth.* Vol. LXXXVI, 1882, pp. 10–14. Cf. also Mach, 'Ueber die physiologische Wirkung raumlich vertheilter Lichtreize', *ibid.*, vol. LVII, 1868; *Analysis of the Sensations*, pp. 97–98; and *Mechanics*, Fifth edn., pp. 598–599. [H. Guébhard's paper referred to is 'Sur un mode d'enregistrement photographique des effluves thermiques', *Compt. Rend.*, Vol. CXXV, 1897, pp. 814–819.]
[6] In regard to the example and the entire discussion, cf. *Analysis of the Sensations*, p. 160 *et seqq.*

CHAPTER VIII (pp. 121–141)

[1] *Lectures on the Elements of Chemistry, delivered in the University of Edinburgh, now published by J. Robinson*, two volumes, Edinburgh, 1803; German translation by Crell under the title; *Vorlesungen über Chemie*, four volumes, Hamburg, 1804–5; new edition, 1818. [The English original is very rare, and therefore Mach's references to the first German edition are given in this book. The present reference is to Vol. I, p. 125.]
[2] *Ars Magna Lucis et Umbrae*, 1671, p. 757.
[3] *Histoire de l'Académie*, Paris, 1699, p. 90.
[4] *Chemische Abhandlung von der Luft und dem Feuer*, Upsala and Leipzig, 1777. [2nd edn., 1782; edited by W. Ostwald in No. 58 of *Ostwald's Klassiker*; English translation by J. R. Forster under the title *Treatise on Air and Fire*, London, 1780.]
[5] On this cf. Prévost, *Du Calorique Rayonnant*, Paris, 1809, pp. 1–2.
[6] *Pyrometrie*, Berlin, 1779, pp. 151–152.
[7] *Ibid.*, p. 201.
[8] *Ibid.*, p. 208.

[9] *Ibid.*, p. 152.
[10] 'Essai sur le Feu' in *Essais de Physique*, Geneva, 1790, p. 83.
[11] *Ibid.*, p. 82.
[12] *On the Philosophy of Light, Heat, and Fire*, Edinburgh, 1794.
[13] [Cf. Rumford's 'Historical Review of the Various Experiments of the Author on the Subject of Heat', translated into English in *The Complete Works of Count Rumford*, Vol. II, Boston, 1873, pp. 188—240, from Vol. IV of Rumford's *Kleine Schriften politischen, ökonomischen und philosophischen Inhalts*, Weimar, 1800—3, which, in its turn, was translated from *Essays, Political, Economical, and Philosophical*, London, 1796—1802.]
[14] *An Experimental Inquiry into the Nature and Propagation of Heat*, London, 1804.
[15] *Phil. Trans.*, 1800, pp. 255—283.
[16] 'Lettre sur une Propriété nouvelle de la Chaleur solaire', *Ann. de Chim.* (2), Vol. XLVIII, 1831, pp. 385—395.
[17] *Complete Works*, Vol. II, pp. 225—232.
[18] *Complete Works*, Vol. II, pp. 234—237.
[19] *Complete Works*, Vol. II, pp. 225—227.
[20] *Ibid.*, p. 228.
[21] *Ibid.*, p. 84.
[22] *Ibid.*, p. 110.
[23] *Ibid.*, p. 199.
[24] *Op. cit.*, p. 24.
[25] *Ibid.*, pp. 66—67.
[26] *Ibid.*, p. 186.
[27] *Op. cit.*, p. 405.
[28] *Ibid.*, p. 273.
[29] *Ibid.*, p. 483.
[30] For reference, see Chapter III, §1.
[31] Lambert, *Pyrometrie*, p. 141.
[32] Cf. the next Chapter.
[33] *Ibid.*, p. 236.
[34] *Ibid.*, p. 244.
[35] *Op. cit.*, p. 405.
[36] Cf. his *Anlage zur Architectonic oder Theorie des Ersten und des Einfachen in der philosophischen und mathematischen Erkenntniss*, Two volumes, Riga, 1771.
[37] *Metaphysische Anfangsgründe der Naturwissenschaft*, Leipzig, 1794.
[38] *Op. cit.*, p. 9.
[39] In addition to this, cf. Prévost's 'Mémoire sur l'équilibre du Feu', *Journ de Phys.*, Vol. XXXVIII, 1791; and *Exposition élémentaire des Principes qui servent de base à la Théorie de la Chaleur rayonnante*, Geneva, 1832.
[40] *Essai sur le Calorique rayonnant*, p. 23.
[41] *Ibid.*, p. 26.
[42] *Ibid.*, p. 92.
[43] *Op. cit.*
[44] *Essai*, p. 115.
[45] *Ibid.*, p. 99.
[46] *Ibid.*, p. 259.

[47] *Ann. de Chim.* (2), Vol. III, 1816, pp. 350–375; *ibid.*, Vol. IV, 1817, pp. 128–145 [*Oeuvres*, Vol. II, pp. 333–348]; *Ann. de Chim.* (2), Vol. VI, 1817, pp. 259–303. [*Oeuvres*, Vol. II, pp. 351–386.]
[48] 'Experimental Illustration of the Radiating and Absorbing Powers of the same Surface', *Journal of the Royal Institution*, Vol. II, 1831, pp. 305–307.
[49] *Grundzüge einer allgem. Photometrie des Himmels*, Berlin, 1861.
[50] 'Bestimmung des Brechungs- und Fahrenzerstreuungsvermögens verschiedener Glasarten in Bezug auf die Verollkommung achromatischer Fernröhre', *Denkschriften der Königl. Bayerischen Akad. der Wiss. zu München*, Vol. V, 1814–1815, pp. 193–226.
[51] *Trans. Roy. Soc. Edinburgh*, Vol. IX, 1823, pp. 433–444. *Phil. Mag.* (3), Vol. II, 1833, pp. 360–363.
[52] *Ann. de Chim.*, Vol. LIII, 1833, pp. 5–73.
[53] Poggendorff's *Annalen*, Vol. CIX, 1860.
[54] *Ann. de Chim.*, Vol. LIX, 1860, pp. 124–128; more fully in Vol. CIX of Poggendorff's *Annalen*. For a more thorough study, cf. Kirchhoff's *Ges. Abh.*, pp. 566 and 571.
[55] *Wiedermann's Annalen*, xxxvii, p. 180ff., 1893.
[56] *An Elementary Treatise on Heat*, 5th edn., Oxford, 1888.
[57] *Phil. Mag.*, Vol. XXX, 1847, pp. 87–93.
[58] Cf. Stewart, *Elementary Treatise*, p. 216.
[59] See §11 of this Chapter.
[60] *Ann. de Chim.*, Vol. VII, 1817, pp. 225–264.
[61] Cf. Stewart, *Elementary Treatise*, p. 229.
[62] *Abhandlungen über die mechanische Wärmetheorie*, Vol. I, Brunswick, 1864, pp. 322 ff.
[63] Poggendorff's *Annalen*, Vol. CXXVII, 1866, pp. 30–45.

CHAPTER IX (pp. 142–145)

[1] Cf. *Mechanics*, Fifth edn., p. 168.
[2] *Ibid.*, pp. 182–184.
[3] *Ibid.*, p. 33.
[4] *Cf.* Chapter XXV, below.

CHAPTER X (pp. 146–170)

[1] See the reference to §1 of Chapter V above.
[2] *Elementa Chemiae*, 2 vols., Leyden, 1732, Vol. I, Exp. XX, Coroll. 17.
[3] *Novi Comment. Acad. Petrop.*, Vol. III, 1753, pp. 309–339.
[4] *Ibid.*, Vol. IV, 1758, pp. 241–270.
[5] *Comment. Acad. Petrop.*, Vol. XIV, 1744–46, pp. 218–239.
[6] *Novi Comment. Acad. Petrop.*, Vol. I, 1750, pp. 152–167.
[7] *Elem. Chem.* Vol. I, 1732, p. 268.
[8] *Ibid.*, p. 270. "But, in this experiment, what is particularly noteworthy is that by it a

wonderful law of nature is manifested, since fire is distributed through bodies according to spaces and not according to densities. For, though a mass of mercury in respect to water, was almost in the proportion 20 to 1, still the force producing heat was, in the result, the same as if the water had been mixed with an equal amount of water. But this very fact is confirmed from other sources by every kind of experiment; as I have said above during my statement that experiments have taught me that all kinds of bodies, after they have been subjected for a sufficiently long time to the same temperature of a common heat, never suffer any difference of heat or of flame in any respect, except only by reason of the space which they occupy. For which reason nothing that would attract fire could be noticed in bodies though density would more constantly detain the fire that was once held in possession." [Latin in the original.]

[9] *Op. cit.*, Vol. I, p. 100.

[10] "Est enim ignis aequaliter per omnia, sed admodum magna, distributus, ita ut in pede cubico auri, aeris et plumarum, par ignis sit quantitas".

[11] At this point Black gave a false conjecture concerning the quantity of heat in a volume of wood and of iron.

[12] Here Black referred to the experiments of Johann Karl Wilcke (*Comment. de Rebus in Medicina gestis*, Vols. XXV and XXVI) and Johann Gadolin (*Nov. Act. Reg. Soc. Upsaliensis*, Vol. V).

[13] *Essay on the Heating and Cooling of Bodies.* [The translators were not able to trace any copy of this work; presumably it was by Benjamin Martin (1704—1782).]

[14] Black, *op. cit.*, Vol. I, p. 131.

[15] *Essay on Chemical Subjects*, London, 1805.

[16] *Kong. Vetensk, Acad. Nya. Handl.* (Stockholm), 1781.

[17] *Ibid.*, 1772.

[18] *Experiments and Observations on Animal Heat and the Inflammation of Combustible Bodies*, London, 1779.

[19] 'Tentamen de Vi Caloris', *Acta Helvetica*, Vol. II, Basel, 1775; and *Pyrometrie*, Berlin, 1779. The former work I know only by the quotations in Riggenbach's *Historische Studie über die Entwicklung der Grundbegriffe der Wärmefortpflanzung*, Basel, 1884.

[20] *Pyrometrie*, p. 280.

[21] *Ibid.*, p. 146, and Riggenbach, *op. cit.*, p. 25.

[22] *Op. cit.*, p. 167.

[23] *Ibid.*, p. 173.

[24] *Op. cit.*, Vol. I, pp. 147 *et seqq.*

[25] *Ibid.*, pp. 162 *et seqq.* Cf. also the German translation of Sir Charles Blagden's two memoirs of 1788 in *Ostwald's Klassiker*, No. 56, entitled: *Die Gesetze der Ueberkaltung und Gefreierpunktserniedrigung*, edited by A. J. von Oettingen.

[26] 'Memoire sur la Chaleur', *Histoire de l'Academie* (Paris), 1780, pp. 355—408; and *Oeuvres de Lavoisier*, Vol. II, Paris, 1862, pp. 283—333.

[27] *Oeuvres de Lavoisier*, Vol. II, pp. 724—736.

[28] 'Calorimetrische Untersuchungen', Poggendorff's *Annalen*, Vol. CXLI, 1870, pp. 1—31.

[29] See Chapter I, §29.

[30] *Ann. de Chim.*, Vol. X, 1819, pp. 395—413.

[31] According to H. F. Weber (Poggendorff's *Annalen*, Vol. CXLVII, and Vol. CLIV, 1875, pp. 367—423) this exception does not hold at high temperatures.
[32] [Cf. Wangerin, *op. cit.*, pp. 60—65.]
[33] *Op. cit.*, Vol. I, pp. 183 *et seqq.*
[34] Cf. Birch's *History of the Royal Society*, Vol. IV.
[35] *Op. cit.*, Vol. I, pp. 196 *et seqq.*
[36] *Op. cit.*, Vol. I, p. 252.
[37] *Ibid.*, p. 266.
[38] *Ibid.*, p. 273.
[39] *Ibid.*, p. 243.
[40] *Ibid.*, p. 323.
[41] Cf. Robinson's note in *ibid.*, p. 400.
[42] Robinson, *ibid.*, pp. 394, 401.

CHAPTER XI (pp. 171–181)

[1] *Op. cit.*, p. 76.
[2] In §3 of Chapter X.
[3] A younger colleague of mine has related to me that he could not see, when he was a schoolboy, why, in this case, the arithmetical mean should be forthcoming. I have constantly had reason to appreciate such *superior* kind of lack of understanding as a sure sign of unusual critical endowment.
[4] Cf. Mach, *Mechanics*, Fifth edn., pp. 265—271. Cf. also §§3—5 of Chapter II of the present book. [Cf. also Mach's *History and Root of the Principle of the Conservation of Energy*, Chicago, 1911, pp. 83—85.]
[5] See *ibid.*
[6] Chapter II, §3.
[7] [This statement is now known, of course, to be true only of bodies whose velocities do not approximate to velocities of the order of the velocity of light.]

CHAPTER XII (pp. 182–195)

[1] *Experiments and Observations on Animal Heat and the Inflammation of Combustible Bodies*, London, 1778, 2nd edn., 1788.
[2] [Cf. the two memoirs of 1780 and 1784 referred to in §17 of Chapter X, and especially the second.]
[3] *Journal de Physique*, Vol. LXXXIX, 1819, pp. 321—346.
[4] *Ann. de Chim.*, Vol. LXXXV, 1813, pp. 72—110.
[5] *Trans. Roy. Soc. Edinburgh*, Vol. X, 1826, pp. 195—216.
[6] *Ann. de Chim.*, Vol. XXXV, 1827, pp. 5—34.
[7] *Mém. de l'Inst.*, Vol. XXVI, 1862, pp. 1—915.
[8] *Phil. Trans.*, 1788, pp. 43—52.
[9] Gilbert *Annalen*, p. 243 (1799).
[10] *Mem. Manchester Lit. and Phil. Soc.*, Vol. V, pt. II, 1802, p. 515.

[11] Cf. Rosenberger, *Geschichte der Physik*, Vol. III, p. 224.
[12] *Mém. de la Societé d'Arcueil*, Vol. I, 1807, p. 180. I am indebted to Professor J. Joubert of Paris for allowing me the perusal of this very rare journal in his possession. [Mach printed the French text, being a rarity, as an appendix to his volume. It is here omitted, being now available elsewhere both in French and in English. *Ed.*]
[13] Chapter I, §30.
[14] [*Traité de Physique*, Paris, 1816, 4 vols.]
[15] German translation, Leipzig, 1829, Vol. V, p. 343.
[16] [See Chapter XIII, §3.]
[17] See §1 of this Chapter.
[18] *Loc. cit.*
[19] *Pyrometrie*, pp. 266 *et seqq.*
[20] *Manch. Mem.*, Vol. V. pt II, pp. 515 *et seqq.*
[21] *Loc. cit.* The literature of this subject was very completely studied shortly before the finishing of this work (1896) by Maneuvrier [see the Author's Preface.]
[22] *Philosophiae Naturalis Principia Mathematica*, London, 1687, p. 364. [Scholium to Proposition L. of Book II. (Section VIII).]
[23] Cf. on this point the references in Gehler's *Physikalisches Worterbuch*. [New edition, Leipzig, 1830.]
[24] *Ann. de Chim.*, Vol. III, 1816, pp. 238—241.
[25] *Ann. de Chim.*, Vol. XIX, 1821, pp. 436—437; cf. Laplace, *Mécanique Céleste*, Vol. V, p. 119.
[26] *Ann. de Chim* Vol. XXIII, 1823, pp. 337—352.
[27] *Ann. de Chim.*, Vol. XLI, 1829, pp. 113—159.

CHAPTER XIII (pp. 196—223)

[1] Cf. Tylor's works cited in the first note to §4 of Chapter IV.
[2] Leyden, 1690, pp. 2—3. [An English translation of this work, by S. P. Thompson, was published at London in 1912.]
[3] Memoir of 1780 (*Ostwald's Klassiker*, No. 40, pp. 5—6) referred to in §17 of Chapter X.
[4] 'An Inquiry concerning the Source of the Heat which is excited by Friction', *Phil. Trans.*, January 25th, 1798. This is also cited in the above mentioned work and also in Tyndall, *op. cit.*, pp. 12—13, 25, 53—57.
[5] 'An Essay on Heat, Light, and the Combinations of Light' in Thomas Beddoes's *Contributions to Physical and Medical Knowledge, principally from the West of England*, Bristol, 1799; *The Collected Works of Sir Humphry Davy*, London, 1839—41, Vol. II, pp. 11 *et seqq.*
[6] *Elements of Chemical Philosophy*, London, 1812.
[7] [This account of Davy's work is rather fuller than that given by Mach, and in it the exposition of Tait, *op. cit.*, pp. 6—8, has been used.]
[8] *A Course of Lectures on Natural Philosophy and the Mechanical Arts*, London, 1807. [New ed., in two vols., London, 1845, Vol. I, p. 502.]
[9] *Ann. de Chim et de Phys.*, Vol. 57, p. 432.

[10] *Réflexions sur la Puissance Motrice du Feu et sur les machines propres à développer cette puissance*, Paris, 1824, note on p. 21.
[11] *Op. cit.*, 1824, note on p. 37.
[12] [See §§1 and 3 of Chapter XIV.]
[13] In this exposition, there is made only one small and unessential, though methodically convenient, variation from Carnot's.
[14] *Op. cit.*, 1824, p. 89.
[15] *Journal de l'École Royale Polytechnique*, Vol. XIV, 1834, pp. 153—190; *Poggendorff's Annalen*, Vol. LIX, 1843, pp. 446—451, 566—586.
[16] *Journ. de l'Éc. Polyt.*, Vol. XIV, pp. 157—158; Poggendorff's *Annalen*, Vol. LIX, p. 452.
[17] The work of expansion is represented by the surface $v_0\ a\ a'\ b'\ v_2$, the work of compression by $v_0\ a\ b\ b'\ v_2$, and consequently the difference by $a\ a'\ b'\ b$.
[18] *Journ. de l'Éc. Polyt.*, Vol. XIV, p. 159; Poggendorff's *Annalen*, Vol. LIX, p. 453.
[19] *Journ. de l'Éc. Polyt.*, Vol. XIV, p. 162; Poggendorff's *Annalen*, Vol. LIX, p. 457.
[20] Except as to an infinitesimal of the second order, as Clausius showed later.
[21] 'On an Absolute Thermometric Scale, etc.', *Proc. Camb. Phil. Soc.* (June 5th, 1848), Vol. I, 1843—63, pp. 66—71. *Phil. Mag.* (3), Vol. XXXIII, 1848, pp. 313—317. [*Math. and Phys. Papers*, Vol. I, Cambridge, 1882, pp. 100—106.]
[22] *Phil. Mag.*, Vol. XXXIII, note on p. 315. [*Math. and Phys. Papers*, Vol. I, note on p. 102.]
[23] *Trans. Roy. Soc. Edinburgh* (Jan. 2nd., 1849), Vol. XVI, Part V, 1849, pp. 575—580; [reprinted, with some slight alterations made by the author, in the *Cambridge and Dublin Math. Journ.*, Vol. V, 1850, pp. 248—255; W. Thomson's (Lord Kelvin's) *Math. and Phys. Papers*, Vol. I, pp. 156—164; and the *Collected Papers in Physics and Engineering* by James Thomson (ed. J. Larmor and J. Thomson), Cambridge, 1912, pp. 196—203.]
[24] [The rest of this Section on some of the work of James and William Thomson is much fuller than the corresponding section of the German original, and is given, as much as possible, in James Thomson's own words.]
[25] [This refers to William Thomson's paper: 'An Account of Carnot's Theory of the Motive Power of Heat; with Numerical Results deduced from Regnault's Experiments on Steam', referred to in §§2 and 9 of Chapter XV below.]
[26] *Proc. Roy. Soc. Edinburgh*, Vol. II, 1851, pp. 267—271; *Phil. Mag.*, Vol. XXXVII, 1850, pp. 123—127; *Math. and Phys. Papers*, Vol. I, pp. 165—169.]

CHAPTER XIV (pp. 224—250)

[1] See the second note to §4 of Chapter XIII.
[2] *Étude sur l'Influence des Chemins de Fer*, Paris, 1839.
[3] Reprint of the work just mentioned, pp. 259, 287.
[4] Letter of Joule in *Phil. Mag.*, Vol. XXVIII, 1864, pp. 150—152.
[5] *Compt. Rend.*, Vol. XXV, 1847, pp. 420—422.
[6] *Ibid.*, pp. 309—311. [*The Scientific Papers of James Prescott Joule*, Vol. I, London, 1884, pp. 283—286.]

[7] Cf. Colding's letter in *Phil. Mag.*, Vol. XXVII, 1864, pp. 56—64.
[8] 'On the caloric effects of Magneto-Electricity, and on the Mechanical Value of Heat', *Philos. Magazine* (1843) Vol. 23, p. 263.
[9] See his *Mechanik der Wärme*, 1867, p. 13.
[10] *Théorie mécanique de la chaleur*, Paris, 1868.
[11] With respect to the ease with which stimulation comes, I will only adduce one case from my own experience. When I was at school, about 1853, I read somewhere the expression "mechanical equivalent of heat". By repeated occupation with mechanical constructions, the impossibility of constructing *mechanically* a *perpetuum mobile* had long become clear to me. The above expression at once made it subjectively certain to me that such a construction is impossible *in any other way* as well. When later I became acquainted with the principle of energy, this principle appeared to me trustworthy and almost self-evident.
[12] *Op. cit.*, 1878, pp. 93—94.
[13] Cf. W. Preyer, *R.v. Mayer über die Erhaltung der Energie* (correspondence of Mayer and Griesinger), Berlin, 1889; and R. Mayer, *Die Mechanik der Wärme in gesammelten Schriften*, 3rd edn., (edited by Jakob Weyrauch), Stuttgart, 1893; *Kleinere Schriften und Briefe nebst Mitteilungen aus seinem Leben* (edited by Weyrauch), Stuttgart, 1893.
[14] Letter to Griesinger of May 16th, 1844.
[15] [The author reproduces Mayer's pronunciation: "Es ischt aso"!]
[16] I am indebted for this story to an oral communication from Jolly, who afterwards repeated it to me by letter.
[17] 'Bemerkungen über die Kräfte der unbelebten Natur', *Annalen der Chemie und Pharmacie*, Vol. XLII, 1842, pp. 233 *sqq.*
[18] Preyer, *op. cit.*, p. 36.
[19] Cf. the English translations of the author's works: *History and Root of the Principle of the Conservation of Energy*, Chicago, 1911, pp. 69—74; *The Science of Mechanics*, 5th edn., pp. 606—609; *Contributions to the Analysis of the Sensations*, Chicago, 1897, pp. 174—176. The first German editions of these works were published in 1872, 1883, and 1886 respectively.
[20] *Mechanik der Wärme*, 1867, p. 25.
[21] *Ibid.*, p. 26.
[22] Thomson (1) 'On the Mechanical Action of Radiant Heat or Light etc.', *Proc. Roy. Soc. Edinb.* Vol. III, 1857, pp. 108—113 [*Math. and Phys. Papers*, Vol. I, pp. 505—510]; (2) 'On the Mechanical Energies of the Solar System', *Trans. Roy. Soc. Edinburgh* Vol. XXI, pp. 63—80 [*Math. and Phys. Papers*, Vol. II, pp. 1—25]; (3) 'Note on the Possible Density of the Luminiferous Medium, and on the Mechanical Value of a Cubic Mile of Sunlight', *Trans. Roy. Soc. Edinburgh* Vol. XXI, 1857, pp. 57—61 [*Math. and Phys. Papers*, Vol. II, pp. 28—33.]
[23] Cf. the letter of Baur to Mayer of September 7th, 1844, in Weyrauch's edition of Mayer's *Kleinere Schriften*, p. 159.
[24] *Wissenschaftliche Abhandlungen* Vol. I (Leipzig, 1882).
[25] The sort of depreciation which such a work can suffer may be illustrated by the following story. When I was a young *docent* I was put right by an old gentleman because I spoke too warmly of Helmholtz's tract. This tract, said he, was a very bad one; it considered quadrature as the summation of ordinates — which is quite absurd — and so on. What must such people — quite without malice — have thought of Mayer's

memoir? We must not require a discoverer to be a professional Philistine, nor must we expect discoveries from a professional Philistine, however learned he may be. A schoolmaster is very unpleasantly affected by Galileo's remark: "The force of impact is infinite as compared with the force of pressure" [cf. the author's *Mechanics*, p. 312] or by Faraday's remark that the electric current is an axis of force, and yet a great wealth of knowledge is contained in these remarks. Cf. also the letter of Reusch to Mayer of April 26th, 1854 (Weyrauch, *op. cit.*, p. 377). It is not to be overlooked that, out of the often criticized view of a surface as a sum of ordinates, the calculus of fluxions has developed.

[26] Cf. the beautiful exposition of Holtzmann in his *Mechanische Wärmetheorie*, Stuttgart, 1866.

[27] 'On the Production of Heat by Voltaic Electricity', *Proc. Roy. Soc.*, 1840; *The Scientific Papers of James Prescott Joule*, Vol. I, 1884, pp. 59—60.

[28] 'On the Calorific Effect of Magneto-Electricity and on the Mechanical Value of Heat', *Phil. Mag.*, Vol. XXIII, 1843, pp. 263—276, 347—355, 435—443; *Scientific Papers*, Vol. I, pp. 123—159.

[29] *Scientific Papers*, Vol. I, pp. 145—146.

[30] *Scientific Papers*, Vol. I, pp. 157—158.

[31] *Scientific Papers*, Vol. I, pp. 268—269.

[32] [This sentence is slightly altered from the original. In fact, the author implied that Joule was a practical engineer, and this was not the case. Joule's father was a practical, businessman; and Joule himself certainly seems to have inherited much of this practical spirit.]

[33] 'On the Changes of Temperature produced by the Rarefaction and Condensation of Air', *Phil. Mag.*, Vol. XXVI, 1845, pp. 369—383; *Scientific Papers*, Vol. I, pp. 172—189.

[34] 'On the Mechanical Equivalent of Heat', *Brit. Assoc. Rep.*, 1845, *Scientific Papers*, Vol. I, p. 202.

[35] 'On the Mechanical Equivalent of Heat', *Phil. Trans.*, 1850, Pt. I, pp. 61—82; *Scientific Papers*, Vol. I, pp. 298—328.

[36] *Recherches experimentales sur la valeur de l'equivalent mécanique de la chaleur*, Colmar and Paris, 1858; and *Théorie mécanique de la chaleur*, Paris, 1868.

[37] *Théorie mécanique de la chaleur*, pp. 26 and 34.

[38] *Ibid.*, pp. 4, 11.

[39] *Op. cit.*, pp. 4, 11.

CHAPTER XV (pp. 251—281)

[1] Cf. §10 of Chapter XIV.

[2] 'An Account of Carnot's Theory of the Motive Power of Heat', *Trans. Roy. Soc. Edinburgh*, Vol. XVI, 1849, pp. 541—574. [*Math. and Phys. Papers*, Vol. I, pp. 113—155.]

[3] Cf. the works just cited, pp. 543—544 and 116—117 respectively.

[4] Cf. notes on p. 545 and pp. 118—119 respectively.

[5] *Phil. Mag.*, Vol. XXVI, 1845, pp. 369—383. [*Scientific Papers*, Vol. I, pp. 172—189.]

[6] Poggendorff's *Annalen*, Vol. LXXIX, 1850, pp. 368—397, 500—524.

[7] *Ann. de Chim.*, Vol. XLI, 1829, pp. 113—159; Poggendorff's *Annalen*, Vol. XVI, 1829, pp. 438—479.

[8] 'On the Dynamical Theory of Heat', *Trans. Roy. Soc. Edinburgh*, Vol. XX, 1853, pp. 261—288. [*Math. and Phys. Papers*, Vol. I, pp. 174—210.]

[9] 'On the dynamical theory etc.', pp. 279—280.

[10] Thomson, 'On a Method of Discovering Experimentally the Relation between the Mechanical Work spent and the Heat produced by the Compression of a Gaseous Fluid', *Trans. Roy. Soc. Edinburgh*, Vol. XX, 1853, pp. 289—298. [*Math. and Phys. Papers*, Vol. I, pp. 210—222 (where it is merely headed as "Part IV").]

[11] See Thomson, *Trans. Roy. Soc. Edinburgh*, Vol. XVI, 1849, p. 566.

[12] 'On the Dynamical theory of Heat. Part VI. Thermo-Electric Currents', *Trans. Roy. Soc. Edinburgh*, Vol. XXI, 1857, pp. 123—171. [*Math. and Phys. Papers*, Vol. I, pp. 232—291, 324—325.]

[13] A simple exposition of this resolution, which in essentials coincides with Thomson's view, will be given later.

[14] 'On a Universal Tendency in Nature towards the Dissipation of Mechanical Energy', *Proc. Roy. Soc. Edinburgh*, Vol. III, pp. 139—142. [*Math. and Phys. Papers*, Vol. I, pp. 511—514.]

[15] *Phil. Mag.*, 1852. Cf. Tait, 'On the Dissipation of Energy', *Phil. Mag.*, Vol. VII, 1879, pp. 344—346; Thomson, 'Note on the preceding Letter', *ibid.*, pp. 346—348; [*Math. and Phys. Papers*, Vol. I, pp. 456—459.]

[16] 'Ueber eine veränderte Form des zweiten Hauptsatzes der mechanischen Wärmetheorie', Poggendorff's *Annalen*, Vol. XCIII, 1854, pp. 481—508.

[17] *Die mechanische Wärmetheorie*, 2nd edn., Brunswick, 1876, Vol. I, pp. 85—87. [Browne's translation, pp. 81—83.]

[18] *Mechanische Wärmetheorie*, 2nd edn., 1876, pp. 87—90. [Browne's translation, pp. 84—87.]

[19] *Grundzüge der mechanischen Wärmetheorie*, 2nd edn., 1866.

[20] The expression "adiabatic" is due to Rankine. Gibbs replaced it by "isentropic".

[21] 'Ueber verschiedene für die Anwendung bequeme Formen der Hauptgleichungen der mechanischen Wärmetheorie', Pogg. *Ann.*, Vol. CXXV, 1865, pp. 353—400.

[22] The word "energy" was introduced by Thomson.

[23] *Theory of Heat*, 9th edn., 1888, p. 163.

[24] Pogg. *Ann.*, 1854.

[25] Pogg. *Ann.*, 1865.

[26] *Theory of Heat*, 9th edn., p. 193.

[27] 'Die thermodynamischen Beziehungen', *Mem. de l'Acad. Imp. des Sci. de St. Petersbourg*, 1885. Cf. also Mach, *Erhaltung der Arbeit*, 1872, and *Mechanik*, 1883, and following chapters. [Mach's work of 1872 was translated and annotated by Philip E. B. Jourdain under the title *History and Root of the Principle of the Conservation of Energy* (Chicago and London, 1911), and the passages to be referred to here are on pp. 85—86, 93—94, 107—108. See also *Mechanics*, 5th Eng. edition, 1942, pp. 598—600; and §4 of Chapter VII of the present work.]

[28] 'On a Mechanical Theory of Thermoelectric Currents', *Proc. Roy. Soc. Edinburgh*, Vol. III, pp. 91—98; [*Math. and Phys. Papers*, Vol. I, pp. 316—323]. Thomson's memoir of 1854 is cited in §14 of this chapter.

[29] Pogg. *Ann.*, Vol. LXXXVI, 1852, pp. 161—205.

[30] Pogg. *Ann.*, Vol. LXXXVI, 1852, pp. 337–375.
[31] Pogg. *Ann.*, Vol. XC, 1853, pp. 513–544.
[32] Cf. *Royal Society of Edinburgh, Transactions*, Vol. 20 and *Proceedings*, 185–183.
[33] *Proceedings R.S.E.*, 4 Jan. 1853.

CHAPTER XVI (pp. 282–286)

[1] This graphical representation was, if I am not mistaken first given by Balfour Stewart (*An Elementary Treatise on Heat*, 5th edn., p. 348).
[2] This representation was given by F. Wald (*Die Energie und ihre Entwerthung*, Leipzig, 1889, p. 60).

CHAPTER XVII (pp. 287–294)

[1] 'On a Method of Discovering Experimentally The Relation between The Mechanical Work spent and the Heat produced by the Compression of Gaseous Fluid', *Trans. Roy. Soc. Edinburgh*, Vol. XX, pp. 289–298. Cf. §13 of Chapter XV.
[2] "Let air be forced continuously and as uniformly as possible, by means of a forcing pump, through a long tube, open to the atmosphere at the far end, and nearly stopped in one place so as to leave, for a short space only an extremely narrow passage on each side of which, and in every other part of the tube, the passage is comparatively wide; and let us suppose, first, that the air in rushing through the narrow passage is not allowed to gain any heat from, nor (if it had any tendency to do so) to part with any to, the surrounding matter. Then, if Mayer's hypotheses were true, the air after leaving the narrow passage would have exactly the same temperature as it had before reaching it. If, on the contrary, the air experiences either a cooling or a heating effect in the circumstances, we may infer that the heat produced by the fluid friction in the rapids, or, which is the same, the thermal equivalent of the work done by the air in expanding from its state of high pressure on one side of the narrow passage to the state of atmospheric pressure which it has after passing the rapids, is in one case less, and in the other more, than sufficient to compensate the cold due to the expansion; and the hypothesis in question would be disproved". Joule's *Scientific Papers*, Vol. II, p. 217.
[3] The first of the papers on this subject was read to the British Association on September 3rd, 1852. All the papers in question are collected in Joule's *Scientific Papers*, Vol. II, pp. 216–362.
[4] This addition is necessary on account of the deviations of gases. The readings of the air thermometer would otherwise have no definite meaning.
[5] Cf. the discussion on absolute temperatures between Schreber (Wiedemann's *Annalen*, Vol. LXIV, 1898, pp. 163–185, Vol. LXV, 1898, pp. 648–654, Vol. LXVI, 1898, pp. 1186–1190) and Auerbach (*ibid.*, Vol. LXIV, 1898, pp. 754–758).

CHAPTER XVIII (pp. 295–305)

[1] [Most of this chapter, together with other things, is already contained in Mach's

lecture 'On the Principle of the Conservation of Energy' (*Popular Scientific Lectures*, 3rd. edn., Chicago and London, 1898, pp. 137—185) and his important work of 1872 translated under the title *History and Root of the Principle of the Conservation of Energy* (Chicago and London, 1911).]

[2] ['On Matter, Living Force, and Heat', Joule's Scientific Papers, London, 1884, Vol. I, pp. 265—276: Reynolds's *Memoir of James Prescott Joule*, Manchester, 1892, pp. 2—16.]

[3] [Cf. Planck, *op. cit.*, p. 37; A Wangerin, *Franz Neumann und sein Wirken als Forscher und Lehrer*, Brunswick, 1907, pp. 107—124.]

[4] If Mayer's theory of energy had first gained a foothold in the domain of electricity, he would have sought in vain for a mechanical equivalent of quantity of electricity to satisfy his needs. But he would have searched until he came across electrical energy, and this would have satisfied his formal needs.

CHAPTER XIX (pp. 306—319)

[1] According to the apposite remark of Popper (*Die physikalischen Grundsätze der elektrischen Kraftübertragung*, Vienna, 1884, p. 9) *gh* should be put everywhere instead of *h*. In order not to disturb the conformity with well-known expressions, I write *h*.

[2] *Conservation of Energy*, pp. 43—47, and 85 of Chapter XVIII above.

[3] For the last twenty years it has been my habit to discuss such examples in my lectures.

[4] Cf. *Conservation of Energy*, pp. 85—86.

[5] *Mechanics* p. 601, Fifth edn.; and *Sitzungsberichte der Wiener Akademie*, Vol. CI, Abt. IIa, 1892, pp. 1589—1612.

[6] In my *Conservation of Energy* (p. 86, 1. 10, 11) "velocity" must be replaced by "square of velocity".

[7] *Die physikalischen Grundsätze der elektrischen Kraftübertragung*, Vienna, 1884.

[8] *Die Lehre von der Energie*, Leipzig, 1887.

[9] *Das Intensitätsgesetz*, Frankfurt A.O., 1888.

[10] *Zeitschr. für phys. Chemie*, Vol. VII, 1891, pp. 544—585.

[11] 'Studien zur Energetik'. II. *Berichte der sächs. Gesellschaft* (Leipzig), 1892, pp. 211—237.

[12] Popper mentioned my little book, but his work was certainly independent of my own; Helm also mentioned my book; Wronsky only knew of Helm's book; Meyerhoffer, whose comparative investigation appeals to me very much although I often cannot agree with his results, seems to have become acquainted with my book only after his own work was finished; Ostwald mentions no predecessors at all with respect to the questions here discussed. Popper's tract seems to me the richest from the point of view of the light that it throws upon the theory of knowledge; and yet it was not reviewed in any physical journal.

[13] It thus seems to me not quite correct for Meyerhoffer (*loc. cit.*, p. 560) to say that the second law is identical with the first. Certainly Carnot had only the fall of level in view while Mayer was concerned only with the transformation of energy. But these are different sides of the same process, and they cannot be separated from one another.

[14] *Mechanics*, pp. 171—174, Fifth edn.

[15] A gas which expands without performing work retains its temperature. An expanding body of given electric charge necessarily undergoes a fall of potential. It seems as though this were connected with the absence of action at a distance in the case of heat; but whether this action is really lacking is questionable: nothing prevents us from regarding a thermoelectric current as simply a stream of heat, and then it has certainly an action at a distance (cf. the remark in my *Leitfaden der Physik*, 1891, p. 221, where also the action at a distance of chemical processes is indicated).
[16] Consider for example, the fundamental difference of the two conceptions of capacity.
[17] It is therefore quite correct to say with Meyerhoffer *loc. cit.*, pp. 568, 571) that we can find an analogue of entropy for every species of energy (cf. my *Mechanics*, p. 498), but it is incorrect that this quantity is increased by every equilibration of potential. This is only the case for heat. Also his double measurement of Q/T is incomprehensible to me, for I cannot admit a distinction between "temperature" and "number of isothermals". The "weight of heat" Q/T is not to be confused with the entropy. The latter may increase, but the value of $\Sigma(Q/T)$ remains the sum of the capacities for heat.
[18] I believe that we must distinguish between reversibility in Carnot's sense and the spontaneous periodicity of a process. If Meyerhoffer (*ibid.*, p. 569) is of the opinion that the equilibration of potentials of the same kind is always irreversible, we must remark that such equilibration is only possible for heat. Other potentials of the same kind do not enter into an equilibration without a transformation, and often in this transformation the same states recur periodically.
[19] This peculiarity of heat may depend on its lack of inertia, that is to say, on the fact that by the differences of temperature *velocities* of equilibration are determined not *accelerations* of equilibration. The case is similar with differences of potential, if these differences are small enough or the damping great enough. We might also say that electrical energy is transformed by resistance into heat, but thermal energy again into heat.
[20] Cf. *Mechanics*, p. 225, and *Analysis of Sensations*, p. 351. Hasty readers of my *Conservation of Energy* have supposed that I there (p. 62) denied the existence of any irreversible processes. But there is no passage which could be so understood. What I said about the expected "death of heat" of the universe I still maintain, not because I suppose all processes to be reversible, but because phrases about "the energy of the *universe*", "the entropy of the *universe*", and so on, have no meaning. For such phrases contain applications of metrical concepts to an object which cannot be measured. If we could actually determine the "entropy of the universe", this would be the best absolute measure of time, and the tautology which lies in the phrase about heat-death would be quite cleared up. Such expressions might be allowed to a Descartes, but they cannot stand before modern scientific criticism. Also from the similarity of different energies emphasized by me in the book mentioned above does not follow the absence of all differences of these energies. It is also to be remarked that even processes which can be reversed all contain an irreversible element in the velocity, acceleration, and so on, — namely time.
[21] I have tried to show in my lecture of 1883 given before the Electrical Exhibition of Vienna how the concept of "quantity of electricity" results quite correctly from the divisibility and transferability of electric force. See the journal *Lotos*, Prague, 1884, and my *Popular Scientific Lectures*, pp. 107—136.

[22] Thus I expressed the matter correctly when I said in my *Conservation of Energy*, p. 73, that the law of the *conservation of energy* differs from other laws of nature by its form. We can easily give a similar form to any other law of nature — Mariotte's law, for instance.
[23] Cf. *Mechanics*, p. 498.
[24] Cf. my article in the *Zeitschr. fur phys. und chem. Unterricht*, 1887, p. 6, and §16 of Chapter II above.
[25] Carnot did not use the absolute zero-point. For this reason he can only determine its economic coefficient empirically. A comprehension expression was lacking with him. However, nothing prevents us imagining the cyclical process of Carnot carried out in a gaseous atmosphere of any desired tension instead of in a vacuum, thereby making the zero-point of temperature arbitrary.
[26] The zero-point of the level of velocity, of the level of electrical potential, and so on, may vary on earth should the earth come into contact with some other body in the universe. Thus there is no meaning in speaking of an "energy of the earth", let alone of an "energy of the universe". Is the zero-point of gaseous tension alone independent of all possible occurrences?
[27] Cf. my lecture of 1882 'On the Economical Nature of Physical Enquiry', *Pop. Sci. Lect.*, p. 203; *Mechanics*, p. 502; *Analysis of Sensations*, p. 332; and *Monist*, 1892, p. 207 note.
[28] *Conservation of Energy*, p. 73.
[29] This point has been known to me for twenty years and I have often discussed it in my lectures. Originally the usability of the conception of entropy seemed to me to be in question, but I soon found the explanation here given. The remark is not new, but is found in a somewhat different form in a publication which has apparently not had a wide circulation and to which my attention was directed only a short time ago. It is Planck, *Ueber den zweiten Hauptsatz der mechanischen Wärmetheorie*, Munich, 1879.
[30] The law of entropy with respect to irreversible processes contains an incomplete statement since an inequality leaves something undetermined.
[31] In my *Conservation of Energy* I limited my derivation of the principle of energy to cases in which the processes can be reversed. In other cases, an attempt to make the principle plausible would certainly fail; in the last instance it remains a purely arbitrary and idle view. A better terminology here appears desirable. William Thomson (1852) seems to have felt this first and F. Wald clearly expressed it. We should call the work which corresponds to a quantity of heat which is present, its mechanical "substitution value", say, while the work which corresponds to the transition from a state of heat A to the state B alone deserves to be called the "energy value of this variation of state". In this way the arbitrary conception as substance would be retained and misunderstandings avoided. Cf. Wiedeburg, 'Wärmestoff, Energie, Entropie', *Zeitschr. fur physikalische Chemie*, Vol. XXIX, 1889, pp. 27—50.

CHAPTER XX (pp. 320—326)

[1] *Proc. Roy. Soc.*, No. 148, 1873; [*Collected Papers in Physics and Engineering*, pp. 307—317.]
[2] See the rich literature in Nernst *Theoretische Chemie*, Stuttgart, 1893. [English translation by Codd, London, 1923.]

[3] Hemholtz's theory is in connection with experiments of James Moer concerning concentration currents, which were carried out in Helmholtz's laboratory (*Monatsber. der Berliner Akademie*, NO. 8, 1877, Dec. 19, 1878).
[4] [Helmholtz, *Sitzungsber. der Berliner Akademie*, Vol. V, Feb. 2nd, 1882; *Gesammelte Abhandlungen*, Vol. II, p. 958.]

CHAPTER XXI (pp. 327–332)

[1] The considerations under discussion have only the starting-point of the principle of comparison in common with Jaumann's "chemical theory" (*Sitzungsberichte der Wiener Akad.*, Vol. CI Abt. IIa, May, 1892). I cannot regard Jaumann's attempt as a solution of the questions referred to, though I am of the opinion that the points debated by him are well worthy of discussion.
[2] F. Wald derives the law of chemical proportions, in a manner that appeals to me very much, not from the atomic theory but from the general delimitation of chemical facts. But in the same paper he carries on a controversy, without naming me, against the view here mentioned as possible; iron remains iron and chemically different from lead. I must confess that this seems to me inconsistent not only with my point of view but with his. There is only *one* mass (as an essentially *mechanical* concept), no particular mass of iron which would be different from that of lead, just as little as the thermal quantity or electric quantity of iron is different from that of lead. If anyone has such a powerful need to regard iron and lead as materially and fundamentally different in some way not more clearly explicable, then an aversion to the atomic theory, which meets this need so simply and naturally, is simply incomprehensible to me. The relations of mass and other physico-chemcial properties are given by chemical laws. What possible meaning, then, is there in transferring these properties to mass in particular? Wald's paper is in the *Zeitschr. für phys. Chemie*, Vol. XXII, 1897, p. 253.
[3] This discontinuity has been referred to by Popper, *Die physikalischen Gesetze der elektrischen Kraftübertragung*, Vienna, 1884, p. 25, note.
[4] Cf. Mach, *Leitfaden der Physik*, Prague, 1891, §317, p. 221.

CHAPTER XXII (pp. 333–335)

[1] *Sitzungsberichte der Wiener Akademie*, Feb., 1866.
[2] *Die Energie und ihre Entwerthung*, 1889, p. 104.

CHAPTER XXIII (pp. 336–337)

[1] See the details given later on.

CHAPTER XXIV (pp. 338–349)

[1] H. Hankel, *Zur Geschichte der Mathematik im Alterthum und Mittelalter*, Leipzig, 1874, p. 301.

[2] Comte, *Cours de Philosophie positive*, Paris, 1852.
[3] Tylor, *Primitive Culture*, cited in Chapter IV, §4.
[4] Simony, *In ein ringformiges geschlossenes Band einen Knoten zu machen*, Vienna, third edition, 1881.
[5] See Max Dessoir, 'The Psychology of Legerdemain', *The Open Court*, Nos. 291—295, 1893.
[6] *Die Grundsätze der elektrischen Kraftübertragung*, Vienna, 1884.

CHAPTER XXV (pp. 350—358)

[1] *Popular Scientific Lectures*, pp. vii, 61, 63—64.
[2] *Ibid.*, pp. 214—235.
[3] *Einige Ideen zur Schöpfungsgeschichte*, Leipzig, 1873.
[4] Theorie der Vorgänge in der lebendigen Substanz, *Lotos*, 1888.
[5] *Kritik der reinen Erfahrung*, Leipzig, 1888.
[6] Ueber Maxima, Minima und Oekonomie., *Vierteljahresschrift für wissenschaftliche Philosophie*, 1891.
[7] Der zweite Hauptsatz der mechanischen Wärmetheorie, *Almanach der Wiener Akademie*, 1886.

CHAPTER XXVI (pp. 359—362)

[1] Ueber die oekonomische Natur der physikalischen Forschung, *Almanach der Wiener Akademie*, 1882; *Popular Scientific Lectures*, pp. 186—213, *Mechanics*, Fifth edn. pp. xiv—xv and 578—596; *Analysis of Sensations*, pp. 49–50, 328–329.
[2] 'Maxima, Minima und Oekonomie', *Vierteljahresschrift für wissenschaftliche Philosophie*, 1891. [Cf. *Mechanics*, pp. 607—608.]
[3] *Mechanics*, pp. 555—556.
[4] Cf. *Mechanics*, pp. 473, 566.
[5] *Open Court*, 1894, No. 375.

CHAPTER XXVIII (pp. 371—377)

[1] To obtain an idea of the extent to which the cries of animals are inborn and the extent to which they are a product of imitation, I once proposed to a celebrated physiologist the plan of interchanging the eggs of house doves and turtle doves brooding some distance apart. But the experiment could not be carried out from our inability to obtain birds which were brooding simultaneously.
[2] A colleague of mine, a Jew, assured me that he was able to recognize a Jew by the sound of a single word, even without seeing him. I believe that I may assert the same with reference to Slavs. And while entire words are certainly not innate, as Psammetichus (*Herodotus*, II., 2) believed, certain characteristic phonic elements are nevertheless inborn in every race.
[3] Young animals perform the movements characteristic of their species at a very early age and after the manner of a piece of mechanism. The sparrow is observed to hop

only, for the reason that he moves mostly from branch to branch on trees where this sort of movement alone is possible. The lark, on the other hand, is seen to run only. Might it not be possible to confine several generations of sparrows to level ground, and in this manner to teach them to run? Such a transformation of habits would doubtless be effected more easily than an anatomic one, and yet would have sufficient weight with respect to the Darwinian theory. The experiment is allied in character to that mentioned above with the doves.

4 *Analysis of the Sensations*, 1897, pp. 82—83.

5 Lubbock took boxes bearing the inscription "Bread", "Meat", "Milk", and succeeded in training his dog to *distinguish* them. But this was unquestionably by the aid of some other characteristic than the inscription. An instance of the common overestimation of the intellect of dogs is the following. A young dog learns to "beg" for sugar. One day it is observed that, while alone in the room with a canary-bird which has a piece of sugar attached to its cage, the dog of its own accord begins to "beg" for it. This act is interpreted as an appeal to the canary-bird, whereas it is nothing but a simple association of the movement with the sight of the sugar. Think of the number of analogies and of the long series of associations which would have to be at the disposal of the dog if this interpretation were correct. The dog would be in the position of the negro who begs from a fetish what he cannot possibly get from a fetish. Paradoxical as it may sound, a far higher degree of reason is required for so colossal a piece of stupidity than is at the disposal of a dog.

6 *Sprache und Vernunft*, Stuttgart, 1868.

7 In his three works *Ursprung der Sprache, Das Werkzeug,* and *Logos.*

8 Compare, for example, my *Analysis of the Sensations*, pp. 160 *et seq.*

9 Compare Marty, *Ursprung der Sprache*, Würzburg, 1875.

10 Compare *Mechanics*, pp. 578—9.

CHAPTER XXIX (pp. 378—383)

1 It is well known that, after children have once been weaned, they can be brought to take the breast again only with great difficulty. But it may be necessary in cases of illness to make them do so, and having noticed, in one instance of this kind, that the movements of sucking were actually performed during sleep, I took advantage of the situation and caused the child, while asleep and unconscious, to be laid to its mother's breast, with the result that the desired movements occurred and the difficulty was overcome.

2 Compare my *Analysis of the Sensations*, Chicago, 1897, note on p. 37.

3 Compare W. James's *Psychology*, New York, 1890, Vol. II.

4 See my *Contributions to the Analysis of the Sensations*, Chicago, 1897, pp. 44 *et seqq.*

5 See the articles on 'The Forms of Liquids' and 'On Symmetry' originally published at Prague in 1872, and now embodied in the English translation of my *Popular Scientific Lectures*, third edition, Chicago, 1898. Compare also Soret, *Sur la perception du beau*, Geneva, 1892, which carries the aesthetic considerations much farther than my work, but does not go so deeply into the fundamental psychological and physiological conditions as does my *Analysis of the Sensations.*

6 Compare Mach, in Fichte's *Zeitschrift für Philosophie*, 1865.

[7] So long as this hazy picture is regarded as the main thing, no understanding of the concept can ever be reached. E. C. Hegler has ingeniously compared the picture in question to Galton's composite photographs, which are obtained by superposing upon one another the pictures of the members of a family, so that the differences are obliterated and the common features of the family brought into more prominent relief (see Paul Carus's *Fundamental Problems*, Chicago, 1889, p. 38). I have compared this accompaniment of the concept to the ancient Egyptian paintings which combine in a single picture things which can be seen only by different views of it. ('On the Economical Nature of Physical Inquiry', *Popular Scientific Lectures*, Chicago, 1898, p. 186). In my *Analysis of the Sensations*, pp. 160 *et seqq*., I have given what I believe to be a more satisfactory explanation of the question. I have here to mention two other books which appeared soon after the first German edition of this work: H. Gomperz, *Zur Psychologie der logischen Grundthatsachen*, Vienna, 1897, and Theodule Ribot, *L'évolution des idées générales*, Paris, 1897, [of which an English translation has been published at Chicago under the title *The Evolution of General Ideas*]. In both of them, especially in the latter, are to be found points of contact with the presentation given in this place. But, although Ribot mentions my *Analysis of the Sensations* on a question which is quite by the way, the fairly extensive discussion of conceptual thought which is given in that place appears to have escaped him.
[8] Compare *Analysis of the Sensations*, p. 162.
[9] Despite all that has been said to the contrary, I find it difficult to comprehend that the innervation of a movement does not come directly to consciousness in some manner. The consequences only of the motion are said to be brought to consciousness by its sensations in the skin, and the mere memory of these sensations is said to be sufficient to produce the movement again. It is quite true that we do not know how we perform a movement, but only what the movement is and that we wish to perform it. When I *will* to go *forward*, this psychical act is to my feeling in no wise opposed to the memories of the sensations which take place in my legs, but appears to me far simpler. It was once attempted to identify all sensations of movement with sensations of the skin, etc., but it is to-day more probable that these sensations proceed in a far simpler and consequently more reliable manner from definite, specific organs. If my view is correct, then that sharp, delicate, and trustworthy feeling for the reactions belonging to certain concept is much more easily intelligible. It appears to me as if one could speak in more than a merely figurative way of the innervation of imagination.
[10] I have had frequent occasion to observe the power which latent psychical elements possess. Approaching, while absorbed in some thought, the house of a friend upon whom I intended to call, I have more than once surprised myself in the act of drawing forth my own latch-key. The word in other cases may call forth the same result as did the sight of the door in the present instance, without arousing to consciousness everything which corresponds to this symbol.

CHAPTER XXX (pp. 384—390)

[1] Cf. *Analysis of the Sensations*, p. 166; cf. pp. 328—330.
[2] The above formulation is — at least for the investigator of nature whose aims are epistemological — quite sufficient, and at the same time rules out philosophical word-

ings, which are always adapted to merely one-sided and temporarily tenable standpoints (idealism, realism, and so on). That I have not thought of replacing ordinary speech, or even the everyday language of men of science, by a new one will without doubt be put to my credit, as also that the simple considerations which Boltzmann advanced ("Ueber die Frage nach der objectiven Existenz der Vorgänge in der unbelebten Natur", *Sitzungber. der math.-nat. Cl.d. Wien. Akademie*, Bd. 106, Abt. IIa, Jan., 1897) were long familiar to me. The questions raised there merely concern our taste in words. Nothing is changed by such questions in the matter itself.

[3] Cf. *Analysis of Sensations*, pp. 192–3.

[4] *Ibid.*, p. 333.

[5] *Mechanics*, pp. 239–242.

[6] *Mechanics*, pp. 264–265.

[7] *Mechanics*, pp. 240–243.

[8] *Ueber die grossen Agentien der Natur*, Bonn, 1885.

[9] The physicist, Sohncke, whom I highly esteemed, and who has unfortunately died since these words were written, was offended by them. Of course I had no intention of giving offence, and there was no reason for it. Indeed, it was clearly pointed out that a hypothesis can have great heuristic value as a working hypothesis and, at the same time, very slight value as a theory of knowledge; and this I wish here to emphasize once again. My point of view with regard to the atom theory is that of J. B. Stallo, whose excellent book, *The Concepts of Modern Physics* (London, 1890), I have lately become acquainted with. To its fundamental exposition I can here merely refer. Here also I must refer to the works of E. Wald, in which he attempts to make chemistry independent of the atomistic philosophy. (*Zeitschrift für physikalische Chemie*, XXII.2, XXXIII.1, XXIV.4, XXV.3, XXVI.1, 1897–1898).

I have, therefore, no objection to make when Boltzmann praises the advantages of the atomic theory, above all other conceptions, for the physicist. The investigator is not only permitted, but he is expected, to employ all means that can assist him. I should be misunderstood, if a bias for the assumption of a continuous plenum were attributed to me. Light rays were investigated long before their periodicity was established. Why should not this also happen with the space-content? All that I oppose is the permanent *adherence* to arbitrary accessories of the facts. I cannot agree with Boltzmann's view concerning the slight use of the *volume element*. I ascribe to the volume element, merely with an altered scale of measurement, such characteristics as are observed in extended bodies; and experience has taught me that the standard of measurement can be diminished to any extent, without the form of the fact being changed. There is thus nothing at all hypothetical about it, and no obscurity whatever. Kirchhoff knew quite well just why he preferred these ways of thinking to any others. The volume elements, with their falls of temperature, behave exactly like finitely extended bodies under similar circumstances; but I have this advantage, that I can build up, out of such small volume elements, any case however complicated, with whatever exactness I desire. I cannot understand, therefore, why every differential equation must necessarily be based upon atomistic views. (Cf. Boltzmann, Ueber die unentbehrlichkeit der Atomistik, *Annal.d. Physik u. Chemi*, Vol. 60, p.231, 1897.)

The position which K. Pearson takes up in his work, *The Grammar of Science* (London, 1892), in regard to the atomistic philosophy, is well fitted to reconcile the contradiction alluded to.

[10] *Mechanics*, p. 239.

CHAPTER XXXI (pp. 391—397)

[1] The reply will be made, that the psychical is not determined throughout merely by the physical. I know that very well. But, the question here is not concerning my opinion, but concerning that which must naturally have presented itself to Descartes and Leibniz, and which was the necessary condition of their systems.

CHAPTER XXXII (pp. 398—401)

[1] Cf. *Popular Scientific Lectures*, pp. 259—281.
[2] Carus, *The Philosophy of the Tool*, Chicago, 1893, pp. 18—19.
[3] As noted before.
[4] According to a statement of R. Franceschini.
[5] He expresses the noteworthy fact that a conception continues, so to speak, to live and act without being in the consciousness. This happens when a word is yet used correctly without the corresponding intuitive conceptions being clear to us. In this connection, the excellent observations of W. Robert concerning dreams (Hamburg, Seippel, 1888) should have an illuminating effect. Robert has observed that the series of associations, interrupted and suspended during the day, continue to spin themselves out as dreams at night. A burning match, for example, that one has been prevented from extinguishing by some incident, may give rise to the dream of a conflagration, and so on. I have found Robert's observations confirmed in my own case, in innumerable instances, and can also add that one spares oneself unpleasant dreams if one thinks out completely, during the day, unpleasant thoughts that arise by chance, or if one talks them over, or writes them out: a procedure to be recommended also to persons inclined to gloomy thoughts. One can also observe phenomena, related to those of Robert, in the wakeful state. Thus, I am in the habit of washing my hands after I have received a handclasp from a damp perspiring hand. If I am prevented from doing this by a chance circumstance, an uncomfortable feeling remains with me; but the reason for it I sometimes forget, and I am not freed from it until it occurs to me that I should wash my hands and I have done so. It is also quite likely that the conceptions once established, even if they are no longer in the consciousness, continue their life. This seems to be particularly intensive if they are prevented, at the entrance into consciousness, from releasing the associated conceptions, motions, etc. they then seem to act as a kind of *charge*. Though the associative connections that are formed in dreams are so feeble that one dees not remember them even immediately, they still leave *traces* behind; and it is intelligible that, on awakening, a *new* psychical situation comes into operation. Somewhat related phenomena are those which Breuer and Freud have briefly described in their book on hysteria. [This is a remarkable foreshadowing of later work.]
[6] *Ansprachen und Reden bei der Helmholtz-Feier*, p. 55.

CHAPTER XXXIII (pp. 402—414)

[1] Liebig, *Induktion und Deduktion*, Akademische Rede, Munich, 1865.
[2] This may at first have been very roundabout and may have been split up into many special cases. To-day the proof is very easy to carry out by imagining the triangle

moved parallel and perpendicularly to the hypotenuse, so that the hypotenuse describes a square and the sides containing the right angle describe parallelograms which are evidently together of the same area as this square. Further, the parallelograms are equal in area to the squares on the sides of the triangle which respectively describe them. Details concerning the history of the theorem are given in Cantor's *Vorlesungen über Geschichte der Mathematik* [Vol. I, 3rd. edn., Leipzig, 1907, pp. 179—186].

[3] Cantor, *op. cit.* [pp. 153, 170, 177, 181, 187, 240].

[4] *Abhandlungen aus dem Gebiete der Mathematik*: Festschrift zum Jubiläum der Universität Wurzburg, 1882.

[5] Against some of my older assertions with respect to unique determination, Petzoldt has brought forward objections which have caused me to think. I must reserve the discussion of these objections for a later opportunity.

[6] Cf. Zindler, *Beiträge zur Theorie der mathematischen Erkenntnis*, Vienna, 1889; Meinong, *Hume-Studien*, Vienna, 1877.

[7] P. Volkmann, 'Hat die Physik Axiome?', *Physikal. Ökonom. Ges. Königsberg*, April 5th, 1894.

[8] In the chemical example, we are able to reach no certainty as to whether the possible cases are exhausted.

CHAPTER XXXIV (pp. 415—416)

[1] With regard to the aesthetic side of science, cf. Popper, *Die technischen Fortschritte nach ihrer aesthetischen und culturellen Bedeutung*, Leipzig, 1888.

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