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HEINRICH HERTZ:
CLASSICAL PHYSICIST, MODERN PHILOSOPHER

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Heinrich Hertz

HEINRICH HERTZ: CLASSICAL PHYSICIST, MODERN PHILOSOPHER

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PREFACE

The sub-title of this symposium is accurate and, in a curious way, promises more than it states: *Classical Physicist, Modern Philosopher*. Heinrich Hertz, as the consummate experimentalist of 19th century technique and as brilliant clarifying critic of physical theory of his time, achieved one of the fulfilments but at the same time opened one of the transition points of classical physics. Thus, in his 'popular' lecture 'On the Relations Between Light and Electricity' at Heidelberg in the Fall of 1889, Hertz identified the ether as henceforth the most fundamental problem of physics, as the conceptual mystery but also the key to understanding mass, electricity, and gravity. Of Hertz's demonstration of electric waves, Helmholtz told the Physical Society of Berlin: "Gentlemen! I have to communicate to you today the most important physical discovery of the century."

Hertz, philosophizing in his direct, lucid, pithy style, once wrote "We have to imagine". Perhaps this is metaphysics on the horizon? In the early pages of his *Principles of Mechanics*, we read

> A doubt which makes an impression on our mind cannot be removed by calling it metaphysical: every thoughtful mind as such has needs which scientific men are accustomed to denote as metaphysical. (PM23)

And at another place, concerning the terms 'force' and 'electricity' and the alleged mystery of their natures, Hertz wrote:

> We have an obscure feeling of this and want to have things cleared up. Our confused wish finds expression in the confused question as to the nature of force and electricity. But the question is mistaken with regard to the answer which it expects. (PM 76)

And after removing contradictions among the known relational properties, Hertz points out:

> When these painful contradictions are removed, the question as to the nature of force will not have been answered; but our minds no longer vexed will cease to ask illegitimate questions.

* * *

The contributors to this book explore the striking anticipations of philosophy of science of our twentieth century in the works of Hertz, in his laboratory physics and in his epistemological reflections. And they investigate Hertz through his great influence upon the history of physics. I must not summarize the fascinating essays which follow, but we may be alert to a few 20th century thinkers waiting in the wings following upon Hertz: Wittgenstein, Carnap, Einstein, Cassirer.

* * *

D. Baird et al. (eds.), Heinrich Hertz: Classical Physicist, Modern Philosopher, ix–x.

Hertz's realism deserves his own comment, from the conclusion to his introduction to his collected papers on *Electric Waves*:

It is not particularly satisfactory to see equations set forth as direct results of observation and experiment, where we used to get long mathematical deductions as apparent proofs of them. Nevertheless, I believe we cannot, without deceiving ourselves, extract much more from known facts than is asserted [in Hertz's papers]. If we wish to lend more colour to the theory, there is nothing to prevent us from supplementing all this and aiding our powers of imagination by concrete representations ... But scientific accuracy requires of us that we should in no wise confuse the simple and homely figure, as it is presented to us by nature, with the gay garment which we use to clothe it. Of our own free will we can make no change whatever in the form of the one, but the cut and colour of the other we can choose as we please. (EW 28)

* * *

Max von Laue, in his moving tribute to Hertz, says that Boltzmann responded to the 1890 paper 'On the Fundamental Equations of Electromagnetics for Bodies at Rest', by saying with astonishment, quoting Goethe's *Faust*, Part I, "Was it a God, who traced this sign?". Helmholtz, grieving the loss of his beloved student after Hertz's death so young, wrote "In the old classical days, it would have been said that he has fallen victim to the envy of the gods". Hertz's last letter, to his parents, 8 December 1893, surely helps to transcend our melancholy:

If anything should really befall me, you are not to mourn; rather you must be proud a little and consider that I am among the especially elect destined to live for only a short while and yet to live enough. I did not desire or choose this fate, but since it has overtaken me, I must be content; and if the choice had been left to me, perhaps I should have chosen it myself.

ROBERT S. COHEN

EDITORIAL NOTE

Heinrich Hertz died on January 1st, 1894. His last work, *The Principles of Mechanics*, appeared a few months later. Together with the introduction to his collected papers on *Electric Waves* it established his posthumous reputation not only as the consummate classical physicist but also as a thinker who exerted a profound and lasting influence on modern philosophy.

To commemorate the centennial of Hertz's death, and to bring together for the first time the most prominent Hertz scholars from around the world, the Philosophy Department at the University of South Carolina hosted in March of 1994 the conference 'Heinrich Hertz: Classical Physicist, Modern Philosopher.' That conference was made possible by a generous conference grant from the National Endowment for the Humanities. The chapters of the present volume are based on contributions to the conference.

Aside from the NEH and the politicians prudent enough to ensure its continued funding, we are grateful for editorial assistance to Alexander Goroncy, Maggie Haughton, Omar Lughod, Joan Spencer-Amado, Joanna Woodward, and especially Kaloyan Hariskov.

Throughout this volume, we refer to the English editions of Hertz's *Principles of Mechanics* (PM), *Electric Waves* (EW), *Miscellaneous Papers* (Misc), and to the bilingual edition of his *Memoirs, Letters, Diaries* (MLD). A concordance to the German editions and to a recent anthology of Hertz's writings (Mulligan 1994a) is provided. The concordance doubles as an index to the passages from Hertz's work which are discussed in this volume. In the text, an author's name and a year of publication (e.g., Buchwald 1994a) refer to works which are included in the extensive Hertz-bibliography on pages 281–305 below. Bibliographic references for works cited which do not relate directly to Hertz are given in endnotes as they occur.

DAVIS BAIRD, R.I.G. HUGHES, ALFRED NORDMANN

DIETER HOFFMANN

HEINRICH HERTZ AND THE BERLIN SCHOOL OF PHYSICS[1]

I grow increasingly aware, and in more ways than expected, that I am at the center of my own field; and whether it be folly or wisdom, it is a very pleasant feeling. (MLD 137)

Heinrich Hertz wrote this in Berlin during the fall of 1880 to his parents in Hamburg. At this time, Hertz was 23 years old; he had just passed his physics exams under Hermann Helmholtz with flying colours and taken a position as assistant to his famous teacher. His enthusiastic statement about Berlin not only concerns his special and very successful situation at the time; above all, it reflects the fact that Berlin had developed into a leading international center of science, especially in physical research. In 1870, Berlin had not only taken over the position of the capital of the German Reich; this was also the year in which Hermann Helmholtz was called to Berlin University as director of its Institute of Physics. Through this appointment, the fame of physical research in Berlin began to spread to establish an international reputation by 1880. With the coming of Helmholtz, the general history of physics became closely connected with physics in Berlin for about half a century.[2] Apart from Helmholtz, many other scientists who contributed fundamentally to the development of physics worked there during this epoch. These include Max Planck and Albert Einstein above all, also Gustav Robert Kirchhoff, Friedrich Kohlrausch, Emil Warburg, Walther Nernst, Max von Laue, James Franck, Gustav Hertz, Erwin Schrödinger, Peter Debye etc. All of them are heroic figures in the history of physics, and many of them were awarded the Nobel prize in this century. During the twenties, Berlin was the place with the "highest density of Nobel prize winners" in the world. This development suddenly ended in January, 1933 when Hitler gained power; many Jewish and politically unpopular scientists were discharged by the Nazis from their positions and had to leave Germany. The famous Berlin School of physics was damaged, and outstanding physical research remained very rare for many decades.

The heyday of physics in Berlin did not come out of the blue; it resulted from a long historical process in which a remarkable level of physical research had already been reached by the mid-19th century. In 1842, Gustav Magnus founded a private laboratory, which gathered young and talented scientists and which soon became the center of one of the most important schools of physics in the 19th century. Most of the scientists who enjoyed great prestige in physics in Germany, and especially in Berlin, had worked in Magnus' laboratory.[3] Hermann Helmholtz, the successor to Magnus, pursued this tradition in every sense and crowned it with his genius. There was hardly an important physical subject or pressing problem of current

D. Baird et al. (eds.), Heinrich Hertz: Classical Physicist, Modern Philosopher, 1–8.

physics which he did not explore theoretically or practically, or whose investigation he did not suggest at his institute.[4] This can be seen most clearly in his research on electricity, which became the center of his work in Berlin. These investigations were centered around a critical assessment of the various competing theories of electromagnetic action. The latter was a very recent field of current research – particularly in Germany, where Maxwell's field theory had to compete with Wilhelm Weber's theory; Weber's was the leading Continental theory for the prediction of electrodynamic effects until the 1880s. The theoretical and experimental investigations of Helmholtz and his students contributed decisively to the acceptance of Maxwell's field theory and its further development in Germany and on the rest of the Continent. An impressive number of publications on electrodynamic problems shows that Helmholtz fought for a real understanding and an experimental testing of Maxwell's field theory during this period (Kaiser 1993). But it was Helmholtz' students and not Helmholtz himself who established the breakthrough. Under his tutorship, for instance, in 1876 the American Henry Rowland carried out a study of the magnetic field produced by a charged rotating disk to see if the field was the same as that produced by a current. Also under the influence of Helmholtz, the Austrian physicist Ludwig Boltzmann proved experimentally at the Berlin Institute the relation between the refraction index and the dielectric constant, a relation required by Maxwell's theory. The experiments of Albert Michelson, carried out in Berlin and Potsdam, should also be mentioned in this context. The Michelson experiment is not only an *experimentum crucis* for the special theory of relativity but for electrodynamics in general. Last but not least comes Heinrich Hertz to whom we return, shortly.

Although these investigations of the "Helmholtz school" were guided chiefly by a strong fundamental interest – the desire to bring electrodynamic theory into harmony with the conservation of energy – there was another research stream in Berlin which undoubtedly influenced the work of Helmholtz and his students. At the time, Berlin was not only a center of physical research but a leading center of electrotechnology and home to a very rapidly expanding electrical industry, the "Electropolis." This industry naturally was very interested in a high level of physical education and research; this could be realized in Berlin through a dense network of personal and institutional contacts. One was the close relationship between Helmholtz and Siemens, which developed in the circle of talented students around Magnus during the 1840s.[5] These subtly diverse points of view could communicate easily over a well-developed and mobile system of interscientific communication. The Berlin Physical Society, founded in 1845, and the Electrotechnical Association founded in 1879, played an outstanding role. As recent investigations have shown,[6] memberships in both societies were common and both occupational groups held talks at each other's colloquia. The high status of electrodynamic problems among the Berlin physicists can also be seen in the list of the topics of the physical dissertations. Electrodynamic subjects are the majority during the last decades of the 19th century. This was no accident, and a close interrelation between the highly developed electrical industry and the physical research community is well illustrated by the big upturn in the lamp and tube industry after 1900. Work on electric discharge and vacuum phenomena increased rapidly and reached a 20% share of published research.[7]

Such an excursion into the History of Science, Technology and Industry of Berlin should not, however, lead us into a one-dimensional explanation of the relation between the work of the Berlin physicists and the engineers at this time. But there still seems to be a very close interrelation in an "atmospheric" or communicative sense, which existed in Berlin between physics, electrotechnology and industry. There was a remarkable coincidence in time between the flourishing of physical research and the development of a highly productive electrical industry towards the end of the 19th century.

This phenomenon can be linked to Helmholtz; during his Berlin period, he not only was interested in fundamental questions of electrodynamics but in the very practical problem of electrical units. Furthermore he was one of the main initiators – together with his friend Werner Siemens – of the establishment of a special physical-technical institute, the profile of which was to be oriented towards fundamental research in a broad sense and directly towards technical practice as well. In this respect it was not accidental that he became the first president of this institute, the Imperial Physical-Technical Institute [*Physikalisch-Technische Reichsanstalt*], founded in 1887. Here many problems of contemporary physics and electrotechnology were intensively investigated (Cahan 1989).

When Heinrich Hertz moved from Munich to Berlin in the fall of 1878 to finish his studies, he entered this intellectually stimulating atmosphere. Hertz was familiar with Berlin, where he had served in the army two years earlier. Yet, there is no evidence that he contacted Berlin's scientific community while in the army. This is not true in other cases. For instance, Friedrich Engels attended lectures at the university during his military service. Hertz matriculated on October 23, 1878[8] and he soon gained the attention of his professors, most notably Helmholtz: "While he was going through the elementary course of practical work," Helmholtz recalled in 1894, "I saw that I had here to deal with a pupil of quite unusual talent" (PM xxv). Helmholtz sponsored Hertz's scientific studies during the following years, and a life-long friendship between student and mentor, stimulating for both, developed. Heinrich Hertz never disappointed Helmholtz's expectations.

It was Helmholtz who led Hertz to his first true appreciation of science. In the summer of 1878, Helmholtz had already posed a prize problem in physics for the Berlin University, an electrodynamical question to determine the extra currents which are produced incidentally when a current starts or stops. The magnitude determined for the extra current should allow conclusions as to the inertial mass of the moving electricity in a conductor. At Helmholtz's suggestion, Hertz took part in the contest, although as he wrote to his parents: "I am only *tentatively* working on it" (MLD 95). In August, 1879, he was awarded the prize, a gold medal plus 25 ducats.[9] His investigation was so excellent that it received unrestricted praise from the faculty including most notably, Helmholtz. Hertz reported to his parents that "not only did I win the prize, but also the judgment of the faculty was expressed in terms of such commendation that I feel twice as proud of it. Most of the other entries, even the winning ones, were treated as students' excercises ... but not mine" (MLD 111).

A genius had bought his entrance ticket to the scientific world. Further brilliant feats followed in rapid succession. At first, his prize-winning investigation was revised for publication (Misc 1–34). His subsequent doctoral thesis was a theoretical analysis of the rotation of metal spheres in a magnetic field (Misc 35–126). Hertz finished it in January, 1880. This was very unusual, because by that time Hertz had studied only three semesters in Berlin. Consequently, it was necessary to get special permission from the ministry to proceed with the doctoral examination. This was given suddenly[10] and on February 5, 1880, the oral examinations took place. Helmholtz and Kirchhoff examined Hertz in physics, Kummer in mathematics and Zeller in philosophy. He passed with the highest grade possible "magna cum laude."[11] "Doctorates in my class," reported Hertz to his parents, "are very few in number, especially Helmholtz and Kirchhoff are said not to have awarded many" (MLD 121). One month later, on March 15, 1880, Hertz took the written exam, and, once again, defended his doctoral thesis successfully. He was now a "Berlin doctor," before whom all were to have respect as he noted in a letter.

External recognition came as well. After a deserved vacation, Hertz got a letter from his teacher, in which he was offered a regular position as an assistant in the Physical Institute. He became the successor of Wilhelm Giese, who had occupied the position since 1873. Hertz was overjoyed to take the position, and he worked for nearly three more years in Berlin, above all, at Helmholtz's side, his venerable teacher and mentor. During these years, Hertz had to help Helmholtz in his tutorial work, especially to supervise the practical courses of the students. Unfortunately, his hope to occupy the more prestigious job of demonstrator and assistant to Helmholtz's lectures, did not pan out. Heinrich Kayser continued in this work. There also was a third assistant at the Institute, Erich Hagen. With such a large and capable scientific staff (usually at an institute there was one professor and one assistant) and such a collection of first-class equipment, Berlin's Institute of Physics was the biggest and most prestigious in Germany.

Table: Heinrich Hertz and the Physical Society

Papers by Hertz:

Vertheilung der Elektricität auf der Oberfläche bewegter Leiter (1880).
Über ein neues Hygrometer (1882).
Über die Härte der Körper (1882).
Über die Spannung des gesättigten Quecksilberdampfes (1882).
Dynamometer (1882).

Reports on Hertz:

Bericht über Versuche der HHrn A. Kundt und H. Hertz (Helmholtz, 1888).
Bericht über Versuche von Hrn. Hertz (Helmholtz, 1888).
Optische Analogien zu den neuern Versuchen von Hrn. H. Hertz (König, 1889).
Nachruf an Heinrich Hertz (Kundt, 1894).
Gedächtnisrede auf Heinrich Hertz (Planck, 1894).

Hertz's research was very broad during his years as an assistant, what he called his "period of study." He carried out more theoretical investigations on problems in thermodynamics, meteorology and elasticity. In the latter, his dual attraction to physics and engineering is documented, because it led to a new measure of the hardness of solids. These results are still important today, above all for testing materials. In meteorology, he explored the theory of tides, ocean currents and trade winds. Furthermore, he dealt with the evaporation of liquids, especially of mercury, and he investigated a couple of other pressing problems of contemporary physics. His most important work was on the conduction of electricity in gases. Eugen Goldstein, the pioneer in the field and an early student of Helmholtz, exercised a lasting influence on Hertz. Hertz retained his great interest in cathode rays all his life, as his and Lenard's future fundamental investigations in Bonn demonstrate.

These years in Berlin seem crowded with intense work. Hertz wrote to his parents:

Time flies for me now as it has never flown before, and a week or two goes by before you know it. [...] I keep on working, only I find it even harder than before to stick to one particular subject; I have several in mind, and since the tools for all of them are available here, I think now about one and now collect the apparatus for the next – in short, my head and my time do not allow me to do all the experiments that could be done with the apparatus at my disposal. (MLD 139)

With this position and with his intellectual gifts, Hertz quickly was totally accepted by his colleagues. For instance, he often was a recognized speaker at the renowned Physical Society of Berlin (see table), which he entered on receiving his doctorate. In this connection, he reported to his parents in February, 1883:

I feel that I am becoming a sort of chatterer, always the one that talks, but it seems better, after all, that I do the speaking than that all remain silent and the society disperse hungry to speak. Moreover they are already used to turning to me when nothing else offers, and that is what happened this time as well; I had no intention of giving a lecture. But then it went quite well; my lecture right away stimulated another. (MLD 173)

Although Hertz was very attached to the stimulating scientific atmosphere of Berlin, for "how much more pleasant to get instruction verbally from persons who are thought of as the best informed in each field instead forever thumbing through those tiresome books" (MLD 141), he had to leave in summer, 1883. There was no real possibility to advance his scientific career in Berlin, at least for the immediate future. Consequently, he accepted an appointment at the University of Kiel: "Coming from the big world to which I am accustomed, I would have to fit myself into a presumably very small one" (MLD 179), he wrote a bit gloomily to his father. In fact, Hertz was not happy in this little Hanseatic town and at this small university. In 1885, he had already been appointed professor of physics at the Technische Hochschule in Karlsruhe. In 1889, he moved to Bonn. All this time, Hertz kept in very close contact with Berlin, above all with his teacher and mentor, Helmholtz.

Beginning in 1886, Hertz's most important work, his famous experiments on electromagnetic waves, were carried out in Karlsruhe. This work can be traced directly to Berlin. It originates in another of Helmholtz's prize problems, this time for the Prussian Academy of Science in 1879: "To establish experimentally any relation between electromagnetic forces and the dielectric polarization of insulators."[12] As he remembered just after Hertz's death in the preface to Hertz's *Mechanics*, Helmholtz had formulated the prize problem "in the belief, that Hertz would have an interest in it and would attack it, as he did, with success" (PM xxv). Hertz, of course, was interested, and Helmholtz even offered his student(!) all the assistance of the Physical Institute for solving this crucial problem. But, a careful analysis in 1879 led Hertz to the conclusion that "any decided effect could scarcely be hoped for, but only an action lying just within the limits of observation" (EW 1). Consequently, he put the problem aside, in favor of the other investigations described above. But, he had a keen mind and as he pointed out in the introduction to the second volume of his collected works:

> I still felt ambitious to discover it by some other method; and my interest in everything connected with electric oscillations had become keener. It was scarcely possible that I should overlook any new form of such oscillations, in case a happy chance should bring such within my notice. (EW 1f.)

Helmholtz became extremely interested in this question. For instance, his 1883 expert's report for the foundation of a special physical-technical institute concerns this question. As a central point of its research program, he proposed the task:

> In the theory of the magnetic actions of electrical currents a velocity, which appears to be exactly equal to that of light, and which W. Weber characterizes as critical, seems to play a fundamental part. Its identity with the velocity of light appears to me to indicate an essential and intimate relation between optical and electrical processes. We seem hereby to acquire a clue to the mysterious aspects of electromagnetic phenomena, which probably may lead us to their deepest foundation.[13]

But, by 1887, when the Berlin Physikalisch-Technische Reichsanstalt was finally founded, and its scientific work could begin (Cahan 1989), this question was already being dealt with by Hertz's important series of experiments. Hertz's experiments are well known, and I want only to point out that he discussed his progress and results immediately and in great detail with Helmholtz. Helmholtz continued to push Hertz's academic career by means of reports and personal recommendations, including that for Friedrich Althoff.[14]

Although Hertz himself doubted that his fundamental results "will attract very much attention," the high rank of Hertz's work was recognized by his colleagues and above all by Helmholtz immediately. The latter sent him a "Bravo" on a postcard (MLD 236) and in a letter to du Bois he used the phrase "very ingenious."[15] Furthermore he recommended the publication of Hertz's results in the proceedings of the Academy. He was even willing to pay the printing expenses himself, if the academy wouldn't accept his recommendation. In 1889, Helmholtz nominated Hertz to be a corresponding member of this notable society; he was elected in the same year.[16]

But Hertz did not want to go back to Berlin, at least not immediately. When Gustav Kirchhoff died in 1887, and after Ludwig Boltzmann had rejected an appointment to Kirchhoff's chair for mathematical physics, Hertz was now the favorite.[17] Helmholtz had delivered the opinion of the faculty, in which he praised him "as exceptionally qualified" for this position.[18] Hertz refused the call. Hertz wrote to his parents on October 5, 1887:

> I protested vigorously against that and assured them that I should very much prefer another university, that I was not really a mathematical physicist, that such a professorship would altogether tear me away from the projects in which I had been successful, that I was too young, etc. (MLD 263)

Although Helmholtz regretted that Hertz would not come to Berlin, he understood the decision of his former student, and wrote him: "A person who yet hopes of grappling with many scientific problems had better stay away from the big cities" (MLD 351). Max Planck came to Berlin instead, and Hertz moved from Karlsruhe to Bonn.

There is no doubt that Hertz, who was for Helmholtz "the most talented among the younger physicists and most replete with original ideas" (MLD 351), would sooner or later find his way back to Berlin, the most prestigious place for a scientist in the German Reich, probably as the successor of Helmholtz. Hertz was not merely his most talented student but the complete Helmholtzian physicist – one who is interested in each fundamental problem of contemporary physics and who surveys sovereignly all branches of physical research, from theory to experiment, from the technique of measurement to technical applications. Unfortunately, Hertz died before his "great master," who wrote of Hertz just after his death, "In the appointment of a successor to H. Hertz there can surely be no thought of finding someone who could replace this unique man" (MLD 353).

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NOTES

1 Dedicated to the 60th birthday of Hubert Laitko.
2 See Christa Kirsten and Hans-Günther Körber (eds.), *Physiker über Physiker* (Berlin: Akademie – Verlag, 1975).
3 Dieter Hoffmann (ed.), *Gustav Magnus und sein Haus* (Stuttgart: GNT-Verlag, 1995).
4 Helmut Rechenberg, *Hermann von Helmholtz: Bilder seines Lebens und Wirkens* (Weinheim: VCH, 1995), pp. 190 ff.
5 Dieter Hoffmann and Horst Kant, "Skizzen zur Entwicklung der Physik in Berlin: Institutionen, Personen, Wechselbeziehungen." *Berichte der Humboldt-Universität* **8:20** (1988), pp. 4–15.
6 Horst Kant, "Werner Siemens und sein Wirken im Berliner Elektrotechnischen Verein," in D. Hoffmann and W. Schreier (eds.), *Studien zu Leben und Werk von Werner von Siemens* (Braunschweig: PTB, 1995), pp. 117–134.
7 Volker May, "Jugendobjekt zur Geschichte der Physik" *Humboldt-Universität* **33/34** (1977), p. 10.
8 Archives of the Humboldt Universität Berlin, Philosophische Fakultät **252**, pp. 539–541, **1461**, pp. 177–184.
9 A ducat is a coin containing about 3.4 grams of gold.
10 Archives, *op. cit.*, (note 8), p. 540.

[11] *Ibid.*, p. 541.
[12] Adolf von Harnack, *Geschichte der Preußischen Akademie der Wissenschaften* (Berlin, 1900), vol. 2, p. 617.
[13] Bundesarchiv Potsdam, RMdI, 13144/8, Bl. 112.
[14] Dieter Hoffmann and Hubert Laitko, *Kompetenz, Autorität und Verantwortung: Hermann von Helmholtz als "Reichskanzler der Wissenschaften"* (Berlin: ERS-Verlag, in press).
[15] Christa Kirsten (ed.), *Dokumente einer Freundschaft* (Berlin: Akademie-Verlag, 1986).
[16] Kirsten and Körber, *op. cit.*, (note 2), p. 114.
[17] Archives, *op. cit.*, (note 8), 1461.
[18] *Ibid.*, Bl. 183.

MICHAEL HEIDELBERGER

FROM HELMHOLTZ'S PHILOSOPHY OF SCIENCE TO HERTZ'S PICTURE-THEORY

Until far into the second half of the last century, many efforts were made to follow the Newtonian paradigm in taking account of electrodynamic phenomena. One wanted to reduce them to attractive and repulsive central forces of electric particles which were supposed to act directly at a distance, in analogy to gravitational force. In later years, this view was superseded by a theory of contiguous action, i.e., electromagnetic field theory as developed by Michael Faraday (1791–1867) and elaborated by James Clerk Maxwell (1831–1879). According to this new view, the main role is not played by the carrier of the charges but by the medium which continuously transports the action from place to place. This transition to field theory is one of the major turning points in the history of physics. In the German speaking world, this change was brought under way and promoted mainly by Hermann von Helmholtz (1821–1894), although in the end he could not free himself from the idea of action at a distance after all. The ultimate establishment of the new theory and the defeat of the action at a distance view is due to his student, Heinrich Hertz (1857–1894).

The transition from action at a distance to field theory was not limited to physics and not merely a matter of one law superseding another. It also represents a philosophically important development which changed the concepts of matter and effect. The notion of action at a distance comprised a certain philosophical idea of material substance, of force and causality as well as a certain view of the peculiarity of scientific theories. These philosophical views which were deeply entrenched in physics became problematic as a result of the introduction of the field concept. The new conception of immediate action demanded a substitute for the old concepts of force and matter as well as a new philosophical conception of scientific theories and their relation to experience.

Hertz himself and his contemporaries were very much conscious of the philosophical dimensions of his work. Hertz noted that a "good part" of the approval for his discovery of electromagnetic waves "was due to reasons of a philosophic nature." Establishing the field concept's superiority by demonstrating the temporal propagation of electromagnetic action was, as Hertz wrote, "the philosophic result of the experiments; and indeed, in a certain sense the most important result" (EW 18f.). In his *Principles of Mechanics*, he wrote that he only attaches importance to the

> order and arrangement of the whole – the logical or philosophical aspect of the matter. According to whether it marks an advance in this direction or not, my work will succeed or fail in its objectives (PM xl).

D. Baird et al. (eds.), Heinrich Hertz: Classical Physicist, Modern Philosopher, 9–24.

Among the philosophers of his time, especially Ernst Cassirer (1874–1945) had a very high opinion of Hertz. For him, Hertz represented the Neo-Kantian conception of physical theory in its purest form, and he thought that with him "began a new phase in the theory of physical methods" (1950, 103). Also, Ludwig Wittgenstein's picture theory is an attempt to understand the relation of a sentence to the world in the same way as Hertz understood the relation of a physical theory to reality.[1] Although Hertz explicitly commented upon philosophical problems only on a few pages, he lastingly influenced the philosophy of the 20th century, at least via Cassirer and Wittgenstein.

In the following, I want to show how the philosophical consequences of Hertz's work arose from a Helmholtzian source in a twofold manner – from Helmholtz's philosophy of science on the one hand, from his electrodynamics on the other. In both respects, Hertz rigorously continued along the path which Helmholtz had shown him, and on it he finally went beyond his teacher.

HELMHOLTZ'S EPISTEMOLOGY

There are two fundamental questions that stand at the beginning of Helmholtz's philosophy.[2] The first question deals with the ontology of the outer world. What can be regarded as real in the physical sense? The second question concerns the proper epistemology. How do we get to know anything of this physical reality? Helmholtz developed the answers to these questions from the three interdependent perspectives of physicist, physiologist, and philosopher. He thought that the procedure of a physicist grasping reality was identical to the procedure of a human being perceiving external objects in space. He was convinced that in both cases the external reality of the objects is not given in the sensations themselves but that it is inferred from them.

Helmholtz answered the first question of ontology in the sense of a special realism that might appropriately be called: "metaphysical" or "hidden causes-realism." This kind of realism can be found for the first time in his memoir, "On the conservation of force" from 1847. It can be summarised in five points:

1. Every change in the physical world has a cause.
2. All these changes are caused by unchanging material substances. These substances form "the hidden and immutable ground of the phenomena" that "lies behind the change of appearances and acts upon us" (1903a, 2:241 and 1903b, 16).
3. The forces with which these substances are furnished, i.e., their capacities to produce effects, are immutable. A force has to be thought of as an unchanging attribute of substance.
4. Matter and force are given to us only in an abstract sense but never in direct experience. "Neither matter nor forces can be the direct object of observation, but always only the inferred causes of experienced facts." (1856, 454).
5. If we knew the causes of the appearances, we could derive all phenomena from them in a strict and unique way. We would then be in the possession of objective truth (cf. 1882, 1:17).

From these points, Helmholtz deduced several consequences for the way in which the relation of material substance and force should be conceived. He insisted especially that for logical reasons the concepts of substance (or matter) and force cannot be separated from one another. It would be inconceivable to suppose the existence of matter without an acting force or of a force without a material seat from which it arises (1882, 1:14 and 3:68; 1903b, 15; Koenigsberger 1902–03, 1:292). Matter without effects would be completely unknown to us since it could have effects neither on other matter nor on our senses. In the same way, we could not picture a force without attributing it to matter. Without such a material seat, force would be something "that, at the same time, should exist and yet should not exist since anything that exists we call matter" (1882, 1:14). Hence, all matter or substance possesses force and all force has to have a material seat.

From the notion that matter is immutable Helmholtz concluded that material substances can differ from each other only through their forces. "If we talk of different matters we see their diversity as resulting from the diversity of their effects, i.e., their forces" (1882, 1:14). Since the elementary forces are unchangeable, the only changes in nature have to be local changes of the underlying substances. Any other alteration would presuppose a variable force. Any real change in the world is thus caused by movement. The forces have to be conceived of as pure "moving forces whose actions are determined by their spatial relations" (1882, 1:15):

> If motion, however, is the basic change underlying all the alterations in the world, then all the elementary forces are moving forces. The final goal of the sciences is thus to find all the movements and driving forces supplying the foundation of all other change. In other words, the final goal of the sciences is to dissolve themselves into mechanics. (1903a, 1:379)

For Helmholtz, to understand nature meant to "reduce the natural phenomena to unalterable forces of attraction and repulsion whose intensity is dependent upon distance" (1882, 1:16).

This "hidden-causes realism" served as a solid frame for Helmholtz's thought throughout his life. In the course of time, however, he changed his opinion as to how this frame should be filled in detail. We can distinguish two different phases in his thought. In the first phase of his life, Helmholtz considered the underlying forces metaphysical. He was led to this view by Kant's *Metaphysical Foundations of Natural Science*, although he did not adopt Kant's dynamism. During this phase he was convinced that his five features of matter and force can be shown as constituting the *a priori* necessary and universally valid preconditions of science.

In the second phase of his thought, which must have started shortly before 1869, Helmholtz tried to modify his early metaphysical attitude and began to advocate an empiricist and phenomenalist approach. As he noted on several occasions, this change of mind was caused by acquaintance with the views of Faraday (cf. Heidelberger 1993, 481–483). Faraday's "principal aim," Helmholtz wrote:

> was to express in his new conceptions only facts, with the least possible use of hypothetical substances and forces. This was really an advance in general scientific method, destined to purify science from the last remnants of metaphysics. (1903a, 2:252; English version 1882, 3:53)

According to Helmholtz, Faraday was able to free himself from metaphysics in his explanations of electromagnetic phenomena "by excluding all presuppositions which comprise assumptions of hypothetical processes or substances which cannot be perceived directly" (1903a, 2:370). Instead of reducing the phenomena to the action at a distance of electric particles in the conductors, he saw the primary causes in the processes taking place in the dielectric medium. Helmholtz wrote:

Faraday detested any hypothetical tendency of putting in any forces [...] He in fact succeeded to find an interpretation [...] for the magnetic and electric forces [...] in which any mention of distance forces for the magnetic and electric and electromagnetic phenomena was dropped. (1903b, 12f.)

Faraday was able to explain the phenomena experimentally and in a way completely different from Helmholtz who had used what he deemed metaphysically necessary concepts. It finally had to become clear to Helmholtz that there is no metaphysics that can justify any preconceived conception about the nature of the ultimate substances and their qualities. Instead of presupposing ultimate characteristics of underlying causes as a necessity of thought, we have to try to discover and infer them from the laws of the appearances. Scientific method first of all has to establish the observational laws before it can deal with the underlying reality. "The first product of rendering the appearances intelligible by thinking are the *lawful relations* [*das Gesetzliche*]" (1903a, 2:240). Only once these laws are established beyond questioning can physics start its proper business and look for the real causes of the phenomena by experimental operations.

During his early metaphysical phase, Helmholtz was convinced that he had justified his view of material substances and forces as the invariable and necessary fundament on which science had to be erected. In the later phase, he viewed statements about the special constitution of forces as mere empirical hypotheses which are subject to confirmation but can never be exhaustively confirmed, let alone proven necessary. Force and substance are now to be seen as the hypothetical sums of the lawful relations between the phenomena. Since our experience is limited and will remain so, we will never have certain knowledge about the real laws behind phenomena. What we can grasp is only a dim reflection and a faint hint of an inaccessibly distant ideal. The "real," or Kant's "thing in itself" cannot be represented in positive terms. We can only approach it gradually through extending our "acquaintance with the lawlike order in the realm of the actual" (1903a, 1:242, cf. also 243).

We are never justified to claim that we have definitely reduced the phenomena to the "underlying substances and forces." Such a claim would be warranted "neither by the incompleteness of our knowledge nor by the nature of the inductive inferences on which all our perception of the real is based from the very first step" (1903a, 2:247). Some time later, Helmholtz expressed more optimism and claimed that "large regions of the phenomena of nature, especially the simpler circumstances in the inorganic realm, have completely been reduced to well-known and sharply defined laws" (1903a, 2:339).

Helmholtz held on to a metaphysical remnant of the ontology of the sciences even if his growing antimetaphysical scepticism makes strong concessions to the phenomenalism and positivism of his time. He never revised his view that the *true* causes of the phenomena are never directly given in experience and that our experiences are only signs from which we have to infer the true nature of the causes. The "lawlike order in the realm of the actual" is always "represented [*dargestellt*]" for us "in the notational system of our sensory impressions" (1903a, 2:242, cf. also 243). The objects to which the signs refer belong to a "completely different world" and cannot be compared with the sense-impressions (1856, 443).

We now have to deal with Helmholtz's second question referred to above, the question concerning epistemology: how is any knowledge of outer reality gained, i.e., how can we actually find out the hidden causes of the single phenomena? The conception from which Helmholtz developed his answer might best be called "experimental interactionism." It is based on Helmholtz's deep-seated conviction that "the actions realised by the will of man [*die durch den Willen gesetzten Handlungen des Menschen*] form an indispensable part of the sources of our knowledge" (1903a, 2:359). We can acquire knowledge only by actively and voluntarily intervening into the course of things. Passive observation alone cannot get anywhere.

> Relatively few but well performed experiments are enough to allow me to see the original causal conditions of an event with greater certainty than a millionfold observations by which I could not arbitrarily vary the conditions. (1856, 451f.; cf. also 1903a, 1:355 and 2:359)

Experimental interactionism forms the inner core of Helmholtz's methodology of science and of his theory of perception and it also remained unchanged throughout his turn towards phenomenalism and empiricism.[3]

Helmholtz illustrates the superiority of deliberate action over mere observation with the example of the expansion of mercury by heat (1856, 451). If we were able only to passively observe the expansion of mercury and not to produce the heating ourselves under varying conditions, we would have no means to exclude alternative hypotheses on the cause of the expansion. We could as well assume, for example, that the moisture of the surrounding atmosphere is the cause of the expansion. If, however, we can vary the conditions and experimentally create the expansion of the mercury ourselves, we can then identify heating as the cause of the expansion.

The same process by which physics finds out the causes of the phenomena also takes place in human sensory perception and generally in any acquisition of knowledge. Again, deliberate action plays the decisive role. "Any epistemology which is based on the physiology of the senses must advise man to proceed to action in order to take possession of reality" (1903a, 2:360). To perceive an outer material object means to develop a representation [*Vorstellung*] of its form and situation in space on the basis of sensory impressions.

> We learn how to make reliable judgments of the causes of our sense perceptions only when we place our sense organs into different perspectives to the objects at our own will. Such experimenting happens from early youth onwards. (1856, 452)

By deliberately varying the conditions, that is in this case, by making movements with our body, we find out that we can make certain sensations reappear at will and that other sensations occur or disappear independently of our intentions. Those impressions which can be changed by voluntary impulses are of a spatial nature. In this case we conclude that they come from a constant unchanged object in space and that the changes arising in the course of our actions are nothing but changes in the appearance of this object (1903a, 2:222ff.).

The perception of an outer material object differs from the experimental discovery of the cause of a certain physical phenomenon only by degrees of deliberateness. Whereas, discovering a cause in physics is the result of *conscious* reasoning, perception is the result of an *unconscious* inference. In both cases the type of inference employed has the same inductive structure.

Helmholtz's example of the expansion of mercury as well as his theory of perception make it obvious that, for him, the discovery of causes was the first and foremost function of an experiment, i.e., the discovery of the irreducible and unchangeable forces behind the phenomena. In order to discover the forces behind the appearances, we have to try to reproduce the same effect under different circumstances. The test of an hypothesis or the determination of a quantity are only secondary functions of experiment and are almost completely neglected by Helmholtz.

Regrettably, it seems that Helmholtz never said anything about the role of scientific instruments in the discovery of the hidden causes of physical phenomena according to his scheme. In light of his experimental interactionism, one would have to grant them two functions: *manipulation* of phenomena, and *detection* of effects. The wilful intervention of the instruments into the phenomena serves the same function as the voluntary movements by which we change our perspective of an object. In using a scientific instrument, one produces manipulations in order to discover the conditions under which a certain effect appears. On the other hand, instruments also serve as refined sensory organs which can detect effects of causes which are hidden to our unaided senses. Instruments are, so to speak, a surrogate for an acting as well as a perceiving body.

HELMHOLTZ'S ELECTRODYNAMICS

How do the two elements of Helmholtz's philosophy of science, his hidden-causes realism and his experimental interactionism bear on his electrodynamics?[4] From about the time of his turn towards phenomenalism and empiricism, Helmholtz made increasing use of Franz E. Neumann's (1798–1895) concept of the potential.[5] With this mathematical aid, Neumann and Helmholtz could express the known electromagnetic phenomena and their lawful relations without leaving the realm of direct experiences. They were not forced to put forward an hypothesis on the special nature of the underlying causes. Helmholtz saw the potential as the "immediate expression of experience" (1882, 1:559). According to Neumann's method, the total electrodynamic effect is given as the sum of the elementary effects of the single observable current-elements and no action of unobservable theoretical entities is stipulated. Neumann himself was deeply influenced in this respect by Joseph Fourier (1768–1830). He found it highly advantageous that Fourier's methods of representing the phenomena avoided the assumption of hidden causes.[6] Fourier's derivation of the laws of heat conduction[7] without employing an hypothesis about the nature of heat served as a model for all currents in 19th century positivism. Ernst Mach, for example, considered Fourier's theory of heat conduction a "model theory for physics."[8]

Neumann's, and thereby also Helmholtz's, conception differed markedly from Wilhelm Weber's (1804–1891) approach. By making use of an idea of Gustav Theodor Fechner (1801–1887), Weber had assumed that the electric and magnetic effects are to be reduced to the forces obtaining among unobservable material objects. These objects are the positive and negative "electric masses" or particles of the electric fluid which flows in the electric conductors. Unlike most theories of action at a distance, Weber's asserts that the forces between electric particles depend on the velocities and accelerations with which they move in respect to one another. Helmholtz was convinced that this dependence violated conservation of energy (1882, 1:553, 639; Koenigsberger 1902–03a, 1:295).

In spite of taking over Neumann's phenomenological approach and in spite of criticizing his own earlier metaphysics, Helmholtz retained the basic metaphysical presupposition of his hidden-causes realism, insofar as he meant to reduce all electrodynamic appearances to the action of substances and their attendant forces. The only difference with his earlier view is that the special nature of the forces is not derived in an *a priori* manner but inferred from the appearances. Neumann's potential served for Helmholtz as an ontologically neutral way of stating the lawful relations. It allowed him to neglect *for the time being* the true nature of the elementary electromagnetic effects. Once a comprehensive and general representation of the lawful relations has been reached on the phenomenological level, the problem of causes could be resumed and tackled experimentally. At this point, Helmholtz's view can be mistaken for an extreme phenomenalism or even positivism, but in fact it represents only a preparatory stage on his way to the ultimate goal: the discovery of the true forces that act behind the different appearances.

In order to arrive at a satisfying phenomenological representation of electrodynamics, Helmholtz thought it necessary to begin with a systematic comparison of the different existing theories. For this reason, he generalised Neumann's electrodynamic potential in such a way that the theories of Neumann, Weber and of Faraday-Maxwell could be conceived as special cases. The difference between the theories reduced to the value of a term, k, which indeed became relevant only for open circuits. One must therefore distinguish between Neumann's potential as Neumann himself used it, Neumann's potential in the generalised form of Helmholtz, and finally, Neumann's theory as a limiting case within this generalised frame.

The Helmholtz-Neumann potential treated the propagation of the electric effect as pure, immediate action-at-a-distance. In order also to take into consideration the influence of the dielectric, Helmholtz introduced a second term into his potential, the value of which determines the kind of polarisation of the medium and thereby the velocity of propagation of the potential in the medium. The electrodynamic effects of dielectric displacement currents are thus put in relation to the electrodynamic effects of currents in conductors.

Helmholtz's underlying goal was to reconcile the field conception with action-at-a-distance. To this end he assumed that electric conductors act on each other in two different ways: directly at a distance, and indirectly through the polarisation of the insulating medium between them. The smallest particles of the dielectric are supposed to be polarised by the distance forces of the conducting bodies and they themselves exert a distance force as a result of this polarisation. In Helmholtz's theory we thus have to distinguish two kinds of effects resulting from the reciprocal electrodynamic interaction between two bodies in a vacuum: there is the direct influence they have on each other through action at a distance and the modification of this action through the polarization of individual particles in the medium. Helmholtz was convinced that, in this way, the contiguous action between two neighbouring volume-elements of the ether, as conceived by Faraday and Maxwell, could be expressed in terms of distance forces.

For a long time, Helmholtz favored Neumann's theory as the limiting case of his general potential without accepting the polarising effects of the medium. He had rejected Weber's theory long before since he thought it violated conservation of energy. During 1874 and 1875, from experiments conducted in collaboration with his student N.N. Schiller, Helmholtz found that electric displacement currents have an inducing effect. This case is not considered by Neumann's theory (in its Helmholtzian form). For this reason, Helmholtz gave up Neumann's theory (1882, 1:781f., 787f.) and started to prefer Maxwell's, yet always couched in terms of his general potential. For Helmholtz, accepting Maxwell's theory did not imply acceptance of the notion that Faraday's lines of force or the electromagnetic field represent the underlying cause. Simply that, as he had taken Neumann's theory before, he now considered Maxwell's theory the most adequate formulation of the phenomena, a formulation which is independent of any assumptions concerning hypothetical entities. Thus, the Faraday-Maxwell theory served as the "immediate expression of experience" as Neumann's did before. In his Faraday-speech of 1881, Helmholtz stated that by accepting Faraday's theory, we do not have to commit ourselves to one "of the deeper hypotheses which we can form of the ultimate nature of electricity and magnetism." And he added that he would "try to imitate Faraday" as well as he could "by keeping carefully within the domain of phenomena" (1903a, 2:262; English version 1882, 3:59f.). One should "keep undetermined by theory what has not yet been established by experiment" (1882, 1:546).

At this point, Helmholtz's realism and his experimental interactionism seem to be pushed very much to the background. But, it is obvious that behind this cautious positivism there still lingers a fair share of metaphysical realism. This can be seen by the fact that he continued to insist that only the acting material body, and not the medium, is, in the words of Hertz, "both the seat and the source of force" (EW 22).

After his theoretical decision against Weber's theory and the experimental decision against Neumann's, Helmholtz could now proceed to investigate experimentally and systematically the true nature of the causes behind the appearances on the basis of Maxwell's theory (as always, in terms of the Helmholtz-Neumann potential). Now, one could go beyond the appearances and venture into the realm of underlying forces. As he had formulated in his foundational article on electrodynamics of 1870, experiments had to be found which would "allow one to infer the true law of the distance force by which two current-elements act on each other" (1882, 1:546), i.e., the law which governs the underlying reality.

Helmholtz did not find it easy to understand Faraday's and Maxwell's deeper conceptions. In 1878 he wrote, for example:

> Which system of metaphysics has prepared concepts for the reciprocal effects of magnets and electricity in motion. For the moment, physics still struggles to reduce these effects to clearly determined elementary actions, without having reached a clear result yet. But it already seems that light also is nothing else but another kind of motion of these two agents, and the space-filling ether as a medium which can be magnetised and electrified shows completely new and characteristic features. (1903a, 2:246)[9]

In spite of this glimmer of hope, Helmholtz had to wait a long time for the "clear determination of the elementary actions." Ten years later he still notes that "there is a crisis one first has to go through" before arriving at Faraday's and Maxwell's theory. For this reason he found it more reasonable to begin his lectures of 1888/89 not with Maxwell's theory, but to present electrodynamics in its historical development (1907, 4).

THE ELECTRODYNAMIC WORK OF HEINRICH HERTZ

This is where Heinrich Hertz took over. Helmholtz expected from his student Hertz that he would carry out the decisive experiments for the discovery of the true causes. But instead of determining the forces of the underlying active material bodies in more detail, Hertz's experiments led to the complete abandonment of the concept of a distance force in electrodynamics.[10] The sources of the electrodynamic effect are not anymore to be sought in agents that are hidden from our view but in the surrounding medium. Hertz demonstrated that electrodynamics is incompatible with Helmholtz's hidden-causes realism. In his work on electrodynamics, Hertz gradually liberated himself from these Helmholtzian preconceptions. And in his *Principles of Mechanics* he finally managed to formulate his new philosophical outlook.

From 1886 on, when Hertz increasingly occupied himself with electrodynamics, he quickly became aware that Helmholtz's version of Maxwell's theory leads to a paradox. Helmholtz supposes that the effect of the reciprocal action-at-a-distance of the current elements vanishes to zero and that only the effect of the polarisation of the dielectric remains. But this amounts to the claim that the polarisation is caused by a force without effect, i.e., by something that does not exist! "It is impossible," wrote Hertz, "to deny the existence of distance-forces, and at the same time to regard them as the cause of the polarisations" (EW 25). From the very beginning, he could not help but draw the conclusion that Helmholtz's theory is inappropriate:

> In the special limiting case of Helmholtz's theory which leads to Maxwell's equations [...] the physical basis of Helmholtz's theory disappears, as indeed it always does, as soon as action-at-a-distance is disregarded. (EW 20)

The problem Hertz encountered with Helmholtz's theory is thus of a very special sort. It does not consist in a contradiction between an empirical observation and a theory, nor does it consist in a contradiction among theoretical statements. Instead it consists in a philosophical problem with theoretical terms: what is their meaning, how do they acquire their meaning, how can they be rendered consistent? The problem of theoretical concepts thus imposed itself on Hertz in connection with Helmholtz's concept of the polarization of the ether and Faraday's concept of the field.

As Hertz turned to Maxwell's own representation, the situation did not improve but worsened. He was forced to realise that at different stages in the development of his theory, Maxwell adhered to different views about the nature of electric phenomena which, at the very least, were profoundly unclear if not incompatible with each other:

> Notwithstanding the greatest admiration for Maxwell's mathematical conceptions, I have not always felt quite certain of having grasped the physical significance of his statements. (EW 20)

The lack of clarity in the central theoretical concepts created a *philosophical* problem for Hertz which he had to solve before beginning to experiment on the propagation of electric forces. He had to find an answer to the question: what is Maxwell's theory? Or to put it in a different way: which version of the theory is the true one, Helmholtz's formulation, Maxwell's own version, or a third version still to be developed? An adequate answer presupposes a clear conception of the relation between theory and experience. Hertz's solution goes one crucial step further than did Helmholtz's.

In order to deal with his problem, Hertz came up with a new terminology. He distinguished between a "theory" and its "representation [*Vorstellung*]," or more specifically its "*physical* representation." As alternatives for "representation" he also used "interpretation [*Deutung*]," "physical meaning (conception, basis, significance) [*physikalische Bedeutung*]," "hypothesis [*Auffassung*]" or "intuition [*Anschauung*]." A "*theory*" is a comprehensive expression of the phenomenal regularities, i.e., of the lawful, stable relations between directly observable events. In more modern terms, we would perhaps say that for Hertz, a theory encompasses the phenomenological or factual content of the theoretical laws without referring to any causes of the phenomena. The "*representation*" or "meaning" of a theory, on the other hand, designates the ultimate unobservable agent which produces the phenomena. In this sense, Boyle's gas law would be a "theory" and the idea, that this law is occasioned by moving molecules with certain properties would be its "representation" or "interpretation."

Hertz's distinction between a theory and its representation is not to be confounded with the distinction found in logical empiricist writing between an uninterpreted formal system and its interpretation. Like Helmholtz, Hertz at this point saw reality through the representation of a theory, as constituted of imperceptible objects behind the appearances – the reality which can only be grasped by experimental intervention (cf. Misc 288).

A theory and its representation differs from its "presentation" or "expression [*Darstellung*]," i.e., from the concrete sensual aids and devices which are used for its more or less contingent formulation in a certain historical context and which depend on our arbitrary choice.[11] These aids can make it easier for us to develop a vivid idea of the theory. They form the "gay garment which we use to clothe" nature, the "cut and colour" of which "we can choose as we please." This garment, however, "we should in no wise confuse [with] the simple and homely figure, as it is presented to us by nature" and in the form of which "of our own free will we can make no change whatever" (EW 28).

The situation in electrodynamics was especially difficult for Hertz since he had to take into account not only Helmholtz's and Maxwell's presentations but also his own – as he had put it forward in an article in 1884 (Misc 273–290). There he had reached the conclusion that Helmholtz's presentation of Maxwell's theory, in terms of Neumann's potential, cannot, by itself, describe the electrodynamic forces in a solenoid. One has to introduce additional terms as corrections on the potential. He derived these terms from what he called the 'principle of the unity of force', which he attributed to Ampère. According to this principle, the magnetic forces coming from a magnet are of the same kind as the magnetic forces produced by an electric current. From this Hertz concluded that there ought to be magnetic phenomena which are analogous to electric induction. Hertz called them "magnetic currents." If one takes these magnetic currents into account, an infinite series of correction terms for Helmholtz's presentation becomes necessary. The summation of these terms leads to equations which agree "materially," as Hertz says, with Maxwell's equations. Hertz soon had to learn, however, that the prize for this derivation takes the form of immense complications. This he took to be sufficient reason for dissociating himself from the potential and for preferring Maxwell's system which "offers by far the simplest exposition [*Darstellung*]" (Misc 289).

At this point Hertz was convinced that the difficulties arising in the three presentations of Maxwell's theory, i.e., in Helmholtz's, Maxwell's, and in his own one, could be overcome by disentangling the theory from its various presentations. He would isolate the theory and not consider what would be an adequate representation of the theory and how this representation could be justified. Only in a second independent step, would he then try to develop a representation for the theory and to justify it experimentally. According to Hertz, both Helmholtz and Maxwell (and Weber anyway) made the mistake of utilizing preconceived representations in the presentation of their theories.

Hertz believed that one could find a way out of the difficulties by distinguishing three different levels of electrodynamic theorising: First, the concrete presentations by Helmholtz and Maxwell; second, the shared empirical content of these presentations – that is, the "theory" in its "simple and homely figure" as it is related by Maxwell's equations; third, the physical representation of this theory and its experimental justification. "I [...] endeavored to form for myself in a consistent manner the necessary physical conceptions [*Vorstellungen*], starting from Maxwell's equations [...]" (EW 20). This leap from the (symbolic) *description* of an unknown underlying reality to its complete *construction* from the appearances distinguishes Hertz from his teacher Helmholtz.

Hertz's famous saying "Maxwell's theory is Maxwell's system of equations" (EW 21) must be understood in this context. It does *not* mean, as is often assumed, that physics has to confine itself to the mere description of observations, leaving to philosophers questions of representation. Hertz meant instead that one has to free oneself from interpretations and their justifications if one wants to establish a theory. And only once the theory is established can one dare to develop an appropriate physical representation. Such a representation presupposes a complete and simple presentation of the theory on the phenomenological level. But this does not mean that one can dispense with representations generally. Without its representation, a theory is just a summary of phenomena without being able to contribute to a true understanding of the phenomena and their causes. Physical representations are thus "necessary" for Hertz (EW 20).

How then should we imagine the construction of a physical representation? This is a process which has a theoretical and an experimental side. Theory alone cannot make any progress in this respect. A theory can indicate which representations must be excluded from the beginning, but without experiment, it is impossible to justify the adequacy of any representation. Hertz finds it possible for one theory to be a limiting case of another in the mathematical sense; and still the two theories may be completely distinct in terms of their physical representations (EW 25).[12] According to the received view of logical empiricism, such a case would be excluded, since there would be no room for a representation which goes beyond the level of description. For Hertz the system of Maxwell's equations is, mathematically speaking, a limiting case of Helmholtz's theory. But understood physically, it provides a completely different account.

In order to justify Maxwell's representation, Hertz set about to demonstrate what he thought was the "gist and the special significance of Faraday's, and therefore Maxwell's, view [*Anschauung*]" (EW 7): that empty space (or, in close approximation, the air) behaves like other dielectrics. For this reason, he investigated the effects of open electric circuits and experimentally determined the finite velocity of propagation of electromagnetic waves in air. He believed that these experiments "are fatal to any and all theories which assume that electric force acts across space independently of time. They mark a brilliant victory for Maxwell's theory" (Misc 324). In a good Helmholtzian manner, Hertz was convinced that he had established the superiority of Maxwell's representation by eliminating the known alternatives (compare Helmholtz's example concerning the expansion of mercury).

Hertz could not claim that Maxwell's representations were complete, nor that they were established once and for all. He believed, however, that one could safely infer from his experiments at least that "the electric field exists in space independently of and without reference to the method of its production" (Misc 274). About the nature of this "separate something" Hertz could not say more than what can be said on the basis of Maxwell's theory and the existing experiments:

> The explanation of the nature of the polarisations, of their relations and effects, we defer, or else seek to find out by mechanical hypotheses; but we decline to recognise in the previously-employed electricities and distance-forces a satisfactory explanation of these relations and effects. (EW 25)

Even if the victory of Maxwell's representation is only a victory *faute de mieux*, it enabled Hertz to irrevocably refute traditional prejudiced representations.

It is interesting to note that as late as 1899 someone like Max Planck (1858–1947) saw the matter quite differently. Planck, at that time still rejecting Helmholtz's realism, considered Maxwell's theory superior, not for its better experimental confirmation, but for its greater simplicity:

What has been tried so often is impossible in principle, namely to carry out an *experimentum crucis* in favor of Maxwell's theory and against the older theories. [...] Maxwell's theory excels over the older theories not by its greater correctness but by its greater simplicity, or in other words: in the end, it is nothing else but the principle of economy, in the sense of Mach, which received one of its greatest triumphs in the carrying through of Maxwell's theory of electricity. (Planck 1899, 79f.)

HERTZ'S MECHANICS AND HIS PICTURE THEORY

In his *Principles of Mechanics* Hertz tried to generalise what he had learned in electrodynamics about the relation between theory and experiment. He tried to formulate it in a philosophically satisfying way, and finally to apply it to mechanics. In a certain sense, he wanted to solve the problem he had deferred when he was still involved with electrodynamics (EW 25), namely the problem of clarifying the notion of a representation of physical theory.

In order to solve this problem, Hertz made use of Helmholtz's theory of signs, but he interpreted it in an entirely novel manner. For Helmholtz, sensory experiences are signs of the inaccessible outer reality of substances and forces. For Hertz, in contrast, representations of theories are signs of sensory impressions that are given to us. Only if we use theory to construct representations will it accomplish the most important task of natural knowledge, foresight of the future from experiences of the past.

We are free to choose the signs, as long as they stand for concrete experiences, and as long as they ensure the required foresight. This is how we have to understand the famous passage from the *Principles of Mechanics*:

We form for ourselves images [*innere Scheinbilder*] or symbols [*Symbole*] of external objects; and the form we give them is such that the necessary consequents of the images in thought are always the images of the necessary consequents in nature of the things pictured. (PM 1)

Hertz explicitly pointed out what he meant by "image" or "picture": "The images which we here speak of are our conceptions [*Vorstellungen*] of things" (PM 1). Thus, a picture is just what he meant by a representation in the electrodynamic context. That is, pictures, in his sense, are neither theories nor expressions or presentations of theories – let alone analogies or models (in the sense of Boltzmann, Maxwell, or Kelvin), although this seems to be the standard account.

By his reinterpretation of Helmholtz's theory of signs, Hertz has shown that it is not necessary for physics to suppose a deep and insurmountable rift between the world of our sensations and the objective world of outer reality, as Helmholtz had claimed. The representations constructed from our theories do not refer to an inaccessible world of causes behind the appearances, to "supra-sensible abstractions" (PM 33), as Hertz had still maintained in his electrodynamic work. They refer instead to the appearances themselves. In Hertz's picture theory, it makes sense to talk of a possible correspondence "between nature and our thought" (PM 1), i.e., between our representations and reality. This is why Hertz does not talk of signs only but also of pictures (cf. 1903a, 2:222f.). According to Helmholtz, a sign is different from a picture in that it does not show a similarity with the object signified. If our representations were only signs, as Helmholtz claimed, it would be senseless to speak of a correspondence between our representations and reality or an agreement between mind and nature as Hertz does.

For Hertz, having a representation means to assume "determinate connections between sensory impressions and perceptions" (PM §541). We can examine through experience whether such connections obtain. Pictures we make with the help of our scientific theories do not refer to "entities of a special and peculiar kind"; they are not something that "belongs to a special category" (PM 25) or "something independent of us and apart from us." They are objects of the same kind as sensory experiences. They cannot, therefore, in themselves, "have anything mysterious to us" (PM 28).

All experiences of the outer world appear in the form of time, space, and mass. These forms also limit the kind of representations which sensibly can be constructed from theories. These limitations give rise to Hertz's criticism of the two traditional images of mechanics. They make the mistake of illegitimately using the unclear representation of force or energy in addition to the conceivable representations of space, time, and mass. Hertz's own image of mechanics differs from these pictures in:

> that it only starts with three independent fundamental conceptions [*Grundvorstellungen*], namely, those of time, space and mass. [...] Difficulties have hitherto been encountered in connection with a fourth idea [*Begriff*], such as the idea of force or of energy; this, as an independent fundamental conception, is here avoided. (PM 24f.)

In this third image of mechanics we need, however, a substitute for the theoretical possibilities which arise with the concepts of force or energy in the other images. This substitute is provided by an explanation of force or energy in terms of the motion of concealed masses (cf. PM 25f., 36f., §546ff.). The assumption of such masses is not, however, a relapse into a world of "supra-sensible abstractions." The fact that only the fundamental conceptions of space, time and mass enter into our representations guarantees that all statements about concealed masses "represent possible experiences; if necessary, they could be confirmed by direct experiments, namely, by measurements made with models" (PM 30, cf. 25). Thus, imperceptible reality is constituted of the same entities as the perceptible one. In his *Principles of Mechanics*, Hertz also understood electrodynamic forces and forces of heat as effects of the motion of concealed masses.

At this point the question might arise whether Hertz's picture theory differs at all from a positivist conception which takes theories as economic descriptions of sensations. The difference is marked by the concept of representation. While Hertz does not tolerate metaphysical concepts in physical theories, he does allow theoretical terms a transcendental function; they have a symbolic role to play. We use our concepts as "images [*innere Scheinbilder*]" or "symbols" which are preconditioned by the manner in which our mind represents the world. We can formulate theories which help us understand the world and predict its course, only if we are able to "translate external experience, i.e. concrete sensations and perceptions, into the symbolic language of the images of them [*unseres inneren Bildes*]" (PM §302), i.e., if we present our experience in the form of an adequate representation (cf. PM 2).

CONCLUSION

Overall, Hertz's philosophy of science seems to be a consistent extension of elements already present in Helmholtz's philosophical conceptions. Hertz's contemporaries viewed his philosophy of science from various vantage-points. First of all, they saw the student of Helmholtz who wanted to reduce contiguous action to the motion of concealed masses. Then they saw Hertz as an empiricist opponent of metaphysics who insisted that theoretical concepts can have no *a priori* or metaphysical justification; they have to be based on experience in order to have meaning at all (cf. e.g. Kleinpeter 1899). And then, there is Hertz, the anti-mechanist who did away with distance force in electrodynamics as an old fetish of mechanist physics and provided a "clean and comprehensible picture of a pure electromagnetic field."[13]

Yet, these empiricist characterizations are counterbalanced by Hertz the Neokantian antiempiricist (cf. e.g. H. Cohen 1984, 72ff., Natorp 1899) who claimed that theories represent the world in a way which transcends immediate observation and description:

> If we wish to obtain an image of the universe [*Weltbild*] which shall be well-rounded, complete and conformable to law, we have to presuppose, behind the things which we see, other, invisible things – to imagine confederates concealed beyond the limits of our senses. (PM 25)

And they are counterbalanced also by Hertz's unqualified commitment to the electric field which precluded him from seeing faint beginnings of something like an electron theory in Helmholtz's late work (Buchwald 1985, ch. 27). But, even if these different facets and various philosophical interpretations of Hertz's work may at first appear as a bewildering and eclectic hodgepodge, a closer look reveals Hertz's admirable philosophical analysis casting them in one piece.

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NOTES

[1] Cf. the contribution of Gerd Grasshoff in this volume.
[2] For a more extensive discussion and further bibliographic references, cf. Heidelberger 1993.

[3] As I have shown in Heidelberger 1993, 488–491, the roots for Helmholtz's experimental interactionism can be found in the philosophy of Johann Gottlieb Fichte.
[4] Cf. Buchheim 1971, Buchwald 1993a, Darrigol 1992a and 1993b, Kaiser 1993, Rechenberg 1994, chs. 30, 33, Wise 1990.
[5] "Olesko 1991, 63, 145, 150; and Woldemar Voigt, 'Gedächtnisrede auf Franz Neumann," *Nachrichten der Königlichen Gesellschaft der Wissenschaften zu Göttingen*, Mathematisch-physikalische Klasse, **2** (1895), pp. 248–265, reprinted in *Franz Neumanns Gesammelte Werke*, volume 1, edited by M. Krafft *et al.* (Leipzig: Teubner, 1928), pp. 3–19, cf. pp. 7, 10f., and 14.
[6] Franz Neumann, "Die mathematischen Gesetze der inducirten elektrischen Ströme" (1845) and "Über ein allgemeines Princip der mathematischen Theorie inducirter elektrischer Ströme" (1847) in *Franz Neumanns Gesammelte Werke*, volume 3, edited by Carl Neumann *et al.* (Leipzig: Teubner, 1912), pp. 257–424.
[7] Jean Baptiste Fourier, *Analytische Theorie der Wärme*, edited by B. Weinstein (Berlin: 1884), §432 (first French edition Paris 1822).
[8] Ernst Mach, *Die Prinzipien der Wärmelehre*, (Leipzig: Barth, 1919), p. 115.
[9] Compare Helmholtz's preface to the German edition of John Tyndall, *Faraday und seine Entdeckungen* (Braunschweig: Vieweg, 1870), p. vif.
[10] Cf. Buchwald 1990 and 1994, D'Agostino 1993.
[11] This terminological point is further complicated by the fact that English translations misleadingly render "*Darstellung*" as "representation."
[12] Cf. D'Agostino's distinction between "theory" and "Theory" in his contribution to this volume.
[13] Wise 1990, p. 354.

GREGOR SCHIEMANN

THE LOSS OF WORLD IN THE IMAGE: ORIGIN AND DEVELOPMENT OF THE CONCEPT OF IMAGE IN THE THOUGHT OF HERMANN VON HELMHOLTZ AND HEINRICH HERTZ[1]

In searching for the origins of current conceptions of science in the history of physics, one encounters a remarkable phenomenon. A typical view today is that theoretical knowledge-claims have only relativized validity. Historically, however, this thesis was supported by proponents of a conception of nature that today is far from typical, a mechanistic conception within which natural phenomena were to be explained by the action of mechanically moved matter.

Two of these proponents, Hermann von Helmholtz and his pupil Heinrich Hertz, contributed significantly to the modernization of the conception of science. Paradigmatic for their common contribution to this development is the way in which they employed the concept of *image*. By considering the origin and the different meanings of this concept we may trace a line of development which begins with Helmholtz's original claim that a universally and forever valid theory provides a unique representation of nature. It continues with the realization that the status of scientific knowledge is capable of revision; and it arrives at Hertz's admission that a variety of theories over a domain of objects is possible, at least at times.[2]

1. PICTORIAL ASPECTS OF THE SIGN
ELEMENTS OF HELMHOLTZ'S CONCEPTION OF SCIENCE UNTIL ABOUT 1870

Throughout his life Helmholtz stood for an empiristic conception of science. That meant that science should derive its knowledge by the generalization of specific experience through the method of induction. On this basis Helmholtz began in the 1860s to characterize the laws known through natural science by the concept of *image* [*Bild*].[3]

Before then, Helmholtz had trusted that scientific theories could do much more than merely provide an image of the world. According to his early scientific and popular lectures, theoretical natural science did not merely comprehend empirical lawful regularities, but also discovered the substantial causes of the appearances, which are, according to Helmholtz, completely determined mechanically (1889, 4f.; 1882, 2:608f.; 1903a, 1:40f. and 45). The concept of the image always implies a separation of the represented from the representation.[4] On Helmholtz's early views, however, scientific theories penetrate their objects, so to speak, exploring their inner structure. Like probes they yield glimpses of hitherto unseen worlds, and they are therefore true in the objective sense (1889, 7; 1903a, 1:41).

D. Baird et al. (eds.), Heinrich Hertz: Classical Physicist, Modern Philosopher, 25–38.

Helmholtz initially placed this objectivity in strict opposition to the merely subjective testimony of sensory perception in the life-world; this perception does not have any immediate access to reality but consists in the psychological processing of sensations. The peculiarity of the sensations is determined by the constitution of the sense organs. Their specific manner of excitation is triggered only by external stimuli. Because the sensations do not bear any resemblance to these stimuli, he labeled them and the perceptions which they trigger "signs" or also "symbols" (1882, 2:608; 1903a, 1:41ff.).[5]

By using the expression "sign," Helmholtz points to a particular analogy: he compares the sensations with the characters of written script. In doing so he suggests that the internal sensations resemble the external world as little as, for instance, the name of a person resembles the person itself (1882, 2:608 et passim). But the analogy has its limits. While a name can designate various persons or objects, the signs of sensation satisfy a one-to-one correspondence, which I will call *sign-constancy*: to one sign of sensation should always correspond at most one thing (1882, 2:608; cf. 1903a, 1:41ff.).[6]

The contrast between the sign-character of perceptions and a scientific cognition of reality is founded on Helmholtz's understanding of causality. According to him scientific statements have the same causal structure as the real happenings in nature. In contrast, the psychological process producing the sensory perceptions is irreducibly determined by acausal elements. Helmholtz envisions a changeable, open-ended learning process that is not free of errors (e.g., hallucinations). In it, an autonomous subject steps into the relationship between sign and signified in a constitutive manner, creating what may be called a triadic relation. Whether a sign has been understood correctly can be judged only in relation to its successful application. Therefore the subject needs to be accorded a certain scope of action which Helmholtz equates with freedom of the will (1903a, 1:114; 1856, 427ff. and 797f.). In contrast to Kant, Helmholtz supposes that those phenomena determined by freedom of the will cannot be completely explained causally (1882, 1:13).[7]

Sensory perception can therefore become an object of science only partially, to the extent that it agrees with the law of causality. Thus, in order to penetrate into reality science must probe, in each individual case, whether what the senses declare to be similar or different is in fact similar or different (1903a, 1:40). The sensations that are expressed in script fail to contain objective truth whenever they fail to be causally connected.

With the introduction of the concept of the image, Helmholtz signals a departure from his critical view of the truth-content of signs of sensation. In 1856, in the first edition of his *Handbook of Physiological Optics*, he partially suspends his previous strict opposition of objective and subjective knowledge in order to ground the truth-claims of scientific statements. Going beyond the afore-mentioned sign-constancy, he now postulates the temporal congruence of sign and signified in a theory of perception:

> The only relation in which there can be real agreement between our perceptions and reality is the temporal sequence of events with its various peculiarities. Simultaneity, succession, the regular recurrence of simultaneity and succession can happen in our sensations just as well as in the events. (1856, 445)

In addition to sign-constancy, a second non-causal congruence of sign and signified is herewith established. While the first concerns the relation of sign to object, the latter relates the temporal structure of the signs to that of the object's properties. Through this congruence in temporal sequence, the signs change their character significantly. They break the scientific monopoly on truth and can give information about "the true essence of things," as Helmholtz says now (1856, 446). In order to denote the sign's more inclusive relation to reality, Helmholtz speaks for the first time in his *Handbook* about "images":

Thus the representations of the external world are images of the lawlike succession of natural events. (1856, 446)

Helmholtz here uses the concept of image in the sense of a strict representation [*Abbild*]. The temporal constitution of the presupposed causal structure of the world is reproduced without distortion in sensory perception. Originating in the theory of perception, this concept of the image soon serves to provide a new determination of the task of science. The object of science is now to "discover and combine into a law" the temporal structure which is inserted into perception (1903a, 1:319f.). Scientific knowledge, insofar as it consists in statements of causal law, becomes the pure presentation of the pictorial component of sensory perception. From now on Helmholtz will emphasize that natural laws have the character of a representation of "natural phenomena ... with respect to succession in time" (1903a, 1:395; cf. 1903a, 2:222f. and 358; 1885, 586). As science's relation to reality becomes restricted to the perceptible, science loses its unrestricted access to reality. Because laws can only picture the non-intuitive and mathematical relations between properties of objects, natural science can no longer claim to reach the objects themselves, the substantial reasons for the phenomena.

While he uses the expression "sign" throughout for a characterization of sensory perception and sensation, Helmholtz's use of the concept of the image fluctuates. Most of the time he interprets it in the sense just elucidated, namely as strict representation, but occasionally also in the sense of a sign.[8] When he tries in some passages of his public speeches to illustrate the relation between sign and image by drawing on the example of the arts, one has the impression that he places the concept of image in a third, more comprehensive meaning above the concept of sign.[9] In this interpretation, the concept of image is akin to the concept of a work of art. It has a content which extends beyond the relationship of equality or similarity, and this content belongs only to the image but not to its object. It is subject to all kinds of intentional shaping and is therefore meaningless for science.

2. SIGN-ASPECTS OF THE IMAGE
ELEMENTS OF HELMHOLTZ'S CONCEPTION OF SCIENCE AFTER ABOUT 1870

Until roughly the end of the sixties Helmholtz endeavoured to justify the truth-claim of scientific knowledge. During the seventies, there occurs a change in the development of Helmholtz's philosophy of science which points in a completely different direction: on the basis of his theory of perception he begins to relativize the claim to validity which hitherto he had held absolute. The conceptual distinction between sign and strict representation becomes less and less marked. The truth-conditions for signs, which always depend on some success of an action, begin to hold more and more for the scientifically established representations of reality as well. The psychological processes determining the creation of signs become elementary conditions of cognition which in principle can no longer be transcended by scientific cognition.

The possible background and motives for this profound change are various; Helmholtz never explicitly addressed them. Among the most important ones, I wish to mention these: Helmholtz followed a general trend in the natural sciences during the second half of the nineteenth century towards an increasing hypothesization of scientific propositions; this change of his conception of science is related to a crisis of his mechanistic conception of nature, which was to have been a representation of the first causes of nature; and finally this change must be understood as part of the extension of his theory of perception towards a comprehensive theory of knowledge.[10]

In his second speech of 1892 about Goethe's scientific work, Helmholtz found the "final result" of his epistemology summarized in the sentence by Goethe, "All things transitory are only symbols [*Gleichnis*]." To this Helmholtz adds:

> That is, what occurs in time and what we perceive through the senses, we know only in symbols. I hardly know a more pregnant way to express the final result of our physiological theory of knowledge. [...] All knowledge of the laws of nature is inductive, and no induction is ever totally complete. We feel [...] our inability to penetrate further [into nature] as a kind of anxiety. (1903a, 2:358)

The nature of "transitory things" is still considered to consist in their temporal causal structure; this is represented in perception, the sign-character of which Helmholtz here refers to as "symbol" [*Gleichnis*]. Until roughly the end of the sixties, Helmholtz assumed that this structure is expressed in the experimentally and inductively established laws of nature and that it can be entirely reduced to mechanical laws. But now it is no longer possible to complete this reduction if the process of induction cannot be completed, i.e., if there is a remnant of the true causal structure or its representation in perception which eludes science. The content of perception is now richer than the laws known by science, which always remains incomplete in regard to reality. But the knowledge of laws neither changes its causal structure nor its formal mode of presentation (1882, 2:640ff., 3:176). What are subjected to change are its conditions of validity. The relationship of representation can be assumed only as an idealized relationship and can be ascertained only approximately (1903a, 2:243, 393, and 183f.; 1882, 2:642).

When comparing Helmholtz's concept of image with that of Hertz, it is important to realize that Helmholtz expands his original conception of signs, which is rooted in a theory of perception, into a naturalistic theory of knowledge. This process finds its most pointed expression in a new determination of thought, which Helmholtz had originally viewed as a high court of cognition that would ensure agreement with reality.[11] But already towards the end of the seventies he was convinced that in principle thinking is not free of the sign-character.

> ...[with this psychological processing of cognition] we are obviously dealing with an elementary process that lies at the bottom of all really so-called thinking, even if the critical review and completion of the particular steps may be missing here, a critical review and completion that enters the scientific formation of concepts and deductions. (1903a, 2:233)

Now all that the scientific formation of concepts and logical deductions from statements of law can accomplish is already predetermined in "every particular step" by the psychology of perception. Under these conditions, no autonomy free of experience adheres to thought (which will be Hertz's point of departure). Thought cannot be an independent court for validity but is part of the domain of an empirical science which can only approximate the ideal of truth.[12]

The aspects discussed so far concern that side of the representation that is inserted into sensory perception and that can be presented as law. However, the mode of existence of the represented, of causally structured reality, also became increasingly questionable for Helmholtz. Well into the seventies he had made the realistic assumption of a reality independent of cognition. In a central passage of his 1878 speech "The Facts of Perception," however, Helmholtz relativizes his realism. He recognizes idealism as an equal and irrefutable epistemological alternative and refers to both realism and idealism as "metaphysical hypotheses" (1903a, 2:238f.).

Briefly, the change in the determination of representation and represented is that Helmholtz becomes increasingly uncertain about the supposed congruence between them. The scientific representation of the world loses its indubitable reference to the world and diminishes in permanence and sharpness. One can also consider this as the outcome of a subordination of thought to the conditions of relating to experience, a relationship to which Helmholtz, in contrast to his early conception of science, now accords only approximative validity.

3. IMAGE-MULTIPLICITY OF SIGNS
HERTZ'S PHILOSOPHY OF SCIENCE IN THE *PRINCIPLES OF MECHANICS* (1894)

Just like Helmholtz, Hertz uses the concept of image to point to the only agreements that can exist between the external world and one of its representations. From sign-constancy and from the simultaneity of sign and signified Helmholtz had derived the claim that all scientific knowledge of laws has the character of a representation. In Hertz's philosophy of science there is also talk of representations. To him, scientific theories are "images" which merely satisfy a "first fundamental requirement" (PM 2) in relation to the external world: in the "necessary consequents of the images in thought" they can agree with the "necessary consequents in nature of the things pictured" (PM 1).[13]

Hertz does not give any criterion for the representation of objects, however, besides the congruence of "consequents." He restricts the relation between the presentation and the presented to the predictions which can be deduced from a theory and tested by experience. Neither the content of a theory, nor its principles, concepts and laws, but only its results can still be linked to the external world. Contrary to Helmholtz, Hertz did not advocate an inductivist conception of science, but a deductivist one.

This additional step towards a loss of truth in theoretical cognition is reflected in the change of meaning of the concept of "image." In contrast to Helmholtz, whose representations concerned merely the temporal structure of reality, Hertz's concept of image postulates elements of theory which have no cognizable connection to what they present and which for Helmholtz would have merely been "signs" or "symbols." To one reality, which Hertz, too, conceives realistically, can now correspond a multiplicity of theories. The world seems remote and the concept of representation inappropriate. If Hertz uses it anyway, this expresses his hope that the gap between presentation and what is presented may only be transitory, and his hope that the surmised mechanical cause of all phenomena can yet be found. Closer scrutiny reveals how this mechanistic objective, which he shares with Helmholtz, influences the determination of his concept of image. It also reveals how this shared objective does not preclude a modernization of the concept of science that goes further than Helmholtz's.

In an article on the seventieth birthday of Helmholtz, Hertz mentions as a third "title to fame" besides the invention of the ophthalmoscope and the discovery of the Principle of the Conservation of Force, Helmholtz's work on the physiology of the senses, and emphasizes "how closely these investigations are connected with the possibility and legitimacy of all natural knowledge" (Misc 336f.). Although he never explicitly refers to Helmholtz's theory of signs or to his concept of image, it can be assumed that Hertz was aware of both and recognized their significance for philosophy of science.

Tellingly, Hertz goes on in his article to present Helmholtz's theory of perception in a manner best suited to its early stage of development. He believes that he finds support in Helmholtz for his own view of sensory sensation as a passive mediator between two entirely separate worlds. He does not at all consider the psychological mechanism involved in the processing of sensations as that elementary process of which Helmholtz later said that it "lies at the bottom of all really so-called thinking."[14] In his article, Hertz poses rather schematically the following question:

> Is the manifold of these relations [mental conceptions formed by the visual sense] sufficient to portray all conceivable manifolds of the external world, to justify all manifolds of the internal world? (Misc 336)

Three years later Hertz provides the answer in the *Principles of Mechanics*, the introduction to which can be considered his contribution to the philosophy of science. There he says that a "universe conformable to law" cannot simply result from perceptions that are triggered by sensations (PM 25). This already contains both the essential contrast with Helmholtz's conception of representation and the point of origin for Hertz's multiplicity of images. While the mind may recognize certain regularities in perception, it cannot derive from them a complex of laws that encompasses the external world. Hertz relates this to representations in the life-world which proceed from immediate sensory perceptions, and he relates it equally to scientific knowledge. In their relation to the world, both life-world representation and scientific knowledge satisfy only the "first fundamental requirement." Only their necessary consequents correspond with nature. Therefore, Hertz designates both as "images."[15]

Scientific knowledge differs from representations in the life-world only in that science requires possible criteria for the evaluation of images to be formulated explicitly.[16] The difference between the two had already been continually diminished by Helmholtz. It now appears to be only a matter of degree. This impression is strengthened by the fact that one does not find in Hertz a distinction comparable to Helmholtz's persistently upheld division between sign and representation. In Hertz's work, the word "sign" generally represents the views, expressions and connections that are contained in images, be they images of the life-world or of science (PM 7, §297).

But it would be a mistake to assume that Hertz equates, in their pictorial aspect, scientific theories and representations in the life-world. A first, though hardly perspicuous clue is provided by Hertz himself when his first mention of the unrestricted possibility of representing one object by means of "various images" is made only in regard to representations in the life-world (PM 2, §297).

Why does this possibility exist and to what extent does it obtain in science too? Hertz first addresses the former question: representations are "not yet uniquely determined" by the agreement of consequents necessary in thought and consequents necessary in nature (PM 2). The supposition of an autonomous mental capacity (shared by all humans) is implicitly involved here. This capacity need not stand in any relationship to real objects or to its properties. It does not, by self-imposed prescriptions deprive itself of multifarious possibilities of representation. This supposition would have been unthinkable within Helmholtz's later conception of science. Can Hertz's supposed freedom of mind unfold in science? Or does it face restrictions which ultimately lead back to Helmholtz's injunction to create a uniquely valid theory?

The three famous criteria and their elucidations used by Hertz to evaluate the images of science embody the encoded answer to these questions. While the first two criteria, which I wish to call liberal-rational, permit a multiplicity of images, the third establishes a rather conservative order among the possible images of a domain of objects.

The first criterion of "permissibility" formulates a minimal condition on the form of images: images may not "implicitly contradict the laws of our thought [and] shall be logically permissible" (PM 2). Hertz accords greatest significance to this criterion (PM 33f.). However, he is rather reluctant to specify more precisely what he means by "laws of thought." He basically rests content with the broad statement that "the nature of our mind" can be decided upon "with validity for all time" (PM 3). Whatever these properties might be, once recognized or established they are equally valid for all images.

Not only do Hertz's remarks on the further determinations of the laws of thought remain vague overall, but they are also not free of contradictions, thus violating the criterion itself. For example, one can learn from the introduction and the main body of the *Principles of Mechanics* that he wishes to prescribe more than the laws of propositional logic to constrain the freedom of mind in science. In the first part of the main body he claims that he develops his groundwork of mechanics (his "image" of mechanics) exclusively by means of propositions that are "*a priori* judgements in Kant's sense" (PM §1). However, he adheres to this statement only with his introduction of the concept of time. As soon as he comes to the concept of space, he no longer cares about the difference between synthetic and analytic judgements.[17]

Even when unsatisfied, the claim to a transcendental philosophy yields the necessity that images that fail to satisfy Kant's conditions of the possibility of experience are impermissible. But it seems that Hertz considers the a priori character of an image rather as a peculiarity of that particular image.[18] This is all the more strange since he applies the criterion of permissibility also to the totality of the multiplicity of images:

> In order that an image of certain external things may [...] be permissible, not only must its characteristics be consistent amongst themselves, but they must not contradict the characteristics of other images already established in our knowledge. (PM 22f.)

If one disregards his perhaps merely verbal commitment to a justification of science along the lines of transcendental philosophy, what remains as the most important minimal condition on the form of the images is the demand for freedom from contradictions. The certainty that those sequences of thought at a remove from the world can be in contact with nature at all may be called the Platonic element of Hertz's conception of science.[19] The second criterion shows now that this contact must be highly constrained and that ample scope therefore remains for theories in spite of the logic prescribed to them.

The second criterion of "correctness" imposes a minimal constraint on the content of permissible images:

> We shall denote as incorrect any permissible images, if their essential relations contradict the relations of external things, i.e. if they do not satisfy our first fundamental requirement. (PM 2)

This criterion restricts the agreement of consequents necessary in thought and consequents necessary in nature ("first fundamental requirement") to "essential relations." "Essential" in this context are exactly those successions which, for whatever reason, claim to be empirically verifiable. For correctness is "perfect," he says, when:

> all those characteristics of our image, which claim to represent observable relations of things, do really and correctly correspond to them. (PM 9)

But this perfection need not be permanent. Much more radically than Helmholtz, Hertz assumes that all empirical knowledge is capable of revision. According to Helmholtz's later conception of science, empirical statements served as an only approximately valid but yet increasingly better confirmed basis of validity for theoretical knowledge (Helmholtz 1903a, 2:22, 186, 233). In contrast, Hertz remarks:

> that which derives from experience can again be annulled by experience. (PM 9)

In contrast to its permissibility, the correctness of a theory cannot be decided "for all time." Thus the agreement of consequents necessary in thought and consequents necessary in nature is deprived of any absolute claim to validity. It is questionable in this context why Hertz also considers incorrect theories (as well as incorrect representations) as images. Why should they be images, if they stand in contradiction to the world? In contrast to Wittgenstein's conception, their logical structure is by no means in itself an image of the world.

Since images do not consist of essential relations only, they can be idle and lead to consequents "superfluous or empty" (PM 2).[20] In spite of this description Hertz does not believe it is possible to do without them. Though he includes them among those elements of the image which one "can arbitrarily add or take away," he considers them as an inescapable consequence of the mental origin and character of images (PM 3).

The image of mechanics that Hertz presents as his own serves as the best example that the choice of which statements should be released for empirical verification and which should not is to some degree arbitrary. For the purpose of a mechanistic explanation of the inanimate world, he introduces a new type of inert mass, and postulates that one of its properties is to be unobservable (PM 25f.).[21] In stressing in this and other passages that those "hidden" [*verborgen*] masses are invisible only to the naked eye, Hertz leaves open the possibility of verifying their properties indirectly through physical measurement. Some of these properties are solid connections [*starre Verbindungen*] between masses which provide for constant distance and for "approximately ... invariable relative accelerations between the masses" (PM 41). At the end of his introduction he writes: "Now, if we could perceive natural motions with sufficient accuracy, we should at once know whether in them the relative accelerations ... are only approximately invariable" (PM 41). Here Hertz even speaks of a "decisive battle" [*Entscheidungskampf*], which has to be "fought out" [*ausgefochten*] against other thinkable explanations like those which do not assume hidden masses (PM 41).

First of all Hertz assumed hidden masses only for the purpose of explanation. But if it were possible to verify these hidden masses empirically then, as matters stand, the respective theoretical statements would attain the character of necessary consequents. In this respect, however, Hertz expressed reservations. His remarks did not in principle exclude the possibility that hidden masses could be the subject of experience.[22] Nonetheless, the preoccupation with hidden masses which continued in physics for some time after Hertz died was governed by the continuing hope that further clues to the nature of these masses might be obtained through a more precise examination of electrodynamic phenomena, in particular those relating to the so-called ether.[23]

The criterion is thus primarily directed against incorrect relations that are contained in theories and cannot be converted by definition into inessential ones.[24] One can see that the criterion does not introduce a serious restriction on the multiplicity of theories. It is rather an encouragement to shield statements which disagree with experience from an empirical test. Had Hertz left it at these first two criteria, he would have closely anticipated a currently widespread liberal attitude towards philosophical evaluation of scientific theories.

The characteristic feature of merely permissible and correct images is that none of them can claim to come closer to its objects than any other. They are equivalent representations of objects. If the domain of objects encompasses all of reality, or – if you will – the truth, and if the only access to this reality consists in equivalent presentations, then the concept of the image itself comes to an end, together with the realistic conception of a reality that exists independently of images. It no longer makes sense to talk about a relation if one of its two components, namely the external world, has completely collapsed into the other.

The full significance of the far-reaching change introduced with Hertz's third criterion of "appropriateness" becomes clear only against the background of this scenario. With this criterion Hertz drastically restricts the conditions under which multiple theories become possible. He subordinates them to a process of adaptation and selection which maximizes the predictive scope and empirical content of theories. The multiplicity of theories is considered not as a permanent state but as a state of beginning or transition, in a development which is directed at the minimization of equivalent presentations. Along with Helmholtz, Hertz assumes that this development approximates the goal of a (mechanical) theory which alone is valid in its time.

Hertz uses "distinctness" to refer to the maximization of predictive scope:

> Of two images of the same object that is the more appropriate which pictures more of the essential relations of the object, – the one which we may call the more distinct. (PM 2)

As long as other objects are disregarded, it is characteristic for Hertz's concept of object that it corresponds rather well to a consilience of a variety of predictions all related to one object within a single image. It corresponds rather less well to the occurrence of such predictions across various images. This theoretical call for unification holds not only for special domains of objects in natural science, but for the totality of natural phenomena in general, at least in the inanimate world:

> We should remember that [when discussing appropriateness] we are considering the whole range of present physical knowledge. (PM 10)[25]

But while the mind has to strive towards a unified image of nature, it can bring one about in a variety of ways. That is why it is possible to start from different sets of principles in the derivation of predictions. Beyond this, any number of "inessential" or "empty" statements are permitted. The maximization of empirical content is directed against this last rest of a superfluous content of images. Hertz refers to this criterion as "simplicity":

> Of two images of equal distinctness the more appropriate is the one which contains, in addition to the essential characteristics, the smaller number of superfluous or empty relations, – the simpler of the two. (PM 2)

Hertz is convinced that in the course of time we can "finally succeed in obtaining the most appropriate" images (PM 3). If this formulation already suggests the substitution of the multiplicity of images by a single image of reality, this is indeed what Hertz considered possible. About his own proposal for an image of mechanics he says:

> Whether the presentation here given to this problem is the only possible one, or whether there are other and perhaps better possible ones, remains to be seen. (PM xviii)

For Hertz, it is certain that the most appropriate image, if it is possible at all, can only be a mechanical one. The highly complicated image proposed by himself would become significantly simpler if it turned out that all empty consequents proved to be essential. Implicitly, he assumes an agreement between his image and a mechanical structure hidden behind the phenomena.

The criterion of appropriateness restricts the multiplicity of permissible and correct images to such an extent that it relieves them of their relativized equal standing. But this criterion also leaves the images that remain as a kind of knowledge that is capable of revision and that may, if only for a while, grasp in a simpler manner a world that is forever separated from mind.

4. CONCLUSION

Compared to Helmholtz, Hertz departed more clearly from the aim of a complete (mechanistic) explanation of nature, which is still recognized by both as the ideal of cognition. While Helmholtz excluded as a matter of principle the justified coexistence of several theories over a domain of phenomena, the whole objective of Hertz's philosophy of science is precisely to justify this coexistence, at least for the current state of inquiry.

In regard to reality, which both had postulated in a realist manner, there occurred a far-reaching loss of truth that began with Helmholtz and continued with Hertz's philosophy of science. Initially, theories were not images of the world, because they themselves invaded their objects and thus came into possession of the truth. With the introduction of the concept of image, scientific theories become distant from the world: they are merely representations of a lawful structure, of the causal relations between real objects. (Helmholtz is much closer to Wittgenstein's later image-theory of meaning than is Hertz.) What appeared to Helmholtz as an obvious consequence of this knowledge (the prediction of future phenomena) becomes for Hertz the remnant of what truth natural science can know about the world. The structure of this knowledge need no longer be determinate, as it was for Helmholtz; different images of a domain of phenomena, which can include the whole (inanimate) world, are now possible and can mutually relativize their validity.

If one takes as a benchmark, not the tradition preceding both physicists, but the subsequent development of the conception of science, Helmholtz appears, roughly speaking, to stand closer to the present in one respect. He proposes much more forcefully than Hertz the now broadly accepted removal of the distinction between a priori presupposed laws of thought and those empirical propositions that are capable of revision. With him, thinking loses its function of safeguarding assertions and becomes subject to the uncertain conditions of experience. Against this loss of validity, Helmholtz places a non-negotiable set of assumptions concerning reality which will legitimate the representational character of laws. With Hertz the situation is inverted. While he acknowledges no absolute support in reality for claims to validity, he takes the laws of pure thought, though no longer sharply determined, to be absolutely valid, and he sees in them a unified point of reference that effectively limits the multiplicity of images.

These tendencies towards relativitized claims to validity, which face and complement each other in the relationship between Helmholtz and Hertz, are united in the subsequent development of the philosophy of science. Just as thought could no longer be kept distinct from experience, so it proved impossible to secure experience independently of arbitrarily fixed theoretical presuppositions.

By focusing on the concept of image, I have addressed an aspect of Helmholtz's and Hertz's thought which, though it is of great importance for their respective philosophies of science, is only of limited significance for their work as a whole. The fact that, in terms of their respective claims of validity of scientific knowledge, Helmholtz and Hertz both appear to be in a single line of development, is due to their congenial approach. How close they were would be more apparent if one considered the relationships that existed between their respective philosophies of science and specific work in their fields of interest.[26] (The fact for example, that Hertz could directly refer to Helmholtz's work with his concept of hidden masses.)

But the philosophy of science has to go beyond the results of specific scientific inquiries and be understandable without reference to their respective contexts. The contrasts between the two scientists are revealed by the independent uses they made of the concept of image within their philosophies of science. There are basic differences between Helmholtz's inductivist and Hertz's deductivist conception of science, between the multiplicity of theories excluded by Helmholtz and permitted by Hertz, between the content of the reality referred to by mechanical principles and laws of nature on Helmholtz's account and the emptiness of the reality referred to by scientific theories on Hertz's, and finally between Helmholtz's view that experience is capable of producing knowledge and Hertz's insistence that experience can annull it.

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NOTES

1 I would like to thank Alexander Goroncy and Alfred Nordmann for translating my text.

2 The origin and first development of this concept is predominantly documented by several lectures in which Helmholtz talks about the tasks and methods of science (1889 and 1903a, including the lecture about Goethe's science), also in both editions of his *Handbook of Physiological Optics* (1856 and 1885). Hertz presented his view of the concept of image in the famous introduction to his *Principles of Mechanics*. For a comparison between Helmholtz's and Hertz's concept of image see Majer 1985.

3 For Helmholtz's conception of science see Cahan 1993b and Schiemann 1997, Chap. B.II.3 and Chap.B.III. In Helmholtz's view induction is a method of inferring general laws from particular experience. It is the foundation for the discovery and the justification of natural laws (cf. 1903a, 1:169ff., 2:338ff.; 1856, 447f.).

4 For the concept of image in German philosophy in the 19th century see Schlüter, D. and W. Hogrebe, "Bild," in J. Ritter and K. Gründer (eds.), *Historisches Wörterbuch der Philosophie* (Darmstadt: Wissenschaftliche Buchgesellschaft, 1971).

5 For Helmholtz's theory of perception, in which he develops the concept of sign see Hatfield 1990, Steven Turner, *In the Eye's Mind: Vision and the Helmholtz-Hering Controversy* (Princeton: University Press, 1994), Theo C. Meyering, *Historical Roots of Cognitive Science* (Dordrecht: Kluwer, 1989), and Schiemann 1997, Chap.B.II.3a.

6 I take the expression *sign-constancy* from Meijering *op. cit.* (note 5).

7 Among these he includes with certainty the phenomena of the human and social sciences (1903a, 1:171), with reservations he includes some phenomena of the inanimate world (1856, 454), and to a certain degree he finally includes sensual perceptions. Helmholtz's understanding of causality reflects an empiricist position that is basically different from Kant's idealistic position.

8 In the sense of representation: 1856, 446; 1903a, 2:222 ("For of the image one demands some sort of similarity with the depicted object") and 358. In the sense of sign: 1903, 2:222 ("Images of the things delivered to us by the senses"); 1885, 590 and 599 ("the totality of perspectival images").

9 "An *image* must be *similar* in some respect to an object. A statue, for example, has the same bodily form as the human being after which it is modeled; a painting has the same color and perspective projection. For a *sign*, it is sufficient that it appear whenever that which it signifies makes an appearance, the correspondence between them being restricted to their appearing simultaneously." (1903a, 1:393; similarly, though without mentioning simultaneity, in 1903a, 2:222)

10 Changes in Helmholtz's conception of science have often been discussed, see e.g. Benno Erdmann, *Die philosophischen Grundlagen von Hermann von Helmholtz' Wahrnehmungstheorie* (Berlin: Abhandlungen der Preussischen Akademie, philosophisch-historische Klasse, 1921); Hörz and Wollgast 1971; König 1968; Buchwald 1994b; Gary Hatfield, "Helmholtz and Classicism: The Science of Aesthetics and the Aesthetics of Science" in Cahan 1933a, pp. 552–558; Heidelberger 1994; and Schiemann 1994 and 1977, Part B. For the increasing hypothesization of scientific, propositions in the nineteenth century, see Diemer 1968, and Herbert Schnädelbach, *Philosophie in Deutschland 1831–1933* (Frankfurt: Suhrkamp, 1983).

11 Helmholtz's view of thought as a high court is expressed not only by his position on causality but also by his views on logic and mathematics in Helmholtz 1903a, 1:175f.

12 Helmholtz already believed in 1868 that his work on the physiology of the senses had intervened for the first time "into the hitherto inaccessible field of mental processes" (1903a, 1:268).

13 For Hertz's concept of image, see D'Agostino 1990, and Majer 1985.

14 Cf. pp. 28f.

15 Hertz also speaks of "symbols" and, in agreement with his realism, about "virtual" or "seeming images [*Scheinbilder*]" (PM 1).

16 As a part of this, the descriptions used in the images and their possible reference to experience need to be rendered distinct (PM 2f.).

17 "The space [...] is therefore the space of Euclid's geometry, with all the properties which this geometry ascribes to it. It is immaterial to us whether these properties are regarded as being given by the laws of our internal intuition, or as consequences of thought which necessarily follow from arbitrary definitions." (PM §2).

[18] Nowhere in his very detailed critiques of the other images of mechanics does he mention that they do not satisfy the principles of transcendental philosophy (PM 4ff.).
[19] This interpretation is directed against the supposition that Kantian philosophy played an important role in Hertz's thinking. See, for example, Kuczera 1983, D'Agostino 1990, Hacker 1986. Cf. note 7.
[20] Hertz applies the term 'hypotheses' to these "inessential" relations (PM 25f.).
[21] Therefore, this is an inessential relation (cf. PM 39f.)
[22] Hertz's uncertainty on the epistemological status of hidden masses is stressed by D'Agostino 1990, p. 60.
[23] For Hertz's ether theory and its influence in German physics, see Breunig 1988 and Grigorjan and Polak 1964.
[24] A theory would thus be incorrect if one of its statements did not agree with the Principle of Conservation of Energy, but could still be related to experience.
[25] For the restriction to the inanimate, see PM 38.
[26] Cf. Mulligan 1987, Buchwald 1994a, and D'Agostino 1971.

JOHN H. BRYANT

HEINRICH HERTZ'S EXPERIMENTS AND EXPERIMENTAL APPARATUS: HIS DISCOVERY OF RADIO WAVES AND HIS DELINEATION OF THEIR PROPERTIES[1]

I. OVERVIEW

At the Heinrich Hertz Conference at the University of South Carolina in March 1994, I gave demonstrations of several replicas of Hertz's experimental apparatus; described my search for and discovery of three identifying photographs, and a set of exact replicas; and gave some of my interpretations of Hertz's work in electromagnetics.

In this paper, I identify Hertz's experimental apparatus in detail through the photographs; they are cross-referenced to each other and cross-referenced to items in the Appendix. This Appendix, in four parts, lists items of Hertz's original apparatus received at various times at the Deutsches Museum, Munich, Germany. Explanatory notes are in parentheses.

Through the photographs, I also identify a number of items of laboratory equipment used by Hertz. I discuss the existence and location of Hertz's original apparatus, and the various sets of replicas from his electromagnetics investigations. For Hertz's experimental work prior to 1886, there are no known surviving items.

I discuss some of the major steps in Hertz's investigations leading to the start of his successful 1886 experiments: the faculty prize problem at Berlin in 1878; his study of the "Berlin Prize" problem in 1879; and his analytical paper of 1884 (see also Tai and Bryant 1994).

I describe the design and mechanism of operation of Hertz's initial 1886 apparatus which uses distributed, open circuits. Hertz proceeded in a step-by-step learning process, alternating experimental and analytical work to delineate properties of electromagnetic waves over a substantial portion of the radio frequency part of the spectrum. He discovered the surface photoelectric effect and experimented in the ultraviolet, "at the outermost limits of the known spectrum." I note that, for reasons well understood today, Hertz was not successful in one area – measuring the velocity of propagation of electromagnetic waves in air. As a pioneer who accomplished so much, Hertz needs no excuse for not understanding clearly that his arrangements did not satisfy boundary conditions, and therefore could not yield quantitative results, or at times even qualitative results.

D. Baird et al. (eds.), Heinrich Hertz: Classical Physicist, Modern Philosopher, 39–58.

Figure 1. The author, demonstrating replicas of Hertzian apparatus.

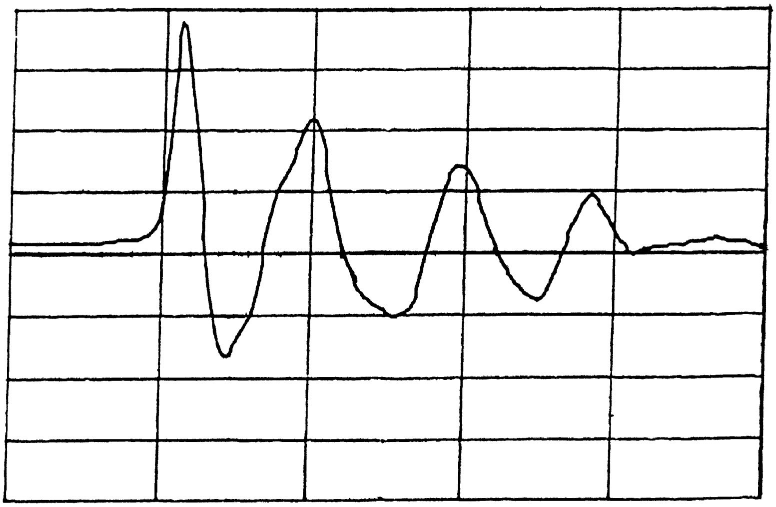
II. A FOUR-YEAR SEARCH

Getting started

In reading Hertz's collected works in electromagnetics (EW and Misc) ten years ago, I formulated my own interpretation of how his apparatus functioned, but questions remained about exactly what Hertz did, how he did it, and what he saw. Hertz speaks of the primary, the secondary, a side circuit, Knochenhauer spirals, and a Riess's spark micrometer – terms decidedly not in current use. It became evident also that he often used the half-wavelength for the wavelength.

I wished to travel the road that Hertz traveled; to attempt to understand his grasp of theory, how he translated theory into concepts, and transformed concepts into the design of apparatus. I needed first-hand familiarity with his apparatus; to see originals if possible – otherwise good replicas – and comprehensive photographs. I needed to identify the laboratory apparatus which Hertz had available in order to understand where Hertz got the technology embodied in his design of experimental apparatus.

In his papers, Hertz provided line drawings, but usually he supplied no sketches to tell what the various items of apparatus he built looked like. He published no photographs of the apparatus or laboratory equipment, much less how these items were arranged and used in specific experiments. We now know that Hertz took photographs (Figures 4a and 5), but none appear in his papers. Apparently the journals, including *Annalen der Physik*, had no facility for printing photographs in the 1880s.



20.0 ns/div

Figure 2. Waveform of the voltage across the receiver gap. The trace represents the relative amplitude versus time of the electric component of electromagnetic energy generated in the transmitter, and propagated into space. A given percentage of the energy present in the transmitter becomes detached and radiates into space each half cycle. The trace cannot be smooth, since the current flowing back and forth, on the transmitter dipole stops and starts twice per cycle. The result is a decremented amplitude, or damped wave. In this illustration, only $3^1/_2$ cycles of oscillation have occurred, at which time the voltage across the transmitter gap became too low to break down the gap and conduct current. The time period for a complete cycle shown is about 18 nanoseconds (18×10^{-9} second). The reciprocal of that, which is the corresponding frequency, is 55 MHz (wavelength 5.4 m).

In 1927 the British writer Rollo Appleyard visited the University of Karlsruhe, and was shown a set of replicas of Hertz's apparatus which he then photographed (Appleyard 1930, 122). Appleyard's technical descriptions are wanting, however, and he shows no laboratory items. Numerous other writers have published photographs of apparatus at the Deutsches Museum, but, typically, with no useful technical information. Most frustrating, it is repeatedly reported that a spark generates radio waves. This cannot possibly be true. Only a circuit can do that – when suitably supplied with stored electrical energy to be converted momentarily into electromagnetic energy. In the Hertzian oscillator, or transmitter, the central spark gap merely serves as a very fast-acting switch to discharge the electrical energy stored in the dipole, which acts as a capacitor.

My first recourse was to replicate some items of Hertz's apparatus using Hertz's verbal descriptions. I might then operate them, and observe what Hertz observed. I did this in 1984, with quite satisfactory results (Figures 1 and 2). Figure 1 shows a receiver on the right (Appendix Part I, #40058), and a dipole transmitter on the left (Appendix Part III, #12774). Hertz called them an "oscillator" and a "resonator" respectively. The operating wavelength is about 5.5 m (frequency 54 MHz). Behind the transmitter, and barely visible, is a modern induction coil, used to pulse the transmitter, that is, supply pulses of electrical energy to be converted into electromagnetic energy. The smaller items in the center left of Figure 1 are replicas of shorter wavelength appa-

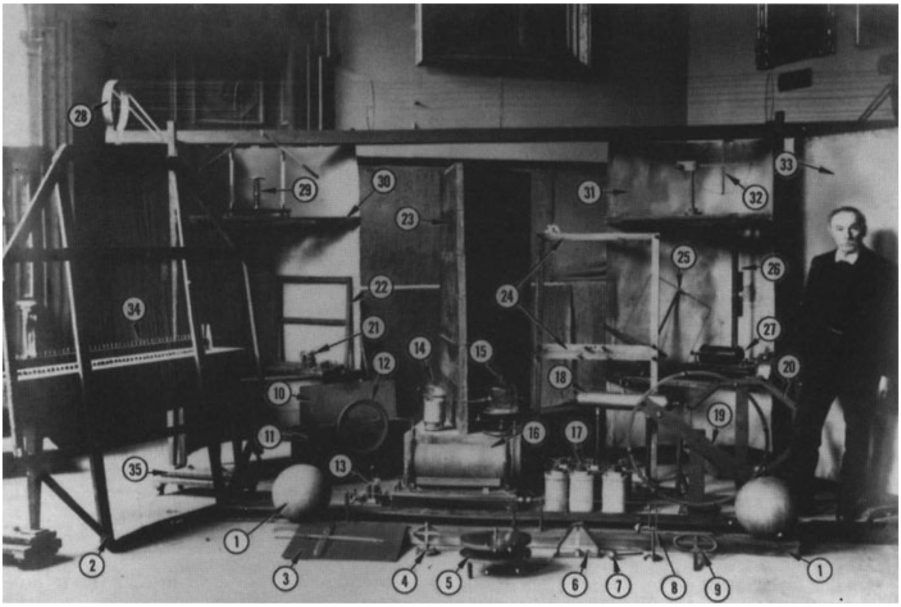


Figure 3. Photograph of equipment used by Hertz. Laboratory equipment, plus apparatus designed by Hertz and built with his mechanic assistant, Julius Amman (shown in Munich in 1913). The corresponding inventory numbers of the Deutsches Museum are given in brackets. Where applicable, items are cross-referenced to items in Figures 4a and 5. (Photograph courtesy of the Museum of Science and Industry, Chicago, 1987; a better quality copy received from the University of Karlsruhe in 1988 is used here).

1. First oscillator/radiator transmitter, signal source, 6 m wavelength (frequency 50 MHz) (40055). See item 3 in Figure 4a.
2. Frame and parallel wires for polarization demonstration, both transmission and reflection (40068). See item 3 in Figure 5.
3. Demountable vacuum apparatus, cathode ray experiments (40076).
4. Hot-wire galvanometer.
5. Riess or Knochenhauer spirals; discovery of the generation and detection of RF (radio frequency) energy by Hertz in 1886 (18155).
6. Rolled-paper galvanometer – RF detector (direction and magnitude of the electric force) (40074).
7. Metal sphere with insulated handle – RF probe (40073) See item 6 in Figure 4a.
8. A Riess spark micrometer.
9. Receiver/detector, coaxial transmission line experiments, 6 m wavelength (40060).
10. 11, & 12. Apparatus to demonstrate dielectric polarization (electric displacement) effects in insulators, as predicted by Maxwell:
10. (40067a).
11. (40067).
12. (40053) & (40054) assembled together.
13. Mercury interrupter (circuit breaker), used to pulse induction coils (40071).
14. Meidinger cell (primary battery); same chemistry as the Daniell cell. See item 2 in Figure 4a.
15. Vacuum bell jar, photoelectric effect experiments (40049).
16. Induction coil, high-voltage pulse generator. See item 1 in Figure 4a.
17. Bunsen cells (primary batteries).
18. Large-area conductor, insulated for high voltage, used for storing electric charge. See item 7, Figure 4a.
19. Circular-loop receiver, 6 m wavelength (40057a).
20. Receiver/detector, eight-sided (40059).

Figure 3 Cont'd.
21. Rotating mirror and mercury interrupter assembly (40071).
22. Square-loop receiver, 6 m wavelength.
23. Stack of three wedge-shaped boxes to hold dielectric material, refraction demonstration and dielectric constant measurement. (40063/66). See item 4 in Figure 5.
24. Assembly of two square-loop receivers, 6 m wavelength. See items 4 and 5 in Figure 4a.
25. Square-loop receiver, 6 m wavelength.
26. Transmitter dipole, 60 cm wavelength (frequency 500 MHz).
27. Induction coil, high-voltage pulse generator. See item 5 in Figure 5.
28. Coaxial transmission line (40070).
29. High-voltage discharger. See item 8 in Figure 4a.
30. Cylindrical parabolic reflector/receiver, 60 cm wavelength (40050). See item 2 in Figure 5.
31. Cylindrical parabolic reflector/transmitter, 60 cm wavelength (40050). See item 1 in Figure 5.
32. Circular-loop receiver, 3 m wavelength (frequency 100 MHz) (40057b).
33. Planar reflector (40052). See also item 6 in Figure 5.
34. Battery of accumulators (40040).
35. Undercarriage for 40040 (40041).

ratus: the dipole of the transmitter (see also item 31 of Figure 3, and item 1 of Figure 5), and the corresponding loop receiver (Appendix Part II, #18151).

Identifying experimental apparatus and laboratory equipment

During the period 1984–87 I searched for the identity of Hertz's apparatus and laboratory equipment.[2] At the Deutsches Museum in 1985, I obtained a copy of the incoming inventory of Hertz's apparatus received from Karlsruhe in 1913 (see Appendix Part I). My guide could locate only a few of these items. A few photographs were available. The verbal descriptions are nontechnical, but do appear to have been written by a person who witnessed the experiments; for example, "oscillator for the paraffin block." The identities became clear when I later discovered the first composite photograph, Figure 3. (In the Appendix, Parts II and III are lists of items received from Bonn in 1908, and Part IV contains one historic item received in 1946 or 1947).

At Karlsruhe in 1987, I learned that the set of replicas which Appleyard saw there in 1927 were removed in early 1945 and were not returned. My search was soon to be rewarded, however.

My first breakthrough came when I found the composite photograph (Figure 3) in the Museum of Science and Industry, Chicago, in 1986. I term it a "Rosetta stone" for identifying Hertz's apparatus and laboratory equipment. The Knochenhauer spirals (item 5, consisting of two flat spiral-wound coils) and the Riess's spark micrometer (item 8, consisting of two balls, one fixed, the other moveable by a micrometer screw adjustment), are in the front row along with Hertz's first transmitter (item 1). The callouts have been added. All items of experimental apparatus except one (# 40072) listed in Part I of the Appendix are identified. In addition, nine items of laboratory equipment are shown. Complete cross-referencing is given for items listed. The photograph was taken on October 1, 1913 in the Bavarian Academy of Science, Munich, and includes Hertz's apparatus

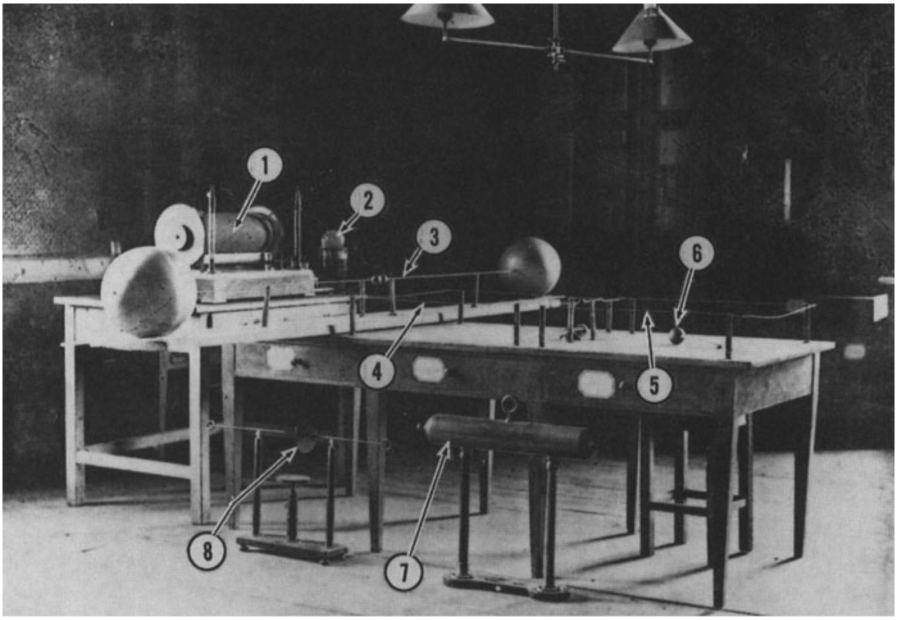


Figure 4a. Photograph taken in Hertz's laboratory. Transmitter and two receivers, 6 m wavelength. (Schleiermacher 1901). All items are cross-referenced to items in Figure 3.

1. Induction coil, to produce pulses of DC potential energy which are converted into RF energy by the transmitter. See item 16 in Figure 3.
2. Meidenger cell (primary battery). See item 14 in Figure 3.
3. Transmitter. See item 1 in Figure 3.
4. Rectangular-loop receiver. See item 24 in Figure 3.
5. Rectangular-loop receiver. See item 24 in Figure 3.
6. Metal sphere, with insulated handle, used to probe (disturb) the fields on each loop and demonstrate resonance. At the nodal points on *cd* or *gh* there is minimum disturbance when the sphere touches the wire. Continuing around a loop, sparks can be drawn from the wire, and, simultaneously, sparks in the detector gap are diminished. The effect increases and is maximum at the detector gaps *1-2* or *3-4*. See item 7 in Figure 3.
7. Large-area conductor, insulated for high voltage. See item 18 in Figure 3.
8. High-voltage discharger. See item 29 in Figure 3.

and laboratory equipment received from Karlsruhe. Unfortunately, the laboratory equipment items were not inventoried.

A copy of the photograph was used in Karlsruhe to publicize the 1988 *Heinrich Hertz Symposium – 100 Jahr Elektromagnetische Wellen.* A resident of the Karlsruhe area, with a better copy of the photograph identified the man in the photograph as her grandfather, Julius Amman, Hertz's mechanic assistant from 1887 onwards. Amman built the apparatus and helped with the experiments.

My second breakthrough came in early 1988 when I received a copy of an old typed manuscript with a handwritten notation of a journal (Schleiermacher 1901). The particular article, by August Schleiermacher, who was junior colleague of Hertz at Karlsruhe, includes two illustrations that are mounted glossy photographs (Figures 4a and 5). These, no doubt, were taken in Hertz's laboratory, around 1887

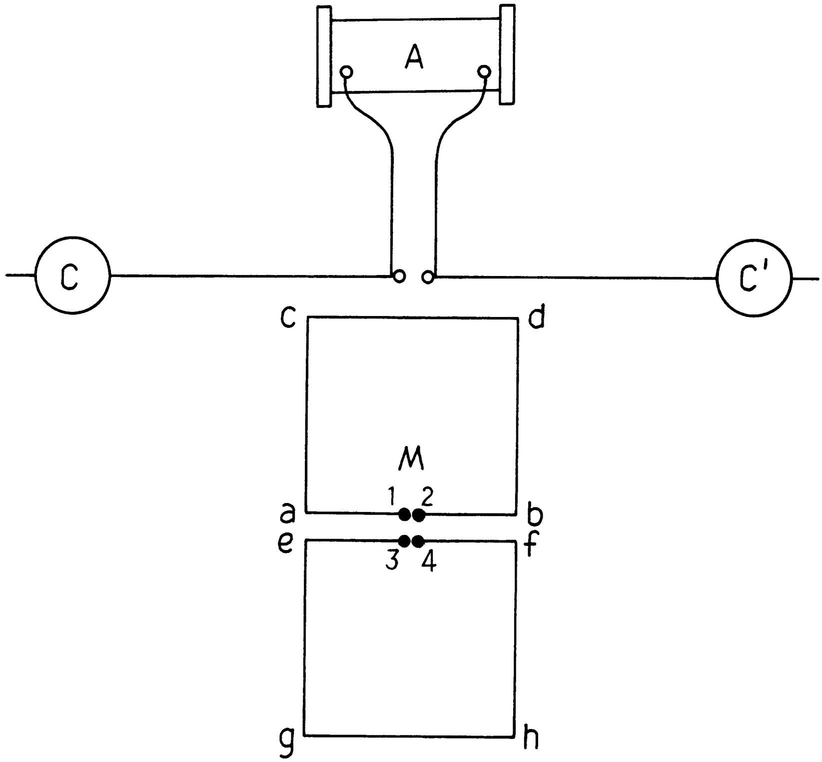


Figure 4b. Sketch by Hertz (EW 47) of the arrangement of apparatus shown in Figure 4a.

and 1888, respectively. All of the items in these two photographs may also be seen in Figure 3. Cross-referencing with items in Figure 3 is included in the captions.

From Figure 4a, one might say that the radio era was ushered in by the light of gas lamps! Figure 4a is valuable in showing the arrangement for several experiments described in Hertz's first article on electromagnetic experiments, "On Very Rapid Oscillations," (EW 29–53). Hertz observed wave interference resulting in voltage standing waves in the wires and signifying oscillatory energy at a single frequency; he observed interaction between open circuits, and resonance effects. Figure 4b is the corresponding line drawing (EW 47) which Hertz included in the article.

Figure 5 shows the ten-times-shorter wavelength apparatus which Hertz designed for demonstrating the optics-like properties of radio waves. The wavelength is about 60 cm (frequency 500 MHz).

Replicas of Hertz's apparatus

With the Chicago Museum of Science and Industry photograph were papers revealing that three sets of exact replicas were built in the late 1920s by a model maker in Munich, Julius Orth, working from the originals. These were made for:

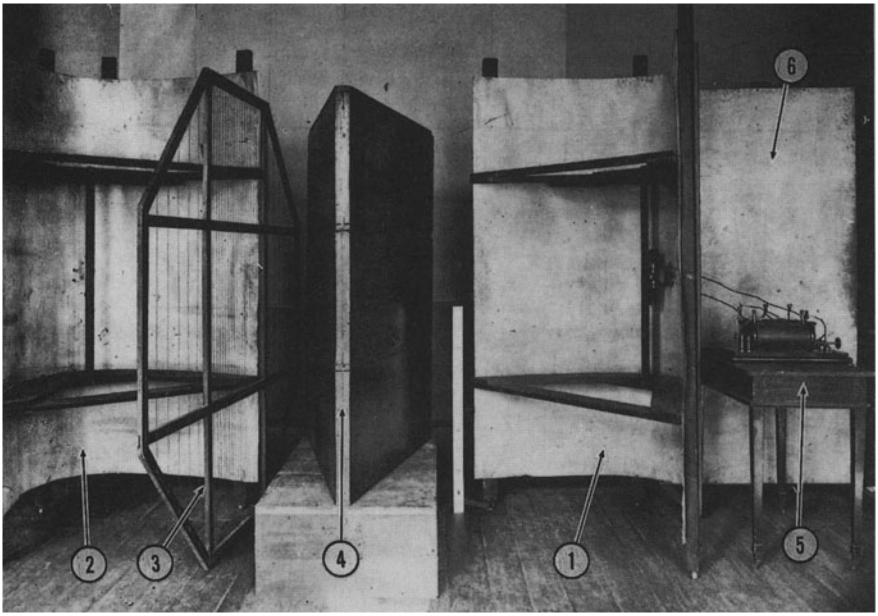


Figure 5. Photograph taken in Hertz's laboratory. Apparatus for demonstrating the optics-like properties of radio waves: rectilinear propagation, polarization, reflection and refraction, 60 cm wavelength (Schleiermacher 1901). All items are cross-referenced to items in Figure 3.

1. Oscillator/transmitter with a cylindrical parabolic reflector. The focal length of the reflector is 12.5 cm and the aperture is 1.2 m wide by 2 m high. See items 26 and 31 in Figure 3.
2. The receiver/detector uses a similar cylindrical parabolic reflector. See item 30 in Figure 3.
3. Octagonal wooden frame with parallel wires, for the demonstration of polarization – both for transmission and reflection. See item 2 in Figure 3.
4. Stack of three wooden boxes (on pedestal) to hold dielectric materials for the demonstration of refraction. See item 23 in Figure 3.
5. Power supply (pulser, induction coil). The two output leads from the induction coil pass through glass tubes in holes in the back of the reflector, and are attached to the transmitter dipole. Items 1, 2, and 5 are on casters, so they can be moved to the desired position for use. See item 27 in Figure 3.
6. Metal sheet reflector for the demonstration of reflection. See item 33 in Figure 3.

(1) The Science Museum, London;

(2) The National Radio Society in Berlin (as I have discovered, these items apparently disappeared in the mid-1930s), and

(3) The 1933 World's Fair in Chicago, after which the items went to the Museum of Science and Industry there. Less than half of the Chicago items survive, and they are in poor condition.

The London set is intact. The items were refurbished, individually photographed, and loaned to the Institute of Electrical and Electronics Engineers (IEEE) for exhibit at the 1988 Microwave Theory and Techniques Society (MTT-S)

International Microwave Symposium, May 25–27, 1988 in New York City. After the symposium, the exhibit was moved to the Massachusetts Institute of Technology Museum in Cambridge, Massachusetts, during the summer and fall before their return to London. A firsthand examination of these items is crucial to understanding their function and use. The separate photographs of each item are a valuable addition to archives. A comparison with the photographs of the originals speaks for these replicas as representative of the original pieces in size, shape, construction, and, therefore, in functional detail.

Making use of the newly found photographs in addition to line drawings by Hertz, I produced a set of 35 posters and a book (Bryant 1988a) to accompany the 1988 IEEE MTT-S exhibit.[3] These are laid out as nearly as possible in chronological order to demonstrate Hertz's step-by-step process of discovery. Identification complete, I can proceed to discuss Hertz's experiments – but first, some background.

III. BACKGROUND TO HERTZ'S EXPERIMENTS

Maxwell and Helmholtz

In a series of papers (1855–64) the British mathematician James Clerk Maxwell put forward a set of equations which put in mathematical form virtually all that was known about electricity and magnetism. A turning point occurred in Part III of his 1861 paper; he mathematically invented the displacement current, the time rate of change in the electric displacement, for his equation describing Ampere's law. These equations came to be known as Maxwell's equations. He did not relate them to any circuit or to any boundary conditions, and therefore not to any source or means for detection. Maxwell derived a wave equation, and related the two resulting constants to already measured quantities. The result predicted the velocity of propagation to be the (already measured) velocity of light. Maxwell believed that light was electromagnetic. He included nine key equations in his 1864 paper, and eleven key equations in his *Treatise* in 1871. He left it at that.

Hermann von Helmholtz came to studies in electricity in 1847 from a problem that arose in physiological researches. In his paper, "On the Conservation of Force, Physical Memoir," he suggested that the discharge from a Leyden jar capacitor is oscillatory:

> ... the discharge of a battery is not a simple motion of the electricity in one direction, but a motion backward and forward between the coatings in oscillations which get smaller and smaller until the entire *vis viva* [energy] is destroyed by the sum of the resistances. The notion that the discharge current consists of alternately opposed currents is supported, first of all, by their alternately opposed magnetic effects, and secondly, by the fact (observed by Wollaston while attempting to decompose water by electric discharges) that both kinds of gases [hydrogen and oxygen] are developed at both electrodes. (1971, 31)

When Helmholtz published the first of his major papers on electromagnetism in 1870, "On the Motional Equations of Electricity for Resting Conducting Bodies" (1882, 1:545–628), there were three principal electrical theories: the potential law of Neumann (1845), Weber's expression for the action at a distance between moving charges (1846), both based on action at a distance, and the aether theory of Maxwell. Helmholtz saw faults in the first two, but promise in Maxwell's equations. Helmholtz was the first Continental European scientist to support Maxwell's theory. In his investigations, Helmholtz studied the interaction of open electric circuits. That he did not succeed in developing a consistent theory and a validating experiment should not be surprising. Helmholtz stated later:

> I set myself the task of surveying the domain of electromagnetism and working out the distinctive consequences of the various theories in order, wherever possible, to decide between them by suitable experiments. (PM xxvi)

The experiments include studies by his students Schiller, Rowland, and Hertz.[4] He suggested another topic for Hertz, in 1879 (see below on the "Berlin Prize" problem).

But why was it left to Hertz, in 1886, over two decades after Maxwell, to be the one to comprehend Maxwell's theory, master the underlying mathematics, and design experimental apparatus to generate and detect electromagnetic energy and explore its properties in detail? Hertz certainly was not the first to observe electromagnetic effects. His discovery in late 1886 was different, however. His was a prepared mind; and he was a gifted experimentalist and a highly competent mathematician.

Hertz's education

As a youth, Hertz had a shop with a lathe; he built household items as well as instruments. He took separate instruction in metalworking, woodworking, and mechanical drafting. He graduated from the *Gymnasium* (high school) with a record that qualified him for admission to any German university of his choice. He was undecided between a career in engineering or science. He spent two years preparing for engineering, and then served his mandatory year of military service (1876–1877). He chose a career in pure science and spent a fruitful year (1877–1878) in Munich attending both the University and the Technical Institute, taking courses and directed study in mathematical and experimental physics.

After transferring to the University of Berlin in 1878, Hertz (age 21) decided to compete for the prize physics problem for the semester. He was fortunate in getting Hermann von Helmholtz as his mentor. Helmholtz helped Hertz determine what instruments he needed, he introduced him to the appropriate technical literature, and he paid daily visits to discuss progress. This resulted in Hertz's first published paper (Misc 1–34), "Experiments to Determine an Upper Limit to the Kinetic Energy of an Electric Current." He designed and built the experimental apparatus. Requiring both analytical acumen and exacting experimental work, this project set the pattern for Hertz's later investigations. It also introduced him to coupled electric circuits: first solenoidal windings of many turns, then rectangular loops of straight wire for which he could calculate the self inductance. This experience proved useful for designing his first transmitter and receiver eight years later.

The "Berlin Prize" problem

In 1879 Helmholtz called for an experimental validation of one facet of Maxwell's theory and had it published as a prize problem of the Prussian Academy of Science (Berlin) in 1879. It is often referred to as the "Berlin Prize" problem:

> Mr. du Bois-Reymond gave a report on the prize in the physical-mathematical class which is to be paid out of the Ellert legacy. The Academy poses the following question for the 1882 prize: The theory of electrodynamics which was brought forth by Faraday and was mathematically executed by Mr. Cl. Maxwell presupposed that the formation and disappearance of the dielectric polarization in insulating media – as well as in space – is a process that has the same electrodynamic effects as an electrical current and that this process, just like a current, can be excited by electrodynamically induced forces. According to that theory, the intensity of the mentioned current would have to be assumed equal to the intensity of the current that charges the contact surfaces of the conductor. The Academy demands that decisive experimental proof be supplied either:
>
> for or against the existence of electrodynamic effects of forming or disappearing dielectric polarization in the intensity as assumed by Maxwell
>
> or
>
> for or against the excitation of dielectric polarization in insulating media by magnetically or electrodynamically induced electromotive forces.
>
> Answers to this question have to be submitted by March 1, 1882. Submissions may, at the author's discretion, be written in German, Latin, French, or English. Each submission has to bear a motto which must be repeated outside of a sealed envelope containing the author's name. The prize of 100 ducats = 955 marks will be awarded at the public meeting of the Academy on the Leibniz anniversary in July 1882.[5]

Helmholtz thought that Hertz would be the most likely to succeed. In 1892 Hertz wrote:

> As I was at the time [1879] engaged upon electromagnetic researches [on the aforementioned faculty prize problem on inertia of electricity] at the Physical Institute in Berlin, Herr von Helmholtz drew my attention to this problem, and promised that I should have the assistance of the Institute in case I decided to take up the work. I reflected on the problem, and considered what results might be expected under favorable conditions by using the oscillations of Leyden jars or of open induction coils. The conclusion at which I arrived was certainly not what I had wished for; it appeared that any decided effect could scarcely be hoped for, but only an action lying just within the limits of observation. I therefore gave up the idea of working at the problem; nor am I aware that it has been attacked by anyone else. But in spite of having abandoned the solution at that time, I still felt ambitious to discover it by some other method; and my interest in everything connected with electric oscillations had become keener. It was scarcely possible that I should overlook any new form of such oscillations, in case a happy chance should bring such to my notice. (EW 1f.)

Thus Hertz concluded that the time was not right for that as a near-term project. He had taken a crucial step, however, in his study of how he might generate and detect electromagnetic energy. He was able to choose a more tractable thesis topic (Misc 35–126), and obtained his doctorate in 1880. Numerous entries in his diary show that Hertz did in fact give a great deal of thought to electromagnetics in the following years. In three years (1880–83) as demonstrator in the Physical Laboratory of the Berlin University under Helmholtz, Hertz's research career blossomed. From work at Berlin, he published papers on diverse topics: mechanics, instrumentation, friction, magnetics, meteorology, electricity, cathode rays, and electrical discharges in gases.

A crucial, analytical step in 1884

In two years at Kiel University as *Privatdozent*, 1883–85, Hertz was isolated from like-minded colleagues and had no laboratory facilities. During this time he wrote an important theoretical paper, "On the Relations Between Maxwell's Fundamental Electromagnetic Equations and the Fundamental Equations of the Opposing Electromagnetics" (Misc 273–290). In it he derived Maxwell's equations for steady time-varying electric current from Ampere's law and Faraday's law. The word "*fundamental*" in the title has particular significance. Hertz's derivation was the first expression of Maxwell's equations in compact, symmetrical form, showing how time-varying inextricably related electric and magnetic fields interact (transporting energy). Hertz did not use vector notation; he wrote all equations in scalar form. This paper was his crucial third step toward successful electromagnetics experiments. The logic and the derivation are quite different from Maxwell's.

A result of Hertz's work is the natural appearance of electric displacement current in the electric system and the magnetic displacement current in the magnetic system.

Hertz stated that if he had to make a choice between Maxwell's theory and another, he would choose Maxwell's theory:

> I have attempted to demonstrate the truth of Maxwell's equations by starting from premises which are generally admitted in the opposing system of electromagnetics, and by using propositions which are familiar in it. [...] I think, however, that from the preceding we may infer without error that if the choice rests only between the usual system of electromagnetics and Maxwell's, the latter is certainly to be preferred. (Misc 289)

In early 1993, my colleague C.T. Tai and I undertook a study of Hertz's 1884 paper. He filled in some detailed steps not found in Hertz's original paper (an exacting undertaking), recast the entire work in modern notation, and, most important of all, deduced some new information which can be extracted from Hertz's work (Tai and Bryant 1994). Hertz's paper has been previously studied. In particular, Havas (1966) first pointed out that the equations as Hertz left them are not acceptable from a physical point of view, but Havas did not offer an amendment. The missing criterion is the radiation condition which Hertz never mentioned in his 1884 paper. We note, however, that in an 1889 paper, "On the Force of Electric Oscillations, Treated According to Maxwell's Theory" (EW 137–159), Hertz derived the retarded potential, satisfying the radiation condition. The principal incompleteness of the 1884 paper is the constraint of closed currents and the missing term in the solution for the Poisson equations. This constraint can be readily removed, resulting in the complete system of Maxwell's equations from Hertz's theory of electromagnetism, based on an independent and original formulation quite distinct from Maxwell's.

There is a dilemma in that Hertz never later referred to this paper, although he used the results in two subsequent papers (EW 137–159, 195–240). D'Agostino (1975, 293–296) discussed the reaction in Germany to the 1884 paper, and what I term Hertz's later partial abandonment of it. It is well known that Hertz suffered severe depression in "the last months at Kiel and first year at Karlsruhe" (MLD, xi and 211), in the years 1884–1885. In addition the paper is incomplete, as he must have realized, and the means for a validating experiment still eluded him.

On 29 March 1885 Hertz moved to Karlsruhe as a professor. He tried to resume experiments with cathode rays (in gas discharges), but had difficulty getting any useful research going in the first year. But here his life changed. He had his own department, including a laboratory, shop, and some staff. On 31 July 1886 he married Elisabeth Doll, the daughter of a faculty colleague, and started successful electromagnetic experiments later that year.

IV. HERTZ'S DEFINING EXPERIMENTS, 1886–1890

Background

A more detailed and complete description of Hertz's experiments in electromagnetics can be found in my *Heinrich Hertz: The Beginnings of Microwaves* (1988a). Here I wish to address two often misunderstood aspects of these experiments: (1) The mechanism of operation of Hertz's transmitter and receiver (in Hertz's terminology, the "primary" and "secondary," or respectively, the oscillator and resonator), and (2) some reasons for Hertz's difficulties in measuring the velocity of propagation in air using wave interference in the confined space of his laboratory, and from using damped waves.

One must marvel at the ongoing insight Hertz showed, as well as his productivity, in delineating the properties of electromagnetic waves while carrying a normal teaching load. He discovered both how to generate and how to detect electromagnetic energy while experimenting with coupled circuits (Knochenhauer spirals), in the form of two laboratory coils with open ends. He then knew how to proceed. In the first paper on his electromagnetic experiments, "On Very Rapid Electric Oscillations," he wrote:

> The electric oscillations of open induction-coils have a period of vibration which is measured in ten-thousandths of a second. The vibrations in the oscillatory discharges of Leyden jars, such as were observed by Feddersen, follow each other about a hundred times as rapidly. Theory admits the possibility of oscillations even more rapid than these in open wire circuits of good conductivity, provided that the ends are not loaded with large capacities; but at the same time theory does not enable us to decide whether such oscillations can be actually excited on such a scale to admit of their being observed. (EW 29)

In the late 1850s, Feddersen produced oscillatory frequencies up to almost one megahertz – wavelength 300 m. Using a revolving mirror, he could record on film a series of flashes for each discharging. There is a flash each half cycle, corresponding to the voltage peaks such as illustrated in Figure 2. This wavelength is too long for apparatus to be used in a laboratory.

Initial experiments

On his first try, Hertz achieved an operating wavelength of about 6 m (frequency 50 MHz). Using his knowledge of coupled open wire circuits from his first experiments at Berlin (Misc 1–34), he emulated the Leyden jar capacitor by replacing the plates with metal spheres 30 cm in diameter, and connected them with a length of wire with a gap in the center (item 1 in Figure 3, and items 3 in Figures 4a and 4b). He thus invented the half-wave dipole, and used it as his transmitter circuit – as the oscillator and the radiator (antenna). Hertz calculated the frequency from the capacitance of the two spheres, and the self-inductance of the wire (EW 29–53). Aside from making the mistake of omitting a factor (the square root of 2), he got a satisfactory result. He used this in subsequent investigations of velocity of propagation (numerically, taking the product of the computed frequency with the measured wavelength).

For charging the dipole, that is, supplying pulses of energy to be converted into oscillatory energy, he used a laboratory induction coil – a technology from almost half a century before. For discharging, he incorporated widely used technology dating from the previous century – the opposing spherical surfaces of two metal balls – placed across the gap – the spark gap. (The terminals of his Leyden jar, as well as the open ends of his Knochenhauer spirals had the same). This completed his transmitter. The spacing of the spark gap determines the voltage to which the dipole capacitor momentarily is charged. For 0.75 cm spacing, the breakdown voltage is roughly 20 000 volts. The potential energy of the momentary charge is stored in the electric field. When the gap fires, current can flow from one side of the dipole to the other. Coulomb forces cause the charge to redistribute itself, but cannot do so instantaneously due to the inductance of the circuit. At the end of one quarter cycle, the charge is evenly divided between the two halves of the dipole, the electric field is zero, and all of the energy is in the magnetic field which is at its peak. This field starts to collapse, further driving the current to charge the dipole in the opposite direction. At the end of one half cycle, the dipole is charged in the opposite direction, the current stops and reverses and the energy is again momentarily all in the electric field. Actually, a given percentage of the energy detaches each half cycle and is propagated into space. The process repeats until the voltage across the gap is no longer great enough for breakdown. (This latter breakdown voltage is substantially lower than initially, due to presence of free ions in the gap.) The detached, radiated electromagnetic energy exists in related oscillatory electric and magnetic fields traveling together – at the velocity of light in the particular medium. Other electromagnetic radiation from the transmitter, coming directly from the arc in the spark gap – at infrared and shorter wavelengths – is due to quantum effects. The infrared and visible affect our senses. Hertz experimented with the ultraviolet. Presumably there are soft X-rays present as well.

To emulate the secondary of the coupled coils, the receiver, but at the higher frequency, Hertz used a single turn of wire with a gap (items 3 in Figures 4a and 4b). He thus invented the half wavelength resonant loop. This was his receiver circuit. When propagating electromagnetic fields encounter or pass by this loop, a finite amount of energy is transferred, causing oscillatory current to flow and corresponding oscillatory voltage across the open terminals, such as illustrated in Figure 2. For a detector, Hertz borrowed from the Riess spark micrometer. A tiny adjustable spark gap placed across the loop gave an indication of the presence of a voltage, and the maximum length of the gap which would just break down was a measure of its relative magnitude. Hertz had just invented the first detector for voltages at very high frequency, where galvanometers and electroscopes are of no use. Fortuitously, the human eye is complementary as an indicator. The great sensitivity of the dark-adapted eye is shown in the ability to detect the visible output of ionized air resulting from an extremely short burst of sparks less than one-tenth of a microsecond (10^{-7} second) duration. In my demonstrations to an audience, I have used a small neon glow lamp as the receiver detector/indicator. It may be noted that both electrodes glow, indicating an alternating voltage.

In his initial experiments, Hertz observed a voltage maximum across the gap and a null on the opposite side of the loop. This indicates a voltage standing wave, due to wave interference. It showed him that he had electromagnetic effects – at a single frequency – not induction.

Hertz used this arrangement to demonstrate resonance. In changing either the length of the dipole, or changing the length of the loop, with the other fixed, the plot of voltage versus maximum length in each case shows a decided voltage maximum at resonance (EW 45).

During the resonance experiments, Hertz noted erratic readings when the detector gap was exposed directly to view of the arc of the switching gap of the oscillator. Hertz's keen observation and skill as an experimentalist, not to mention his ability to communicate his discovery, are manifest in his paper, "On an Effect of the Ultraviolet Light upon the Electric Discharge" (EW 63–79). In it he launched a new arena for physical investigation to be pursued by others. He identified the effect as being due to radiation from the arc, in the ultraviolet range, "... at the outermost limits of the known spectrum" (EW 77).

Hertz now had the apparatus and techniques to tackle the Berlin Prize problem, alas five years late. Presumably, Hertz did not collect the prize money, the time having expired in 1882. No one else had entered the contest. Perhaps he was the only one besides Helmholtz who had given it any thought. His ingenious use of a microwave free-space Wheatstone type of bridge bears explanation, which I discuss elsewhere (1988a, 26–29).

Wave interference and the measurement of wavelength

Hertz turned next to determining the velocity of propagation, a key feature deducible from Maxwell's theory. Hertz's approach was to take the product of frequency that he calculated, times wavelength that he measured, and compare with the known velocity of light.

The reflected wave is phase shifted by 180° at a conducting surface, resulting in a null at that surface. The reflected wave propagates back along the incident path, interfering with the direct wave, and producing positions of minima, or nulls, spaced at multiples of one-half wavelength from the reflector. For the $3^1/_2$ cycle pulse illustrated in Figure 2, two minima should exist, in addition to the one at the reflector (the existence of the nulls would indicate a wave with a finite velocity of propagation). Hertz calculated the frequency satisfactorily, but encountered problems in measuring the wavelength that are quite readily understood today.

Without a textbook for guidance, Hertz suffered some pitfalls as befits a pioneer explorer. He showed his mastery of concepts in three different arrangements he employed to create waves in attempts to measure the wavelength using wave interference effects: (1) By transmitting the wave along a single-wire transmission line, suspended horizontally, with reflection from the open far end: "On the Finite Velocity of Propagation of Electromagnetic Actions" (EW 107–123). (2) By radiating directly from the transmitter, for waves in air, with reflection from a planar mirror (sheet of metal) a few meters away: "On Electromagnetic Waves in Air and their Reflection" (EW 124–136). (3) By transmitting the wave inside a two-wire transmission line of coaxial construction (item 28 in Figure 3): "On the Propagation of Electric Waves by Means of Wires" (EW 160–171). In (3) Hertz did satisfy boundary conditions, and apparently got a good result using guided waves on a two-wire, coaxial transmission line, as would be expected.

For (1) he did get near to the correct value for the velocity – within a few percentage points after correcting his calculation of frequency, but this was very likely accidental. Perhaps he should not be criticized for not realizing that in (1) he could not satisfy the boundary conditions for launching the wave, that is, without a large conducting ground plane (at least by several wavelengths wide); nor the boundary conditions at the far end without attaching a large planar reflector.

Hertz encountered other problems in (1), and especially in (2). For a damped wave from a Hertzian oscillator, illustrated in Figure 2, the minima (except the one at a conducting surface) will be substantially less pronounced, compared to a wave of constant amplitude, resulting in their positions being correspondingly more difficult to pinpoint accurately. Also, in the confines of his laboratory, there were one or more additional direct and reflected paths, due to reflections from the floor and from posts. All of these scattered waves add up vectorially and result in a shifting in spacing between minima, as well as making the nulls even less distinct. In my own investigations using replicas, and in a similarly confined space, I could readily observe the null at the conducting surface, but found it difficult to discern even the first null at half-wavelength distance. This obviously plagued Hertz, and in his diary, he used terms such as "infinite propagation," and "... could not deduce any definite velocity...." (MLD 235, 251). It may be noted that Hertz's daily writing in his diary consists of brief, often terse comments, some of which could be taken out of context and misunderstood. He did conclude his "waves in air" experiments with a result showing that waves in air travel with a finite velocity, but greater than the velocity on wires. Rather than getting bogged down, he published and other investigators promptly got into the act.

Testing the replicas

Testing the replicas served to clarify the identity of Hertz's apparatus and how it was used. It also helped to understand how the Hertzian oscillator functions and how the Hertzian resonator/receiver functions. Especially important was experiencing the difficulty of determining wavelength by wave interference, using the damped waves from the Hertzian oscillator/radiator compared to using waves of uniform amplitude. Use of an oscilloscope in place of the tiny spark gap, so as to observe the time history of voltage across the receiver gap (Figure 2), shows the damped, oscillatory nature of the wave. It also shows that the oscillation of electricity on the receiver circuit is regular, with a definite period, or frequency. Substituting a small neon glow lamp for the oscilloscope results in a very portable and economical method for demonstration to an audience. A bonus is that this shows the peak voltage across the gap to be greater than 85 volts (the specified minimum breakdown voltage of this type of lamp). And, since both electrodes of the lamp glow, it is alternating current (AC) electricity.

V. CONCLUDING REMARK

It is my conviction that Hertz ultimately relied on his own analysis, and that this was a powerful aid to him in gaining his own understanding of the behavior of his apparatus. His derivation of Maxwell's equations in 1884, based on an independent and original formulation quite distinct from Maxwell's, helped to focus his attention. To turn concepts into apparatus, he often borrowed techniques from laboratory equipment. The items of apparatus designed by Hertz, and built with the help of a mechanic helper, are elegant for their simplicity and functional capability.

Hertz's analytical and especially experimental work in electromagnetics verified Maxwell's theory of electromagnetism, and opened the radio frequency portion of the spectrum (up to, but not including infrared) for scientific and practical uses, and started a new line of investigation in the ultraviolet, pursued by others. His work and his outlook were that of pure science, yet the results of his work form the basis for a wide range of products and services represented in diverse industries and institutions today.

VI. APPENDIX

INVENTORY LISTS OF HERTZ'S APPARATUS RECEIVED AT THE DEUTSCHES MUSEUM

I. *Items received from the Technische Hochschule Karlsruhe, 24 September 1913.* The second column refers to numbered items shown in Figures 3, 4a, and 5. Explanatory notes are included in parentheses.

Museum Inv. #	*Fig. # & Item #*	*Description*
40040	3–34	Battery of accumulators with 1000 cells. (A handwritten notation indicates 1060 cells).
40041	3–35	Undercarriage for the accumulator 40040.
40043–40048		Items received, but promptly returned to Karlsruhe.
40049	3–15	Spark apparatus to generate ultraviolet light, under glass cover. (This is a confused description of a vacuum bell jar used in part of the "effect of ultraviolet light" experiment (EW 63–79). Diameter of the cover 20 cm. Height of the apparatus 35 cm).
40050	3–30 & 5–2	Curved mirror of zinc sheet (cylindrical parabolic mirror), with resonator (receiver) built in; height 2 m.

40051	3–31 & 5–1	Curved mirror of zinc sheet (cylindrical parabolic mirror), with oscillator (transmitter) built in; height 2 m.
40052	3–33 & 5–6	Plane mirror of zinc sheet, height 2 m, width 1 m.
40053	3–12	Oscillator (transmitter) for the paraffin block, with square plates, on a wooden frame. Length 1 m, width 20 cm (goes with 3–10, and 3–11). (Assembled with 40056.) Note: the expression "paraffin block" is jargon. The details describe the transmitter portion of the 3 m wavelength (frequency 100 MHz) transmitter/receiver assembly used in the experiments to demonstrate polarization effects in insulators (the Berlin Prize problem, 1879).
40054	3–25	Resonator (receiver), rectangular, with microscope (missing). Length of side 46 cm.
40055	3–1 & 4a–1	Big open oscillator, consisting of one spark gap, and 2 copper wires each 1 m long with spheres of zinc 30 cm diameter at the ends. Mounted on a board (260 cm × 7.5 cm).
40056	3–12	Circular resonator (receiver) for the paraffin block (assembled with 40053), 35 cm diameter. (3 m wavelength; frequency 100 MHz). (Goes with 3–10 and 3–11).
40057a	3–19	Round resonator (receiver) 70 cm diameter.
40057b	3–32	Round resonator (receiver) 35 cm diameter.
40058	3–22	Square resonator (receiver) 60 cm on side. (This corresponds to the receiver replica shown in Figure 1.)
40059	3–20	Eight-sided resonator (receiver) 67 cm diameter, with stand.
40060	3–9	Resonator (receiver), brass spiral 21 cm diameter.
40061 and 40062		Not listed.
40063/66	3–23 & 5–4	Prism of asphalt for refraction of electric waves. Height 1.59 m, length of side 1.2 m, weight 12 hundredweight (600 kg) when filled with asphalt.
40067	3–11	Paraffin block for induction by dielectric displacement, length 70 cm, height 32 cm, width 18 cm. (Goes with 3–10 and 3–12.) (This item is the wooden container for holding dielectric material for the Berlin Prize problem experiment. See note under 40053.)
40067a	3–10	Condenser sheet 25 cm × 70 cm. (Goes with 3–11 and 3–12). (This is the metal sheet assembly used in the Berlin Prize problem experiment.)
40068	3–2 & 5–3	Eight-sided wire grid for polarization demonstration, both transmission and reflection, 2 m across.
40069	3–18 & 4a–8	Cylindrical conductor on two glass rods for altering the capacity of open oscillating circuits, length 70 cm, diameter 24 cm, height of stand 40 cm.

40070	3–28	Electrodynamical cage to verify the distribution of waves on the surface, consisting of 24 copper wires 5 m long making a cylinder 30 cm diameter. (This is the coaxial transmission line.)
40071	3–21	Rotating mirror to analyze the electric spark. (This is a combination of rotating mirror and mercury interrupter switch. The switch is gear-driven to run at 1/5 the speed of the mirror.)
40072		Wooden trough for electric waves in fluids, height 21.5 cm, width 23 cm, length 45 cm. (This is not identified in Figure 3, but is shown in a photograph in Appleyard 1930, 143. Hertz mentions propagation along columns of conducting fluids (EW 158).)
40073	3–7	Spherical probe 4.5 cm diameter on insulating handle 45 cm long.
40074	3–6	Paper roll electrometer, glass cover, height 17 cm, width 18 cm.
40075		Not listed.
40076	3–3	Cathode ray tube (Hertz did some cathode-ray experiments – discharges in rarefied gases – at Karlsruhe (MLD 207), but did not publish on that work.)

II. *Items received from Frau Elisabeth Hertz, at Bonn, November 1908.* It appears that all of these items were built at Karlsruhe, and taken to Bonn. None of these items appear in Figures 3, 4a, or 5.

Museum Inv. #	*Description*
18150	Commutator made by H. Hertz himself.
18151	Circular resonator (receiver), 9.5 cm diameter, made by Hertz himself.
18152	Circular resonator (receiver), 14 cm diameter, made of a spiral of copper wire. (This is identical in construction to item 15 in Figure 3, but 2/3 the diameter.)
18153	Set-up to investigate glow-discharge; so-called plate-tube. (Cannot identify with anything described or used by Hertz.)
18154	Tangent galvanometer consisting of copper wire, positioned around one wooden disk of 22 cm diameter, and magnet with mirror with silk fiber; height 40 cm.
18155	Fragment of one hot-wire galvanometer, self-made, on a board 12.5 cm × 12 cm.

III. *Received from University of Bonn, 21 December 1908.*

12774	Oscillator with square brass plates, 40 × 40 cm. (This corresponds to the transmitter replica shown in Figure 1.)

IV. *One item received in October 1946 from the Institute of Physics of the University of Bonn*, by Prof. Dr. Gerlach.

70673 Discharge tube to prove the perviousness of aluminum to cathode rays. (Hertz designed an experimental cathode ray discharge tube at Bonn, and three, serial numbers 344, 345, and 346, were procured from an outside vendor. He used one in his last experiment, "On the passage of cathode rays through thin metallic layers" (Misc 328–331). His junior colleague Philipp Lenard fitted one with an extremely thin window of aluminum foil as an airtight seal, and got rays that (Hertz stated): "propagate in air-filled space ... which unlocks an entire new field of research...." (MLD 333). (This item 70673 is #344.)
Note: No laboratory equipment items are on any of the inventory lists.

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John Bryant passed away on June 10th, 1997.

NOTES

[1] I want especially to thank two colleagues at the University of Michigan, Prof. Valdes Liepa for continuing discussions and his assistance on instrumentation and measurement during the past ten years, and Prof. C.T. Tai for his collaboration, and in clarifying Hertz's 1884 paper. I wish to thank the organizers of the Hertz symposium for inviting me to participate, Prof. T.L. Simpson of the Department of Electrical and Computer Engineering of USC for support and help with the demonstrations at the Hertz Conference. I have benefited from stimulating discussions with Prof. John D. Kraus, Prof. Jed Buchwald, and Prof. Joseph F. Mulligan.

[2] The stages of this search are marked by a 1984 lecture "The First Ten Years of Radio, 1887–1897: Hertz to Marconi," by a 1985 demonstration at the IEEE MTT-S Symposium in St. Louis, and by Bryant 1988b.

[3] Four sets of the posters were produced. One set was expended at the exhibit in New York. A second set now resides with the London set of replicas, owned by the Science Museum, London. A third set is in the IEEE MTT-S historical collection, in the Historical Electronics Museum, Baltimore, and was on exhibit at the Hertz Conference in March 1994. A fourth set is in possession of this author.

[4] Schiller 1876 in *Annalen der Physik und Chemie*, (**2. Folge**) **159**, pp. 456–473, 537–553; Rowland 1876 in *Annalen der Physik und Chemie*, (**2. Folge**) **158**, pp. 487–493; and Hertz in 1878, cf. Misc 1–34.

[5] Monthly Report of the Prussian Academy of Sciences in Berlin, July 1879, pp. 519, 528 and 529.

GIORA HON

HERTZ'S STUDY OF PROPAGATION VS. RUTHERFORD'S STUDY OF STRUCTURE: TWO MODES OF EXPERIMENTATION AND THEIR THEORETICAL UNDERPINNINGS

Now, there is no arguing with nature; it must be as it is, but I should have certainly liked it better to obtain a clear, positive result than this more negative one. (MLD 240)

Hertz's experimental studies are essentially studies of propagation. They were carried out in a rich theoretical context with a view to judging which of the competing theories was the correct one. The principal theoretical difficulty was to formulate the most appropriate problem amenable to experimental testing, given the sensitivity of the available instruments. I argue that Hertz abstracted from this experience the philosophical principles which he presented in the Introduction to his *Principles of Mechanics.*

The experience of Rutherford was categorically different. Commencing where Hertz had finished, he found himself in a land rich with amazing phenomena but barren of theory. He had to plant by himself the signposts which eventually directed the way from classical physics to quantum theory; the way, that is, from propagation phenomena to the study of structure by collision processes: the very mark of the principal experiments in physics in this century.

The comparison between these two great physicists and their respective contrasting experimental studies of propagation and structure is revealing and throws light on the link between theory and experiment.

HERTZ VS. RUTHERFORD: THE PRINCIPAL CLAIMS

Just before the turn of the century, in an address to the meeting of the natural scientists at Munich, Boltzmann drew a sharp demarcation between experimental and theoretical physics. "In the experimental field," Boltzmann observed, "[one] simply continues working automatically, and the enquirer needs only to go on supplying fresh material as it were, just as a weaver puts fresh yarn on his mechanized loom. Thus a physicist needs only to continue to test new substances for viscosity, electric resistance and so on, repeating these measurements at [very low and high] temperatures." To be sure, Boltzmann did acknowledge the measure of ingenuity which is required to discover in each case the experimental conditions under which these measurements can be done. Still, this task appeared to him different from that which challenges the theoretician. "It is not quite so simple with the methods of theoretical physics," he observed, though as he admitted, "[t]here too we can in a

D. Baird et al. (eds.), Heinrich Hertz: Classical Physicist, Modern Philosopher, 59–72.

sense speak of an automatic running-on." However, in theoretical physics, Boltzmann observed, the urgent need for discovering and perfecting of an especially suitable method of research explains "why men soon started to think not just about things but also about the method of our thinking itself; thus arose the so-called theory of knowledge, which, in spite of a certain tang of old-style metaphysics now discredited, is highly important to science" (Boltzmann 1974, 78).

Heinrich Hertz was the man who in Boltzmann's view had gone furthest in discovering and perfecting methods of research. For Boltzmann, Hertz's mechanics was really a program for the distant future. Indeed, according to Boltzmann, Hertz complemented not only Kirchhoff's mathematico-physical ideas but also the epistemological conceptions of Maxwell. Hertz, Boltzmann pointed out, "makes physicists properly aware of something philosophers had no doubt long since stated, namely, that no theory can be objective, actually coinciding with nature, but rather that each theory is only a mental picture of phenomena, related to them as sign is to designatum [...] It cannot be our task," Boltzmann continued, "to find an absolutely correct theory but rather a picture that is as simple as possible and that represents phenomena as accurately as possible." He then concluded in an Hertzian spirit, "[t]he assertion that a given theory is the only correct one can only express our subjective conviction that there could not be another equally simple and fitting image" (1974, 90f.).

Some four years later, in an address given to the Scientific Congress in St. Louis, Boltzmann realized that the splitting of physics into the theoretical and the experimental is only a consequence of the misleading two-fold division of methods dealing separately with conceptions and experiences of nature. He insisted that, "we must not aspire to derive nature from our concepts, but must adapt the latter to the former. We must not think that everything can be arranged according to our categories or that there is such a thing as a most perfect arrangement: it will only ever be a variable one, merely adapted to current needs." Thus, the splitting of physics into the theoretical and the experimental "will not remain forever" (1974, 166).

Did Boltzmann realize the new course which physics was taking at the time? Did Boltzmann have Rutherford in mind? The case of Rutherford certainly demonstrates that experimental physics is not like weaving: it does not continue working automatically; the experimenter, or better, the good experimenter, is not a weaver who puts fresh yarn on his mechanized loom so that the weaving will continue with no interruption. Rutherford's discoveries show that good experimental physics has no automated procedures, nor is it an independent discipline which can stand on its own, separated from theory. These discoveries also demonstrate that nature cannot be arranged solely on categories. Boltzmann, however, did not mention Rutherford. By 1904 Rutherford had not yet been recognized as "a force of nature," to use Rosenfeld's apt description. With the benefit of hindsight I wish to do precisely that: to study Rutherford's contribution in juxtaposition to Hertz's. I claim that with his physics Hertz presented experiments, theories and methodology – lock, stock, and barrel, the quintessential nineteenth century physics. His greatness was to cast all the principal elements: the experiments and theories of Helmholtz, the imagery of Faraday, the mathematics of Maxwell and a few more elements, into one whole. What characterizes this physics is the sole concern with dynamics: at stake were propagation phenomena. The comparison with Rutherford is therefore particularly instructive since Rutherford's physics was that of structure and, what is more, nothing was available to him: neither experiments, nor imagery, nor mathematics. He had to start literally from scratch, transforming as he did each discovery into a new tool for further experimental researches. His was a new physics, the physics of the atom. It is thus interesting to examine whether the insights which Hertz had gained had an influence on physics in general and on the development of atomic physics in particular.

To anticipate my conclusions: Hertz was indeed a great classical physicist. He was also a modern philosopher. As he reflected on the traditional methods of science and indicated their epistemological limitations he thereby contributed to philosophy of science. It is however doubtful whether he succeeded in imparting anything of his great insights to the new physics of the twentieth century, at least not to the physics in the period succeeding his premature death. As much as Hertz's strength was in both abstract generalizations and concrete experimental demonstrations, his experience was nevertheless limited to the study of propagation phenomena. The study of structure, I shall conclude, required a different outlook, a different intuition, an intuition that did not seem to follow the rules which Hertz had laid down. Nothing of the philosophical sophistication clearly displayed in Hertz's *Principles of Mechanics* may be found in Rutherford's physics. As Rosenfeld reminisced, "[a]nyone who has seen Rutherford remembers a force of nature, and not a very deep, subtle or intricate thinker." "I like simple pictures," Rutherford once said in a public lecture, "because I am a simple person myself."[1] Paradoxically, it was a very crude picture of reality based on simple classical conceptions which ushered in the physics of the atom.

I seek then to demonstrate that Hertz's contribution, be it in physics or in philosophy, is the crystallization of the nineteenth century reflections on the methodology, theories and experiments which are all associated in one way or another with propagation phenomena. This body of knowledge stands apart from the study of structure – the very essence of the physics which Rutherford developed.

HERTZ AND THE NINETEENTH CENTURY PHYSICS OF PROPAGATION

The urgent need for suitable methods of research encouraged men, as Boltzmann observed, to reflect on methods of thinking and propose theories of knowledge *vis-à-vis* the method of science. Indeed, Hertz's Introduction to his *Principles of Mechanics* should be seen in precisely this light. This idea of Hertz to reflect on the methods of thinking in science was not however entirely new. Helmholtz's discussion of fundamental methodological principles, Mach's influential historical studies and Maxwell's revealing reflections on the methods of theorizing, constitute, amongst others, major sources of philosophical contemplations which had preceded and, in effect, influenced Hertz's philosophy and methodology. A brief summary of each of these principal sources is now in order.

In his introductory lectures on theoretical physics, Helmholtz discussed not the expected topics of mechanics but a more fundamental matter, namely, methodological principles that apply to all parts of physics. Helmholtz justified this discussion on the grounds that "we must investigate the instrument we work with," arguing that "the construction of concepts, hypotheses, and laws and their quantitative formulation in differential equations and integrals have to be discussed if the work of theoretical physics is to be understood." In his Berlin lectures, Helmholtz presented physics not as a set of many isolated explanations, but as a whole – a connected science. In Helmholtz's physics the connecting thread between the various parts of physics is above all dynamics. Dynamics provide the concepts, pictures, laws, comprehensive principles, and invariant magnitudes by which physics expresses its most general viewpoints. Dynamics was the source of the universal in physics. Indeed, terms such as "universal," "comprehensive," "general," and "invariant," are the concepts that stand out in Helmholtz's presentation of theoretical physics (Jungnickel and McCormmach 1986, Vol. II, 135, 140–141).

"We are considering the whole range of present physical knowledge," wrote Hertz, echoing this view, in the Introduction to his *Principles of Mechanics* (PM 10). The three "ultimate" problems of physics which he had earlier singled out in his Heidelberg address, "On the Relations between Light and Electricity," reflect his strong preoccupation with dynamics, but at the same time these problems indicate the limit of this approach. The first problem is gravitation, the one remaining action at a distance, which Hertz believed would also be shown to be finitely propagated. The second is the nature of electricity, which was now understood to extend "over all of nature." The third, and in Hertz's view the "all important question," is the "nature, the properties of the space-filling medium, of the ether, its structure, its rest or motion, its infinite or bounded extent." The solution of this problem, Hertz believed, would reveal the nature of electricity and matter: "Today's physics is inclined to ask the question if all that exists had not been created from the ether" (Misc 326; *cf.*, Jungnickel and McCormmach 1986, Vol. II, 91f.).

The vision with which Hertz closed his talk was of a unified physical theory of the ether, the substratum of the entire phenomenal world, of light, electricity and magnetism, gravitation and heat. We can see here how the connecting thread leads from dynamics to structure. It stopped short however of structure. By and large, nineteenth century physics dealt with dynamics, it had no solution to offer as to the structure of the ether. The medium remained concealed. Hertz was right: the future of physics lay then in structure, albeit a structure of a different entity, namely, the structure of the atom. Alas, Hertz did not live to see this development.

Another source of influence is Mach. Mach used the history of science as a critical means for interpreting and illuminating epistemological problems in science. He sought in his *Science of Mechanics* to demonstrate how the development of statics and dynamics turned from historical contingency into philosophical necessity. Mach stressed the need to acknowledge and, indeed, to avoid the subtle way in which scientific constructs take on a status of philosophical necessity rather than historical contingency. Accordingly, he sought, as Boltzmann put it, "to describe every group of facts by enumeration and by an account of the natural history of all phenomena that belong to that area, without restriction as to means employed except that it renounces any uniform conception of nature, any mechanical explanation or other rational foundation" (1974, 95). Hence Mach's dictum that electricity is nothing but the sum of all experience that we have had in this field and still hope to have. This view of "general phenomenology," as Boltzmann called it (1974, 95), seeks to represent phenomena without going beyond experience.

Mach's vehement objection to the process in which scientific constructs take on a status of philosophical necessity rather than historical contingency, is reflected in his praise of intuition. Hence the criticism by Planck, who argued that Mach's prescriptions for a physics in which reality is identified with human sense perceptions do not offer a useful approach to a unified world picture. The history of the physical world picture, Planck said, shows a progressive liberation of physical ideas from the vagaries of time, place, and individual intellects. If Mach were right, there would be as many different physical world pictures as there are different intellects. But this is not the case, Planck argued, the physical world picture has indeed achieved a large measure of unity (Jungnickel and McCormmach 1986, Vol. II, 252).

Notwithstanding Planck's criticism, it is intuitive knowledge which is very frequently taken as the starting-point of investigations. Every experimenter can daily observe the guidance that such a knowledge furnishes him or her. If one were to succeed in abstractly formulating what is contained in it, one would have made an important contribution to science. But then, how does intuitive knowledge originate, and what are its contents? Everything which we experience in nature imprints itself *uncomprehended and unanalyzed* in our precepts and ideas, which, then in their turn, mimic the processes of nature in their most general and most striking features. In these accumulated experiences we possess a treasure-store of which only the smallest portion is embodied in clear, articulate thought. It takes a great power of abstraction to tap this reservoir of intuitive knowledge and bring it to fruition. Indeed, the great natural inquirer exhibits as a rule the union of the strongest intuition with the greatest power of abstract formulation.

The reader might think that this view belongs to Hertz. It does not. This is how Mach perceived the working of a scientist's mind (1960, 36f.).[2] But the analysis indeed fits Hertz's position very well. I believe that the Introduction of Hertz's *Principles of Mechanics* is precisely an attempt to lay bare the processes of the mind, both the intuitive and the discursive, and to provide criteria of judgement. However, what Hertz offers is not the "general phenomenology" of Mach, but rather what Boltzmann called "mathematical phenomenology":

> [P]hysics must [...] pursue the sole aim of writing down for each series of phenomena, without any hypothesis, model or mechanical explanation, equations from which the course of the phenomena can be quantitatively determined. (Boltzmann 1974, 95)

Hertz's reformulation of Maxwell's theory held an interest of its own, quite apart from the clarity it brought to the subject. By starting from bare differential equations describing experimental results rather than from detailed physical pictures, Hertz offered physicists a fine example of mathematical phenomenology: "To the question, 'What is Maxwell's theory?' I know of no shorter or more definite answer," Hertz stated, "than the following: – Maxwell's theory is Maxwell's system of equations" (EW 21). This approach was of course very appealing to Mach. He admired this way of doing physics. He told Hertz that he read his 1890 work with "special interest," since in it Hertz followed the "ideal of a physics free of mythology" (Jungnickel and McCormmach 1986, Vol. II, 96). Hertz concluded the theoretical part of his Introduction to *Electric Waves* with a clear and somewhat colorful statement of this position:

> [S]cientific accuracy requires of us that we should in no wise confuse the simple and homely figure, as it is presented to us by nature, with the gay garment which we use to clothe it. Of our own free will we can make no change whatever in the form of the one, but the cut and color of the other we can choose as we please. (EW 28)

The roots of this view reach back not only to Mach but also to Maxwell's methodological instructions.

In his essay, "On Faraday's Lines of Force," Maxwell reflected on some crucial methodological aspects of theorizing in physics. According to Maxwell:

> [T]he first process [...] in the effectual study of the science, must be one of simplification and reduction [...] The results of this simplification may take the form of a purely mathematical formula or of a physical hypothesis. In the first case we entirely lose sight of the phenomena to be explained; [... while] if [...] we adopt a physical hypothesis, we see the phenomena only through a medium. [...] We must therefore discover some method of investigation which allows the mind at every step to lay hold of a clear physical conception, without being committed to any theory founded on the physical science from which that conception is borrowed [...][3]

The conceptual tool for executing this reduction without falling into the traps of either mathematical formula or physical hypothesis is the physical analogy. "By a physical analogy," Maxwell explained, "I mean that partial similarity between the laws of one science and those of another which makes each of them illustrate the other." To understand is to see analogies, that is Maxwell's dictum. He was much impressed by the analogies he had discerned in nature; by the reappearance of the same plan: the same laws, the same differential equations apply to heat conduction as to the distributions of electricity in conductors. Light and the vibrations of an elastic medium present another analogy which subsists on the resemblance *in form* between the laws of light and those of vibrations. "By stripping [the science...] of its physical dress and reducing it to a theory of 'transverse alternations,' we might obtain a system of truth strictly founded on observations [...]" Let me stress that this is not a quotation from Hertz, but once again one finds a view which Hertz could have easily expressed. As a matter of fact, this view comes from an early work of Maxwell.[4]

Maxwell elaborated this methodology in his address to the Mathematical and Physical Societies of the British Association. Being acquainted with several different sciences, the student of nature finds that "the mathematical processes and trains of reasoning in one science resemble those in another so much that [...] knowledge of the one science may be made a most useful help in the study of the other." Hence, the mathematical forms of the relations of the quantities are found to be the same in these systems of quantities, while the physical nature of the quantities may be utterly different. One is thus led to recognize a classification of quantities on a new principle, according to which the physical nature of the quantity is subordinated to its mathematical form. However, this form succeeds its physical aspect and does not precede it, because, as Maxwell explained, the human mind, in order to conceive of different kinds of quantities, must have them presented to it in the first place by nature.[5]

Maxwell considered the scientific work of Faraday the source of this methodology whose traces can also be found in Hertz's (Misc 316f.). Faraday began by getting rid of parasitical ideas. He therefore endeavored to strip all such terms as "electric fluid," "current," and "attraction" of every meaning except that which is warranted by the phenomena themselves, and to invent new terms, such as "electrolysis," "electrode," and "dielectric," which suggest no other meaning than that assigned to them by their definitions. The result was, according to Maxwell, the remodeling of the whole according to an entirely new method.

Consider the popular phrase "electric fluid"; it has done what it could to keep men's minds fixed upon those particular parts of bodies where the "fluid" was supposed to exist. Faraday, by contrast, invented the word "dielectric," and redirected the research towards the examination of all that is going on in the air or other medium between the electrified bodies. Hertz indeed realized that the most important result of his experiments on electric waves, what he called the "philosophic result of the experiments," was precisely that the experimental proof includes "a recognition of the fact that the electric forces can disentangle themselves from material bodies, and can continue to subsist as conditions or changes in the state of space" (EW 19).

Consider further Faraday's concept "lines of force": it furnishes a method of building up an exact mental image of the thing we are reasoning about. The way in which Faraday made use of his idea of lines of force in co-ordinating the phenomena of magneto-electric induction shows him, as Maxwell remarked, to have been in reality a mathematician of a very high order. I suggest that Hertz's concepts of "concealed masses" and "concealed motions" are intended to play similar roles in mechanics as those concepts of Faraday in electricity. With the concepts of concealed elements, Hertz hoped to obtain a new coherent picture of phenomena which would overcome the logical and empirical difficulties of the classical, action-at-a-distance, picture. For example, he remarked, "in Electromagnetics we are almost convinced that the mutual action between moving magnets is not in all cases strictly subject to the principle [of reaction]." With respect to both form and content, Newton's third law, Hertz argued, exhibited grave shortcomings on the "action-at-a distance" mode of interpretation (PM§§469, 470). Like the idea of "lines of force," so the idea of "concealed masses" would, Hertz believed, co-ordinate the phenomena in a new fashion, leading to new discoveries.

Maxwell summarized Faraday's methodology thus: "We have, first, the careful observation of selected phenomena, then the examination of the received ideas, and the formation, when necessary, of new ideas; and, lastly, the invention of scientific terms adapted for the discussion of the phenomena in the light of the new ideas." One may recall here that Hertz's book, *Electric Waves*, opens with the experimental part and continues to the theoretical. According to Maxwell, "the advance of the exact sciences depends upon the discovery and development of appropriate and exact ideas, by means of which we may form a mental representation of the facts, sufficiently general, on the one hand, to stand for any particular case, and sufficiently exact, on the other, to warrant the deductions we may draw from them by the application of mathematical reasoning."[6]

I suggest that the *Principles of Mechanics* exhibits these characteristics. Hertz sought in the *Principles* to trace back "the supposed actions-at-a-distance to motions in an all-pervading medium whose smallest parts are subjected to rigid connections" (PM 41). He regarded the concept of force as logically obscure and belonging to the superseded physics of action at a distance. The concept of force therefore does not appear as one of the fundamental concepts.

Hertz expected the decision between the different formulations of mechanics to be associated with this problem. He probably discerned here an analogy between the state of electricity and that of mechanics: just as he had helped decide between rival electrodynamic theories, he could do the same for mechanics. "Absolute clarity," to use Hertz's own words, was of paramount importance to the success of this project (Jungnickel and McCormmach 1986, Vol. II, 142).

The first part of the *Principles* treats geometry and kinematics and is "completely independent of experience" (PM §1). It is concerned solely with statements about the paths and connections of material particles that satisfy the demands of thought. Hence the use of definitions and propositions. The second part treats mechanics proper and appeals to experience through the "fundamental law" of mechanics, which is a statement about the path followed by a free system (PM §309).

Now, consider by way of comparison, Maxwell's study of the motion of an incompressible fluid. He writes:

> The substance here treated must not be assumed to possess any of the properties of ordinary fluids except those of freedom of motion and resistance of compression. It is not even a hypothetical fluid which is introduced to explain actual phenomena. It is merely a collection of imaginary properties which may be employed for establishing certain theorems in pure mathematics in a way more intelligible to many minds and more applicable to physical problems than that in which algebraic symbols alone are used. The use of the word "Fluid" will not lead us into error, if we remember that it denotes a purely imaginary substance with the following property: *The portion of fluid which at any instant occupied a given volume, will at any succeeding instant occupy an equal volume.*[7]

There is no need to dwell upon this point – the methodological similarity between Maxwell and Hertz is clearly displayed.

It appears then that most of the methodological and theoretical elements in Hertz's physics may be traced back to several sources spread throughout the century. Indeed, this fact affected the reception of Hertz's electrical wave experiments, especially in England. Heaviside, for example, wrote to Hertz that he had learned something from the experiments about resonators for detecting electric waves, and that he thought the experiments served a purpose in persuading people to give up untenable theories, especially the German electrodynamic theories. However, for his part, he had expounded Maxwell's theory since 1882 and had long been theoretically convinced of the existence of electric waves in dielectrics "in spite of the absence of experimental evidence." He explained that one "who goes by hardheaded reasoning of a legitimate nature, on the basis of laws known with great exactness, does not want an experimental proof." But he assured Hertz that his experiments were "highly appreciated in England," even if he did not need them himself. Hertz replied to Heaviside in 1889 that he had been in earnest when he had said that he did not expect Heaviside to learn much from the experiments. For whoever "was fully convinced of the truth of Maxwell's equations and was able to interpret them, did know as much about these things before my experiments as after them" (Jungnickel and McCormmach 1986, Vol. II, 90). The following passage is characteristic of this situation:

> The properties of the electromagnetic medium are [...] as far as we have gone similar to those of the luminiferous medium, but the best way to compare them is to determine the velocity with which an electromagnetic disturbance would be propagated through the medium. If this should be equal to the velocity of light, we would have strong reason to believe that the two media, occupying as they do the same space, are really identical. The data for making the calculation are furnished by the experiments made in order to compare the electromagnetic with the electrostatic system of units. The velocity of propagation of an electromagnetic disturbance in air, as calculated from different sets of data, does not differ more from the velocity of light in air, as determined by different observers, than the several calculated values of these quantities differed among each other.

This claim is not Hertz's; this is once again Maxwell.[8] It should be stressed, however, that Maxwell used indirect experimental evidence for the proof of the claim that electric waves and light are of a similar nature. Maxwell, it transpires, was not interested in direct experimental demonstrations of the propagation of electromagnetic effects. It is likely that he regarded his inquiries throughout as an investigation of the nature of light. His concern in experimental physics was not with electric and magnetic phenomena for their own interest, but as clues to the nature of light and of the light-bearing medium. It has been convincingly argued elsewhere that all the experimental elements for the direct demonstration of electromagnetic propagation in space were available to Maxwell, but his interest lay somewhere else (Simpson 1966, 430). Hertz's greatness therefore lies in redirecting the search by casting all the available elements of propagation phenomena into one coherent whole. This was definitely not the case with Rutherford.

RUTHERFORD AND THE TWENTIETH CENTURY PHYSICS OF STRUCTURE

In his essay on Faraday, Maxwell remarked, "We are probably ignorant even of the name of the science which will be developed out of the material we are now collecting, when the great philosopher next after Faraday makes his appearance."[9] We are now in a position to name this science: it is atomic and nuclear physics; and the great philosopher, the Faraday of our time, as Kapitza called him, is none other than Rutherford.

Rutherford's first publication in Cambridge, shortly after his arrival there, concerned a magnetic detector of electric waves which he had developed back in New Zealand. Under the influence of Hertz's experimental demonstration of the propagation of electric waves, Rutherford sought other and more efficient means of demonstrating this propagation. He delivered his paper on June 18, 1896.[10] Some six months later he reported on a new set of experiments: "On the Passage of Electricity through Gases Exposed to Röntgen Rays."[11] This paper marks the beginning of an epoch, the beginning of atomic physics. Together with J.J. Thomson, he demonstrated that the newly discovered X-rays, when sent through a gas such as air, cause the formation of ions and thus render the gas a conductor. Studies of this ionizing property would lead Rutherford to the discoveries of what he called alpha, beta and gamma radiations, to a theory of radioactivity, the statistical description of rates of decay, and then further to transmutation, and ultimately to the structure of the atom and the discovery of the nucleus.

At a particular stage of the development of science, when new fundamental concepts have to be found, wide erudition and conventional training are not the most important characteristics of a scientist seeking solutions for such problems. It appears that in this case qualities such as imagination, very concrete thinking, and most of all, daring are needed. Strict logical thinking, which is so necessary in mathematics and in analyzing physical theories, hinders the imagination of a scientist when new fundamental concepts must be found. The ability to solve such scientific problems without showing a logical trend of thought is none other than intuition. Rutherford, according to Kapitza, mastered this ability. Studying the works of Rutherford and observing how he worked, Kapitza came to the conclusion that the basic characteristics of Rutherford's thinking were great independence and daring.[12]

Consider for example Rutherford's ideas of the "disintegration of matter" and the "planetary model of the atom." Both conceptions seemed at first sight to contradict the most fundamental laws of nature: the law of conservation of energy and the laws of classical electrodynamics respectively. But these ideas provided at once not only the key to the understanding of radiation phenomena but also led all investigations in the right direction. Rutherford imagined collisions and disintegrations of particles so vividly and concretely that even the contradictions with the fundamental laws could not prevent him from establishing the structure of the atom.[13]

New knowledge of the structure of matter has been obtained throughout the present century not by the invention of new experimental possibilities of investigating nuclear phenomena but through the possibility of investigating nucleus collisions of a *larger* number of elements. These collisions are studied in the domain of higher energies which are reached by the use of powerful modern machines. But that method, namely, the scattering experiment, is the very method Rutherford had discovered early in the century. He was the first to appreciate its fundamental value. Rutherford's battle cry: "Smash the atom!" has reverberated throughout twentieth century physics.

The reader should note the ingenuity of this method of experimentation which appears to have originally no connection whatsoever to a guiding theory. It is, in other words, pure intuition. Indeed, even to Rutherford it came as a surprise that significant results had been obtained. Marsden told the story that Rutherford had instructed him to see if one could get some effect of alpha particles directly reflected from a metal surface. Marsden thought that Rutherford had not really expected any such result; it was one of those "hunches." In a lecture given many years later, Rutherford confessed openly to his complete amazement at the positive result which Geiger and Marsden had obtained: the back-scattering effect. A small fraction of the swift alpha particles from radioactive substances were deflected through an angle of more than 90 degrees. "It was quite the most incredible event that has ever happened to me in my life," Rutherford intimated. "It was almost as incredible as if you fired a 15-inch shell at a piece of tissue paper and it came back and hit you."[14]

Rutherford stressed the importance of the study of the passage of the high speed alpha particles through matter as a means of throwing light on the internal structure of the atom. He drew attention to this remarkable fact of back-scattering and showed that the type of atom devised by Lord Kelvin and worked out in great detail by J.J. Thomson would not produce such large deflections unless the diameter of the positive sphere was exceedingly small. In order to account for this large angle scattering of alpha particles, he supposed that the atom consists of a positively charged nucleus of small dimensions in which practically all the mass of the atom was concentrated. The nucleus was supposed to be surrounded by a distribution of electrons to make the atom electrically neutral, and extending to distances from the nucleus comparable with the ordinary accepted radius of the atom.[15]

Rutherford's ability to seize on one definite fact, to realize that *independently* of all other arguments it showed a fundamental error in the current physical picture, and then to follow this trail wherever it led, this ability was the measure of his genius. The real achievement was the realization of the experimental fact and the insistence on its importance.[16]

With the scattering experiment, Rutherford posed a clear question to nature, not realizing how pertinent it was. It was a question outside the realm of the rational – this is the mark of intuition which no methodology, theory or philosophy can initiate. However, it is definitely a question related to the study of structure and one should see the origin of this ingenious method in this context. For Rutherford the development of modern physics was closely connected with the problem of the structure of the chemical atom. On the one hand, he observed, the examination of radioactive phenomena had thrown much light on the processes of the transformation of heavy atoms, and the products of their disintegration. On the other hand, a study of the effects produced by the new types of penetrating radiation in their passage through matter had yielded information of great importance in regard to the structure of the atoms themselves.[17]

Throughout his life Rutherford viewed with considerable impatience the growing tendency to replace simple theoretical models or pictures and their experimental testing by an elaborate analysis and procedure designed to be as much as possible independent of all preconceived ideas. He believed that this tended to introduce unnecessary philosophical problems and difficulties. By direct experimental attack, Rutherford was able quickly to outline a simple picture concerning the new radiations. Here is a fine example of such a picture:

> We can thus form the following picture of the emission of an alpha particle from a radioactive element. Occasionally one of the neutral alpha particles which are probably circulating in quantized orbits is for some cause displaced from its position of equilibrium and has sufficient energy of motion to escape from the attractive field of the central nucleus. When the field falls below the critical value, the neutralizing electrons are removed and fall back towards the nucleus. The alpha particle, which has now two positive charges, gains additional energy in passing through the repulsive electric field of the nucleus and emerges as a high-speed alpha particle.[18]

Notice the clarity and the direct use of simple language. What characterizes this kind of picture building is the stress put on fundamental principles *established* by experiments and to achieve the ability to apply these principles and to reason about them. We see here Rutherford's gift of seeing the essential *physics* in any problem.

Ellis reported of his experience with Rutherford:

> [H]e had such a simple outlook, *so unbiased by current theoretical ideas*, that it was difficult to appreciate immediately all that he meant just because it was so direct and so simple. He did not think logically, it would be far more just to say that he had an artistic feeling of the way nature works. It was always so clear to him what was the next thing to do that he neither would give reasons for it nor felt the need to do so, he did not in fact appear to be greatly interested in what current theory might have to say for or against his ideas. To the end he was unappreciative of quantum mechanics, and had little use for the wave picture. Particles were particles and that was the end of it.[19]

Rutherford felt compelled to do theoretical physics if, and only if, he otherwise could not interpret data of his own or from his laboratory. General theoretical issues tended to be alien to him especially if they were speculative in character. Thus he was distinctly reserved in his early response to Bohr's quantum theory of the atom. The quantum theory was somewhat remote from Rutherford's inborn way of visualizing everything. Still, it was he who communicated in 1913 to the *Philosophical Magazine* Bohr's celebrated serial publication on the constitution of atoms. Bohr opened his first paper thus: "In order to explain the results of experiments on scattering of alpha rays by matter Professor Rutherford has given a theory of the structure of atoms. [...] Great interest is to be attributed to this atom-model."[20]

Rutherford made it clear that he preferred the theory to follow the experiment and not *vice versa*. In 1933 he made a characteristic comment on theoretical physics when he responded to Blackett's discovery of the positron. "It seems to me," he remarked, "that in some way it is regrettable that we had a theory of the positive electron before the beginning of the experiments. Blackett did everything possible not to be influenced by the theory, but the way of anticipating results must inevitably be influenced to some extent by the theory. I would have liked it better if the theory had arrived after the experimental facts had been established."[21]

However, when it came to experimenting, Rutherford usually made all the relevant calculations in advance and he could thus detect instantly the first indication that something unexpected was happening. He was the complete antithesis of the man who observes first and then goes home to work up the measurements to find out what had happened.

Rutherford had that deep insight which told him when a reasonable experiment was really only a side line; when a certain measurement, though important, could yet afford to be left a while; and when, although good experiments were waiting, they were not really progressive and it was the moment to abandon old methods and develop new technique.[22]

CONCLUSION

There emerges from the juxtaposition between Hertz and Rutherford a clear distinction between two different modes of experimentation. Hertz succeeded in proving experimentally the correctness of Maxwell's theory; he thereby eliminated from the scene several other theories. He executed his experimental work always within a rich theoretical background concerned with propagation phenomena. This multitude of theories and the establishment of crucial experimental facts constituted the experience out of which Hertz, I claim, forged his philosophical position. But Hertz's rich and insightful philosophical lesson did not impress Rutherford. Rutherford started his career with an improvement on Hertz's detector but soon afterwards changed his tack. He realized that the future of physics at that time lay not in propagation phenomena but rather in the question of structure – an entirely new field without experimental foundation and above all with no theory to guide the researcher. The establishment of experimental facts *independent* of theories is of paramount importance in this situation. Rutherford was the right man at the right time. Pure intuition carried the day.

I end on a Maxwellian note which captures succinctly the difficulties involved in doing experiments:

It is not till we attempt to bring the theoretical part of our training into contact with the practical that we begin to experience the full effect of what Faraday had called "mental inertia" – not only the difficulty of recognizing, among the concrete objects before us, the abstract relation which we have learned from books, but the distracting pain of wrenching the mind away from the symbols to the objects, and from the objects back to the symbols [...][23]

For Hertz symbols became eventually to consist of regulative principles, but for Rutherford they were pictures of physical content and form. Thus emerges an interesting historical parallel, given the fact that the current symmetry principles which are believed to govern the world of elementary particles – the building blocks of matter – are essentially regulative principles. The new physics which Rutherford inaugurated at the beginning of this century has eventually developed in much the same way as the physics of nineteenth century: from physical content and form to regulative principles. Hertz's *philosophical* teaching has not been lost.

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NOTES

[1] Leon Rosenfeld, "The Wave-Particle Dilemma," in Jagdish Mehra (ed.), *The Physicist's Conception of Nature* (Dordrecht: Reidel, 1987), pp. 251–263, cf. p. 261.
[2] *Cf.* Giora Hon, "The Unnatural Nature of the Laws of Nature: Symmetry and Asymmetry," in Steven French and Harmke Kamminga (eds.), *Correspondence, Invariance, and Heuristics* (Dordrecht: Kluwer, 1993), pp. 171–187, esp. pp. 176f.
[3] James Clerk Maxwell, *The Scientific Papers of James Clerk Maxwell*, W.D. Niven (ed.) (New York: Dover, 1965), 2 vols., vol. I, pp. 155f.
[4] *Ibid.*, p. 156.
[5] *Op. cit.* (note 3), vol. II, p. 218.
[6] *Ibid.*, pp. 358–360.
[7] *Op. cit.* (note 3), vol. I, p. 160; the emphasis is Maxwell's.
[8] *Op. cit.* (note 3), vol. II, p. 771.
[9] *Ibid.*, p. 360.
[10] Ernest Rutherford, *The Collected Papers of Lord Rutherford of Nelson* (London: Allen and Unwin, 1962), 3 vols., vol. I, pp. 80ff.
[11] *Ibid.*, pp. 105ff.
[12] P.L. Kapitza, "Recollections of Lord Rutherford," in Jagdish Mehra (ed.), *op. cit.* (note 1), pp. 749–765, esp. pp. 750–753.
[13] *Ibid.*, p. 754.
[14] Abraham Pais, *Inward Bound* (New York: Oxford University Press, 1986), p. 186.
[15] *Op. cit.* (note 10), vol. II, p. 423.
[16] C.D. Ellis, "The Cavendish Chair," *Proceedings of the Physical Society, London* **50** (1938), pp. 463–466, esp. p. 465.
[17] *Op. cit.* (note 10), vol. II, pp. 448f.
[18] *Op. cit.* (note 10), vol. III, pp. 178f.
[19] *Op. cit.* (note 16), p. 466; emphasis added.
[20] Niels Bohr, *Collected Works: Volume 2* (Amsterdam: North-Holland, 1981), p. 161.
[21] *Op. cit.* (note 14), pp. 190, 363.
[22] *Op. cit.* (note 16), p. 464.
[23] *Op. cit.* (note 3), vol. II, p. 248.

MANUEL G. DONCEL[1]

ON HERTZ'S CONCEPTUAL CONVERSION: FROM WIRE WAVES TO AIR WAVES

WHAT KIND OF CONVERSION?

During November, 1887, in his Karlsruhe laboratory, Heinrich Hertz observed for the first time "wire waves," that is, regular alternating currents with a very high frequency in conductive wires. These were the only electric waves he had yet detected. He described them to his master Hermann von Helmholtz in Berlin:

> In the meantime I have succeeded in several further experiments. By means of the oscillations I used in my previous work I am now able to produce standing waves with many nodes in straight stretched wires. If I content myself with 4 to 5 nodes, I can make them almost as clearly visible *as the nodes of a vibrating string*. (Letter of December 8th, 1887; MLD 239, emphasis added).

Today we know these "further experiments" in great detail from Hertz's *Laboratory Notes* (Hertz and Doncel 1995). On Saturday, November 5th, Hertz sent for publication in *Akademieberichte* his paper proving the existence of "polarization currents" in insulators (our "displacement currents"), by detecting their electrodynamic actions (henceforth №6_{Ak}).[2] On Monday, November 7th, he was observing those "stationary oscillations ... in straight stretched wires, throughout the entire laboratory" (Hertz and Doncel 1995, 235; cf. MLD 235). The experimental details were visualized in a figure sketched by Hertz on the next day, November 8th, just a month before his communication to Helmholtz (figure 1 reproduces the English redrawn version from Hertz and Doncel 1995).

In the foreground of figure 1 we can see the flat oscillator used by Hertz for the electrodynamic balance of his "previous work" (see the figure in №6_{Ak} or in EW 97). Over one of its 40 × 40 centimeter terminal plates is a similar forward plate, which is connected to straight stretched wire of about 10 meters and, through it, to another similar backward plate. Owing to simple electrostatic induction from the oscillator plate, the forward plate will be alternately positively and then negatively charged. Opposite charges will run away through the wire in the form of alternating currents of the same very high frequency as the oscillator (about 100 megahertz). Thus, after a very short delay, the backward plate will also be charged and will originate (as if it were a mirror for the current) a similar return current, which will thus arrive at the forward plate with the same delay. By regulating the length of the wire, the round-trip delay can be made to coincide with an exact multiple of the period of oscillation, so that the whole process will be maintained stationary. When this is successfully accomplished the wire will show clear nodal points, at which the

D. Baird et al. (eds.), Heinrich Hertz: Classical Physicist, Modern Philosopher, 73–87.

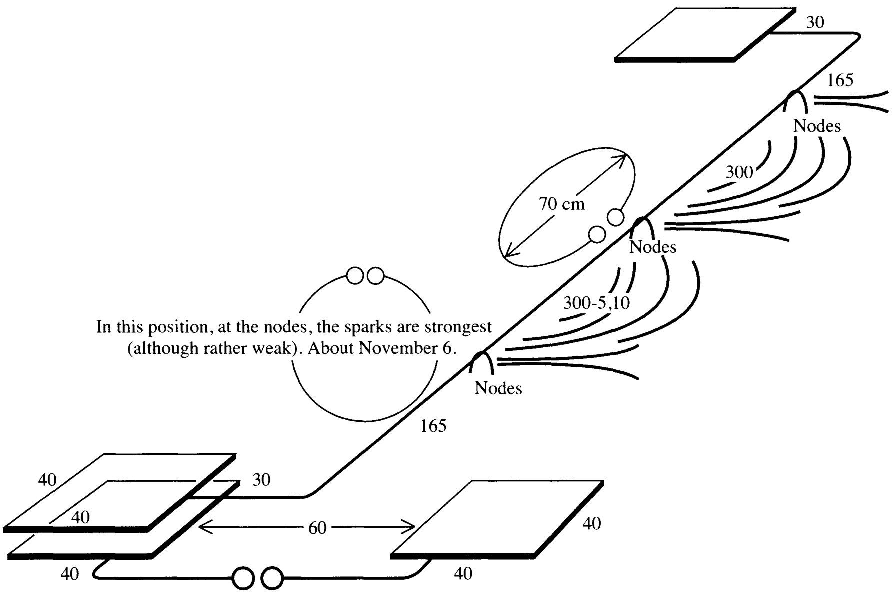

Figure 1. Stationary waves in wires, observed by Hertz on November 8, 1887. (Redrawn version of Hertz's own figure, in Hertz and Doncel 1995, 235.)

forward and backward currents exactly and constantly counterbalance each other. Hertz detected three such nodes, by studying the sparks induced in his resonator, when it was approached in a position discovered on "about November 6th." He marked these nodes with small riding forks. They were for him "as clearly visible as the nodes of a vibrating string." With this experiment Hertz demonstrated the existence of standing waves in the wire, and directly measured their length.

Note that such wire waves consist in oscillations of the electric charges, or oscillations of the intensity of the electric current. These concepts were familiar to contemporary electrodynamicists. Hertz's contribution thus far had been to increase the frequency while maintaining the regularity of these oscillations. He did this by means of his spark oscillator. Note also that such waves are longitudinal and monodimensional: current intensity and wave propagation are in the same direction, that fixed by the wire.

Four months later, in March 1888 Hertz was able to demonstrate the wave nature of something new, his recently conjectured "air waves," or what we call "Hertzian waves." At this point he wrote Helmholtz as follows:

> Electrodynamical waves in air are reflected from solid conducting walls; at normal incidence the reflected waves interfere with the incident and give rise to standing waves in the air. In the first wavelength in front of the wall the phenomena are very pronounced and manifold, and I believe that *the wave nature of sound* in empty air space cannot be demonstrated so clearly as the wave nature of this electrodynamic propagation. (Letter of March 19th, 1888; MLD 255, emphasis added).

We do not have the detailed information about these experiments which Hertz's *Laboratory Notes* provide for the earlier experiments; the *Laboratory Notes* only cover the second half of 1887. But we know the exact dates of Hertz's epic manipulation of these first 10-meter air waves: removing all hanging gas candelabra and tubes from the main lecture hall, walking in the dark down a corridor of classroom tables, contorting himself so his body does not perturb the waves... (Diary of March 12th and 15th, and letter of March 17th; MLD 252–5; EW 125–6). The scientific presentation of these experiments was made in paper №8 (EW 124–36), sent for publication on April 1st, 1888, (see also the covering letter to Wiedemann in Deutsches Museum, HN 3229). The figure in this paper shows these standing waves, with their nodes and the positions of the resonator detecting them, very clearly.

But of what do these air waves consist? What oscillates? This was so new for Hertz, that the first times that he described it, he did not even dare to use the same verb, "to oscillate" [*schwingen*], used for the wire waves; instead he introduced a new expression, "to fluctuate up and down," "to fluctuate to and fro" [*schwanken auf und ab*, №8, EW 126, there translated by "to oscillate up and down"; *schwanken hin und her,* №5, of February 17th, EW 82, translated simply by "to vary"]. What it is that fluctuates Hertz called successively (in paper №8): "inductive action," "electrodynamic action" and "electric force" [*Inductionswirkung, elektrodynamische Wirkung* and *electrische Kraft*]. We note that these air waves are transverse; this is clearly shown in the figure of paper №8: the direction in which the electric force fluctuates or oscillates is perpendicular to the direction of the wave propagation.[3] At this point, to study the standing waves, Hertz considered only the air waves in the direction normal to the reflecting wall, but of course they are tridimensional.

From these two letters to Helmholtz, we can see Hertz's conceptual shift, from the wire waves of November 1887 to the air waves of March 1888. The transformation is analogous to the shift from tasting a vibrating violin string to hearing its sound waves in air. Here is a graphic way to visualize Hertz's conceptual conversion.[4] This is the conversion from "electrodynamicist" to "field-theoretician." Because, so to speak, electrodynamicists would never think of sound waves propagating to our ears. They would have the vibration of the violin string exciting immediately our acoustical nerve, acting at a distance on our hearing. Most of the physicists in continental Europe at the time were then dazzled with the lucidity of the electrodynamic views of Weber and Helmholtz. In 1846, Wilhelm Eduard Weber had formulated his "universal fundamental law of the electric actions":[5]

$$F = \frac{ee'}{r^2}\left[1 - \frac{1}{c^2}\left(\frac{dr}{dt}\right)^2 + \frac{2r}{c^2}\frac{d^2r}{dt^2}\right].$$

The first term is the Coulomb electrostatic force, while the last two terms give the Ampère electrodynamic force between current elements, applying Weber's model for the current as motion of positive and negative charges in opposite directions. Both kinds of force were conceived from an action-at-a-distance point of view in the purest Newtonian tradition. Hermann von Helmholtz, in 1870, generalized this expression of the electrodynamic force, with his curious inter-theoretical formula,

Table

Nº6_{Ak}	Ueber Inductionserscheinungen, hervorgerufen durch die elektrischen Vorgänge in Isolatoren (On Electromagnetic Effects Produced by Electrical Disturbances in Insulators)	submitted for publication in *Akademieberichte* November 5th, 1887; read at Berlin Academy, November 10th.
Nº7_{Ak}	Ueber die Ausbreitungsgeschwindigkeit der elektrodynamischen Wirkungen (On the Finite Velocity of Propagation of Electromagnetic Actions)	submitted for publication in *Akademieberichte* January 21st, 1888; read at Berlin Academy, February 2nd.
Nº5	Ueber die Einwirkung einer geradlinigen elektrischen Schwingung auf eine benachbarte Strombahn (On the Action of a Rectilinear Electric Oscillation Upon a Neighboring Circuit)	submitted for publication in *Wiedemanns Annalen* February 17th, 1888 (chapter 5 in EW)
Nº6	Ueber Inductionserscheinungen, hervorgerufen durch die elektrischen Vorgänge in Isolatoren (On Electromagnetic Effects Produced by Electrical Disturbances in Insulators)	revision of Nº6_{Ak} submitted along with Nº5 February 17th, 1888 (chapter 6 in EW)
Nº7	Ueber die Ausbreitungsgeschwindigkeit der elektrodynamischen Wirkungen (On the Finite Velocity of Propagation of Electromagnetic Actions)	revision of Nº7_{Ak} submitted along with Nºs 5 and 6 February 17th, 1888 (chapter 7 in EW)
Nº8	Ueber elektrodynamische Wellen im Luftraume und deren Reflexion (On Electromagnetic Waves in Air and Their Reflection)	submitted to *Wiedemanns Annalen* April 1st,1888 (chapter 8 in EW)
Nº9	Die Kräfte elektrischer Schwingungen, behandelt nach der Maxwell'schen Theorie (The Forces of Electric Oscillations, Treated According to Maxwell's Theory)	submitted to *Wiedemanns Annalen* End of October, 1888 (chapter 9 in EW)
Nº11	Ueber Strahlen elekrischer Kraft (On Electric Radiation)	submitted to *Akademieberichte* December 11th; read at Berlin Academy, December 13th. With figures and their explanation submitted to *Wiedemanns Annalen*, January 31st, 1889; (chapter 11 in EW)

Hertz's publications in chronological order (Nºs 5 to 7 are referred to as the 'Conversion Trilogy') which was supposed to reproduce the theories of Weber, Franz Neumann and Maxwell, by giving convenient values to a parameter k in the formula. But this reinterpretation transformed Maxwell's electromagnetic theory into a limiting case of electrodynamics.[6]

After the diffusion of Hertz's experiments on air waves, the genuine field-theoretical view of Faraday and Maxwell was slowly accepted by the continental physicists. But, as I show below, Hertz did not approach these experiments as proofs of Maxwell's theory. Rather, he was driven to the discovery of air waves by the necessary electrodynamic conceptual framework underlying his experiments, and, by the problems of his master Helmholtz's theory. Ultimately Hertz came to understand his air waves through Maxwell's theory. Thus, these months of Hertz's conceptual conversion anticipate "ontogenetically" the "phylogenetic" slow evolution of the physical community. Hence they are of paramount interest to analyze in detail, using all available primary sources.[7]

THE THREE STEPS OF THE CONVERSION

In my analysis of Hertz's conversion I distinguish three conceptual steps that correspond roughly to the original versions of papers №6–8. The first step assures the existence of electrodynamic effects ("displacement currents") in insulators; these experiments were described in paper $№6_{Ak}$ (sent for publication on November 5th, 1887). The second step assures a finite velocity for the propagation of the electrodynamic action, from the comparison with the propagation of waves in wires as presented in paper $№7_{Ak}$ (sent on January 21st, 1888). The third conceptual step assures the existence of electrodynamic waves in air, or in empty space; the idea of such waves follows logically from the preceding step, but their existence was only proved by Hertz's epic experiments on stationary air-waves, as we know from the second letter to Helmholtz; this was presented in paper №8 (sent on April 1st, 1888).

Note that the wire waves we know from the first letter to Helmholtz are the proper starting point for the second step. But the first step is important, both for understanding the roots which fed Hertz's research, and for understanding the consequences of this research. Note also that Hertz's conceptual progress cannot be understood if one starts from paper №5. We know that this paper was written as the first of a trilogy of papers, №5–7, sent for publication in the *Wiedemann's Annalen* on February 17th, 1888 (see Doncel 1991, 1–6). Thus, from the date it was edited, it clearly belongs to the third step. As a matter of fact, the paper contains two figures which illustrate the standing mean direction of the electric force all around the oscillator, anticipating waves in air. The second of these figures can be precisely dated from the *Laboratory Notes*, it represents the experiments of December 29th, 1887. This figure, and its explanation (chiefly on some mysterious points in space where the direction of the electric force could not be determined, see points marked * in figs. 2–3 below), clearly belong, as we will see, to the starting point of the third conceptual step.

Let us consider, then, the three steps in order (see Doncel 1991, 7–24). The first step assured the existence of electrodynamic effects in insulators – in Maxwellian terms, "displacement currents." Nevertheless, it would be improper to search for the roots of Hertz's research in Maxwell. It is clear that the historical origin of this research is the prize proposed by the Berlin Academy in 1879. Moreover, the context in which Helmholtz proposed this prize was his universal electrodynamic theory of 1870 – prior to Maxwell's 1873 *Treatise*. It is well known that Hertz wrote a paper in the summer of 1879 investigating the possibility of experimentally solving the question posed for the Academy prize. The original manuscript has been found and partially published (O'Hara and Pricha 1987, 122–128).

The complete text of this manuscript of 1879 is in preparation.[8] The conceptual analysis in this publication is of great importance. It will establish definitely the connection, on the one hand, between Hertz's view of matters in 1879 with Helmholtz's view in his 1870 paper, and, on the other hand, with Hertz's view of matters as he came to the experiments of 1887. On this last point, and based only on a cursory look at this manuscript, I can only guess that some experiments of the "second kind" show a close kinship with those of the first "fruitless endeavours" described by Hertz in the Introduction of the *Electric Waves* (see Doncel 1991, note 21). A figure in folio 8 of the manuscript, which Hertz numerated "1)," shows a capacitor of sorts connected to an induction coil with a thin bar between the capacitor plates, and near to both of them. According to the text, this bar could be of a conducting or of a dielectric material, the effect of the latter being weaker, "but qualitatively very similar," to that of the former. If we look at Figure 1 in the introductory chapter №1 (EW 5) a close kinship is readily apparent. The main difference is that the new figure shows a spark-gap, between the connections of the induction coil. This allowed Hertz to increase the frequency of the discharge circuit by a factor of 10,000 compared to that obtained from the best induction coils, or at least by a factor of 100 compared to the discharge of Leyden jars. But, as Hertz tells us, even with so high a frequency, the experiments were "fruitless."

Hertz had more success using the electrodynamical balance described and depicted in paper №6 (EW 97). This clever device was conceived and built during the month of September 1887; it first produced "clear results" on October 5th, after the birth of Hertz's first daughter, Johanna (see MLD 229–31, and the annotations in Hertz and Doncel 1995, 233). On November 5th Hertz sent paper $№6_{Ak}$ to Helmholtz. In it are clear allusions to the old prize of the Academy (see the cover letter in MLD 233–35, and the note in Hertz and Doncel 1995, 235). But his conviction of the existence of currents in dielectrics did not force any conceptual conversion. Such polarization effects were required in Helmholtz's general electrodynamics, and they also could be well understood from Weber's formula, as action at a distance onto and from the charges inside the insulator. Nevertheless, this first step is suggestive to an electrodynamicist to look, not only at charges and currents in circuits, but also at a new kind of electrical polarization which changes and propagates in three-dimensional dielectrics.

Much more decisive, however, is the second step, which assures a finite velocity for the propagation of this electrodynamic action. Still, there remains some distance from the goal of the full conceptual conversion to Hertzian waves. When we look at the figure in chapter №7 of the *Electric Waves* (108), we see immediately both wire waves and parallel air waves of the same frequency leaving the oscillator. By carefully positioning the resonator one could analyze the interference of their actions at different distances of the oscillator and with different lengths of wire inserted to delay the wire waves; this can be seen in the different tables in the paper. We can understand how to use these "interference-lengths" to measure the relative speed of both waves. If, for example, the speeds were equal, the interference would be identical all along the wire; if, on the other hand, the speed of the air waves was infinite, the "interference-length" would reproduce the wire waves. Nevertheless, Hertz, in paper $№7_{Ak}$ (sent to the *Akademieberichte* on January 21st) does not speak of air waves, but only of wire waves. Of course, the "concluding section" of the final version of the paper speaks explicitly of "electrodynamic transverse waves in air" (see its §3 in EW 123). But Hertz added this section on February 17th together with many small corrections, while preparing the paper for publication as №7 of the Trilogy (see Doncel 1991, note 9).

From Hertz's *Laboratory Notes*, we know a great detail about the stages of this second conceptual step (19 of their 23 folios are dedicated to it). The starting point is the series of experiments, starting on November 7th, which led to Hertz's observing of standing waves in wires, upon which I have already commented. From the *Memoirs*, we know of Hertz's excitement about these "new experiments." Typical here is the delightful letter of November 9th, in which Hertz's wife, Elisabeth, substituted for Hertz in giving news to his parents. She described Heins as a magician who "simply pulls these beautiful things out of his sleeve now!"; she confessed her deep sharing in his happiness, "when he tells me about it with a radiant face." Hertz himself wrote the following Sunday, November 13th:

> This week I have again had good luck with my experiments... I must be quite spoiled if I do not take sincere pleasure in what has been actually accomplished..., prospects are opening right and left for new, interesting experiments... (MLD 235–37).

At the end of this first week, Hertz began experimentally to probe the main aspects of the second conceptual step. By lengthening his straight wire to about 60 meters (through a window and over the gardens of the Technische Hochschule finishing with an earth connection), he suppressed the reflections in his standing-waves experiment; thus, he had at his disposal progressive wire-waves, of a known length, which he could use to measure the speed of the "direct action" of the oscillator. But from November 11th until December 22nd Hertz consistently obtained results which implied that the direct action of the oscillator was much faster than that of the wire waves; apparently its speed was infinite (see diary of November 12th, 17th, 18th, and 26th, and the letters of December 8th and 23rd in MLD, 235–41). Note that these results excluded any kind of waves in air, at all.

The situation changed on the evening of December 23rd. Hertz was explicit in a letter to his parents, "I received a great gift in my work on the night before Christmas," i.e., the night before Christmas Eve (letter of December 26th, MLD

241). From the *Laboratory Notes* we now know in detail the novelty of December 23rd, and how he established by the 27th of December the finite velocity for the electrodynamic action.

On December 22nd Hertz had started a new series of experiments, "more precise than previously," in which he systematically varied, half meter by half meter, both the inserted length of wire and the distance of the resonator to the oscillator. On the first day he only had time to perform measurements for five different lengths of inserted wire.[9] In this way, he obtained five measurements of the interference half-length, which happened to be very short (they practically corresponded to the first half period of oscillation, in which the air wave is not yet formed). Hertz computed the mean value for these five half-lengths of interference to be 3.1 meters, this was compatible with the half wavelength of 3 meters measured in the previous experiment of the standing wire-waves. Thus, it seemed, the interferences reproduced the wire waves, and thereby confirmed the infinite speed of the direct action. But on December 23rd he finished his measurements and from fourteen half-lengths of interference he obtained a new mean value of 3.4 meters. This no longer was compatible with the half wave length in wire; it required a finite velocity for the direct action.

On December 23rd Hertz also began a new series of experiments. In this series he set the resonator in a new position which he thought would be insensible to electrostatic action which dominates the electrodynamic action over short distances. In this way, he obtained a longer interference half-length, and a clear finite speed for the direct electrodynamic action.[10] Such results were confirmed after Christmas on December 26th and 27th. That allowed him to publish paper №7_{Ak}, in which the finite "speed of electrodynamic actions" was justified without any explicit mention of air waves.[11] The final paragraph of this paper, under the heading "The electrostatic forces," explicitly states that the velocity of these forces "remains for the present unknown," and that its value should be different from that for the electrodynamic action, and could be infinite. (In №7 the text substantially remains, but the scandalous heading was suppressed, see EW 121).

The third step of Hertz's conceptual conversion is the explicit conviction of the existence of waves in air (or, what for him was the same, in empty space). Logically, it is tied to the preceding step: When the speed of the propagation of the direct action was taken to be infinite, waves in air were unthinkable. But, given a finite speed, it becomes difficult to interpret such interference experiments without conjecturing some kind of air waves. As a matter of fact, for interpreting the experiments and deriving a first (absurd!) comparison of the velocities, Hertz, in the *Laboratory Notes* for December 27th, coins the names of "induction wave" [*Inductionswelle*] and "air wave" [*Luftwelle*; see Hertz and Doncel 1995, 260]. In the printed paper, №7_{Ak}, such explicit terms were not introduced, instead Hertz used once the idea of "wavelength of the electrodynamic action in air" (see EW 121).

Note that the air waves, referred to implicitly in this paper, are monodimensional, parallel to the wire waves. But we know from the *Laboratory Notes*, that on December 29th Hertz made a bidimensional study of the electrodynamic

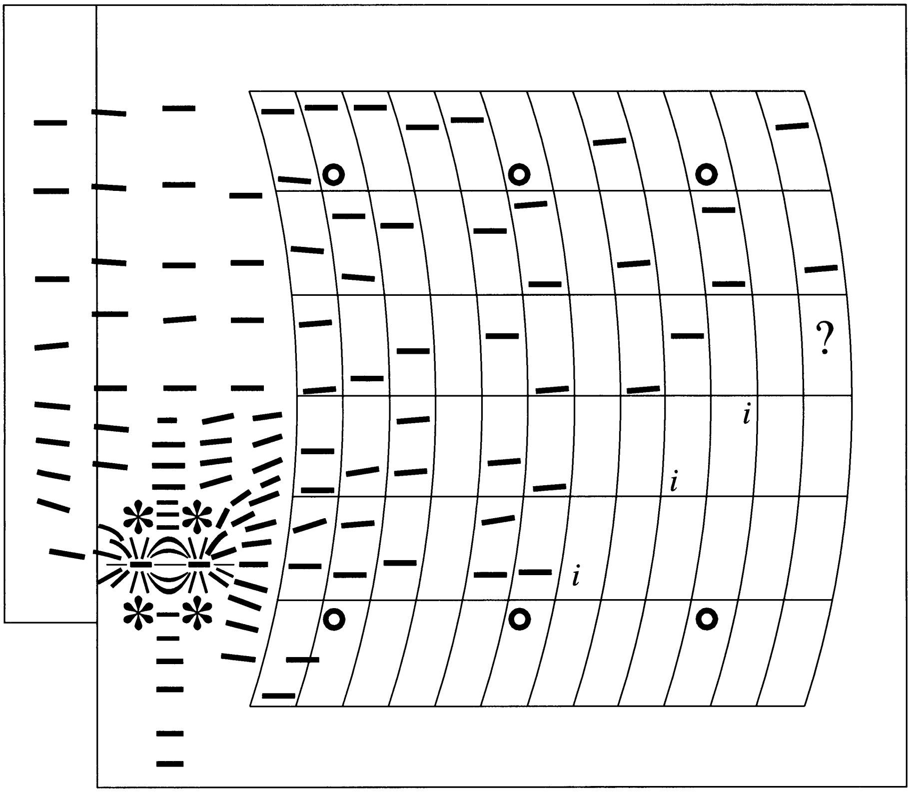

Figure 2. Propagation of the inductive action "in all directions through the entire lecture hall" observed by Hertz on December 29th, 1887. The lines indicate the main direction of the electric force. They sketch some steady air waves coming out from the oscillator, which is sketched at the lower left corner. The student benches and the six columns of the lecture hall can be seen. (Redrawn version of Hertz's own figure, in Hertz and Doncel 1995, 267.)

action "in all directions throughout the entire lecture hall," and illustrated it in two marvelous figures.[12] Significantly, with his resonator Hertz had experimentally detected only the steady mean-orientation of the electric field at a set of space points along the horizontal meridian plane of the oscillator. This he depicted in figure 2. But, in order to explain what happened at four points where the mean-orientation was undecided, Hertz conjectured that they are "points with rotating force," i.e., that at these points the electric force "during each oscillation passes through all points of the compass" (EW 122). Hertz annotated this conjecture in figure 3. And, he immediately generalized it for all the other points in which a mean-orientation was well defined. In this way Hertz first imagined a whole dynamism of air waves.

Again on December 29th Hertz was surprised by the "shielding effect" and the "reflection from the wall" of these waves. These wave characteristics of the electrodynamic action were first publicly described in the Trilogy of papers №5–7 (sent for publication on February 17th), mainly in the introductory new paper №5 and in

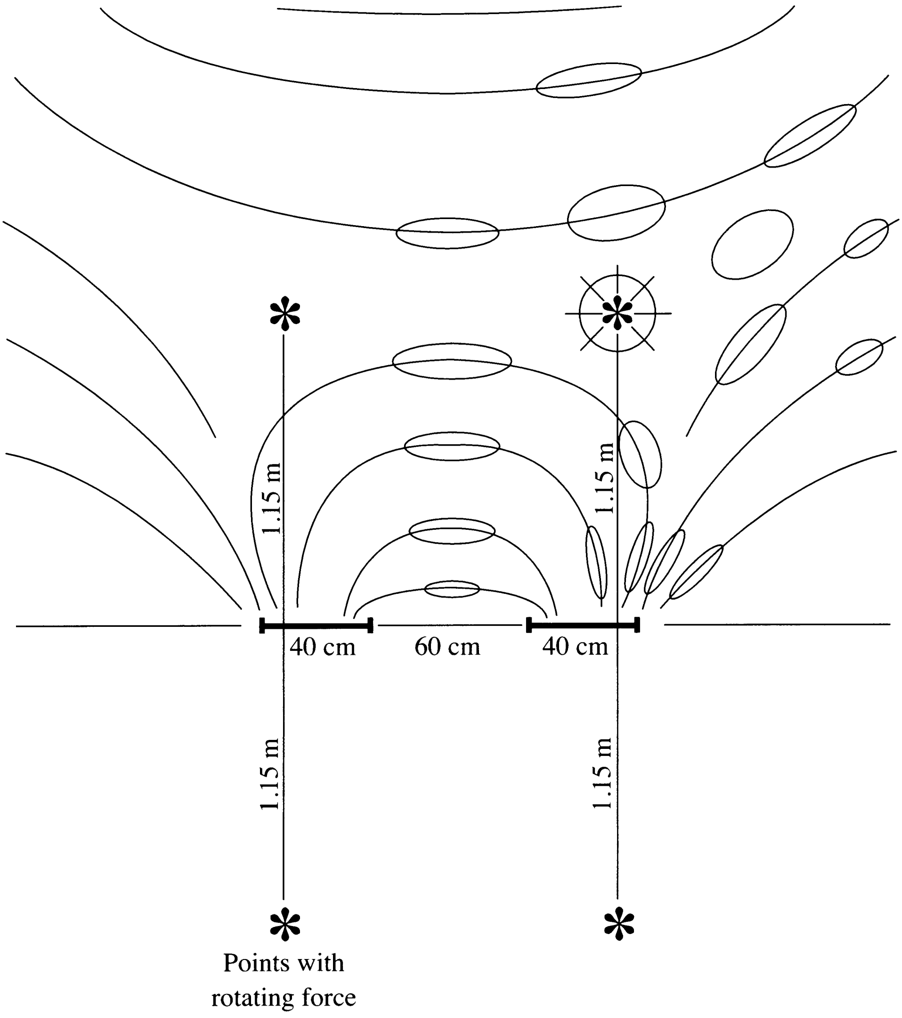

Figure 3. Detail of the preceding figure around the oscillator. The four points with undecided mean direction for the electric force are here interpreted as "points with rotating force," i.e., with a force turning round itself with constant intensity. The ellipses in other points indicate similarly the variation of the electric force in direction and intensity during a period. That was for Hertz a first dynamical view of the wave field. (Redrawn version of Hertz's own figure, in Hertz and Doncel 1995, 269.)

the new conclusions added to papers $N^{\underline{o}}6_{Ak}$ and $N^{\underline{o}}7_{Ak}$. But the experimental proof of these air waves was first made in March, by the detection of stationary air-waves. The proof was inspired by the reflections Hertz had observed from the lecture hall walls. These positive results were communicated to Helmholtz in the letter of March 19th quoted above, and were scientifically described in paper $N^{\underline{o}}8$ (sent to *Wiedemanns Annalen* on April 1st). There he writes of "electrodynamic waves in the air" in the title, and the magnetic forces and waves just appear in the figure and its explanation.

This experiment was nearly the culmination of the third step of conceptual conversion. At this point Hertz had "the comfortable feeling" of being on his "own ground and territory... alone with nature...," without worrying "about human opinions, views and demands," sure that then "the philological impetus ceases and only the philosophical remains" (letter to his parents of March 17th, MLD 255). Other physicists will now have to worry about the philological impetus of the amount of Hertz's papers!

Still it would be only after his complete conversion, with the decision to use Maxwell's equations to calculate the form of the waves produced by his oscillator, that Hertz would fully understand his air waves. For example, the form of his first waves around the oscillator allowed Hertz to reinterpret these anomalies previously related to an electrostatic force which, in Maxwellian terms, could not be distinguished from the electrodynamic force (see paper №9, EW 137–159).

Simple experiments with rays of air-waves proved the optical properties of air waves. They force the identification of electromagnetic air-waves with light, but of a million of times shorter wavelength (see paper №11, EW 172–185). This made it clear to the whole scientific community that, in the same way as one affirms the difference between light and its source, one must likewise affirm the existence of air-waves apart from their oscillator.

PREVIOUS, FOREIGN AND SUBCONSCIOUS IDEAS OF HERTZ

In the preceding paragraphs, I have tried to trace the process of Hertz's conversion, from his initial Helmholtzian view of the Academy prize to a final Maxwellian view where he treats his electromagnetic waves on a par with light. I have substantiated my description of Hertz's conversion by exhibiting Hertz's own expressions in published or private documents. In fairness, I mention here some older, implicit or occasional expressions, which seem to complicate this linear process, mainly by projecting Maxwellian wave conceptions at the beginning of the process. Nevertheless, I believe that such understandings, if present at all, belong to another foreign and subconscious level of Hertz's thinking. They show the complexity of human feeling and thinking. They do not undermine the validity of my description of Hertz's conversion.

Consider first Hertz's 1884 paper, "On the relations between Maxwell's Fundamental Electromagnetic Equations and the Fundamental Equations of the Opposing Electromagnetics" (Misc 273–290). He wrote this paper while *Privatdozent* in Kiel, after months of "meditating about electromagnetic rays" and the "electromagnetic theory of light" (see the diary of January 27th and 29th, and of March 13th to June 28th in MLD 191–93). The paper demonstrates Hertz's knowledge of and ability to manipulate Maxwell's equations, which he writes (for the first time!) treating the electric and magnetic fields symmetrically. He obtains these equations from very general principles, including the uniqueness of electric forces and the parallel uniqueness of magnetic forces. This paper, with its Maxwellian orientation poses a serious problem for understanding Hertz's position on these matters at the time of his 1887 experiments. The fact that this paper was never cited in the *Electric Waves*, together with the fact that Hertz attributes to Heaviside priority for using the symmetrical form of Maxwell's equations, has brought some scholars to the conclusion that Hertz suffered amnesia about his 1884 paper (D'Agostino 1975, 295).

Much could be said about the state of Hertz's views in 1884 regarding Maxwell's and Helmholtz's approaches to electrodynamics. To my mind, one point is clear: the viewpoint of a single electric force, basic to the arguments of the 1884 paper, is completely excluded in the experiments of 1887. The electrostatic action (as opposed to the electrodynamic one) should have been, and was, explicitly excluded in Hertz's experiment on the polarization currents in isolators, as discussed in paper №6_{Ak}. Furthermore, it could not be – and was not – included in the assertion of a finite speed of propagation, as stated in paper №7_{Ak} under the special heading previously mentioned. Finally as the title and the introduction of paper №8 makes clear, Hertz was thinking only of inductive or electrodynamic waves and actions.[13] The unified terminology "electric force" in parallel with "magnetic force" appeared first in paper №8; here it is used to explain both kinds of standing waves. It is fully justified in Hertz's introduction of Maxwell's equations at the beginning of paper №9. That, however, clearly is after Hertz's conversion.

I now consider a sentence Hertz wrote obviously previous to his conversion, where he speaks of air waves in his description of the oscillator (№2, EW 29–53). The sentence is in the last section, titled "Theoretical," in which he tries to compute the period of his oscillator from Thomson's formula. He first computes a length which, when divided by the speed of light, first gives the period. Hertz then comments:

> This is the distance through which light travels in the time of an oscillation, and is at the same time the wave-length of the electrodynamic waves, which the *Maxwellian* view presupposes as outwards action of the oscillations. (EW 51)[14]

Clearly, at the time of these experiments, Hertz knew of Maxwell's views, in particular, of air waves. On the other hand, it is clear that he mentions these ideas as foreign things. They are different from the proper discourse of his paper and the experiments he relates in it.

During that critical time, in which his interference experiments seemed to give an infinite speed for the "direct action," Hertz expressed in his diary and letters a certain displeasure. At this point in his "thoughts about Maxwell's theory,"[15] Hertz could not believe it was promising, in spite of the fact that he had called it "the most promising electrical theories."[16] One might conclude from this that Hertz then tried to prove Maxwell's theory by finding waves in air. But, he is explicit in the first of these letters that by "the most promising electrical theories" he refers to those mentioned at the beginning of paper №6_{Ak} on polarization currents in insulators. We know that those allude at least as much to Helmholtz's theory as they do to Maxwell's theory. As a matter of fact, for Hertz, no logical proof could decide in favor of Maxwell's theory as compared with Helmholtz's; Hertz understood Maxwell's theory as a limiting case of Helmholtz's. Even computing the evolution of electromagnetic waves out from the oscillator, by means of Maxwell equations, would not be a proof of Maxwell (see the last lines of paper №9, EW 159). For Hertz those two theories were two different presentations [*Darstellungen*] of the same conception [*Vorstellung*] (see the theoretical part of the Introduction to *Electric Waves*). Hertz's choice of presentation went in Maxwell's direction, and even overtook it. In the end, the choice rested upon the principle of simplicity and the artificiality of Helmholtz's Maxwellian limit. For me, this choice also belongs to the described process of conversion.

Finally consider a French review-paper written by Hertz in February and March of 1889, and published in April in the *Archives de Genève* under the title "Researches on the Electric Undulations" (see the diary of February 20th and March 1st to 6th in MLD 283 and Hertz 1889). This paper preserves the fresh triumphant air of Hertz's last days in Karlsruhe (on his last triumphal lectures, see the letters of February 28th and March 10th; MLD 283–85). Hertz vividly presents the experiments which had shown successively oscillations in conductors, in dielectrics and in air.

In the introduction of this paper, Hertz alludes to Maxwell's unifying theory of 1865; furthermore, Hertz writes that "nevertheless a direct proof inferred from sure experiments was still missing." He, Hertz, will present his spark oscillator and its "electric vibrations rapid enough to obtain a proof of Maxwell's hypotheses," so that "those hypotheses were found fully confirmed."[17] Such an introduction seems to present Hertz's experiments, from the discovery of the oscillator on, as an effort to prove Maxwell's old theory (previous to Helmholtz's 1870 reinterpretation). I do not believe this paper by Hertz undermines my interpretation of his process of conversion. Rather, this paper should be understood in the context of Hertz's connection to the Maxwellians. In September, 1888, Hertz knew of FitzGerald's lecture at the British Association for the Advancement of Science at Bath; here FitzGerald presented Hertz's experiments as the decisive proof of Maxwell's proposal of contiguous action. After the opening of 1889, this lecture prompted correspondence between Hertz and the English Maxwellians, a correspondence that was especially lively in February, and which culminated with Hertz's journey to England in November-December 1890.[18] It is understandable that, in the euphoric moment of realizing the paramount importance of his work in this respect, and for a publication abroad, Hertz would write this introduction emphasizing this aspect of his work. What Hertz wrote in February, 1892, about his Introduction to *Electric Waves* completely agrees with what he wrote to his mentor at about the same time:

> ...my work derives not merely from the direct study of Maxwell's works, as I am constantly told, but rather essentially from the study of the works of Your Excellency [Helmholtz], and that the original impetus even came from your personal suggestion. (Letter of February 24th, 1892; MLD 321)

Hertz is not merely flattering Helmholtz, but presenting a sincere version of the development of his thought.

CONCLUSIONS

My thesis is this: Hertz's explicit idea of air waves arrived at the last step of a three-step conceptual conversion. The main reasons in support of this thesis are:

a. The starting point of Hertz's experimental research was rooted in the Academy prize of 1879, and therefore it was conceived in clear Helmholtzian terms.

b. No air waves could honestly be thought to be real before the experimental proof that their speed of propagation was finite, and consequently, not before December 23th, 1887.

c. Even after experimental proof of a finite speed for the electrodynamic action (with doubts on the electrostatic one), Hertz did not reach the full idea of electromagnetic waves. A further experimental and theoretical study remained necessary.

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NOTES

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2 I refer to Hertz's published papers by their chapter-numbers in *Electric Waves*. The index Ak labels the publishing version of some of them, published in the *Akademieberichte* in Berlin. The chronological order of their publication is Nº6_{Ak}, Nº7_{Ak}, Nº5–7 (the so called Conversion Trilogy in EW 80–123), Nº8 (EW 124–136), Nº9 (EW 137–159), Nº11 (EW 172–185), see Doncel 1991 and Table here on p. 76.

3 It is noteworthy how difficult it was to understand this point. So conspicuous a physicist as William Thomson (Lord Kelvin) is a case in point. Being the inspirer of Maxwell's mathematical formulation of the electromagnetic field theory, he could not accept his electromagnetic explanation of the transverse oscillation of light, not even after writing the preface to the English translation of Hertz's *Electric Waves* (see Smith and Wise 1989, 458–482).

4 Put aside the fact that string vibrations are transverse and sound waves longitudinal.

5 This expression gives the intensity, F, of the central force between two charges, e, and e', as a function of their distance, r, and its time derivatives: their relative velocity, dr/dt, and acceleration, d^2r/dt^2. The speed of light, c, appears here only as the relation between the electrostatic and the electrodynamic units.

6 Helmholtz studied the action of his electrodynamic force on a polarizable dielectric: different longitudinal and transverse waves were produced as a function of the value of k. Consequently, Helmholtz could claim to cover Maxwell's theory as a limiting case of his own, where these were only transverse waves (the speed of the longitudinal waves coming out to be infinite).

7 These include (a) Hertz's published papers (see note 2 above); (b) Hertz's short diary and letters to his parents and to Helmholtz, published by his daughter in *Memoirs, Letters, Diaries* (very few other interesting letters of this time are preserved in the Deutsches Museum, Munich); (c) the recently discovered *Laboratory Notes*, or *Protokolle*, written by Hertz mainly in the crucial period November–December 1887 (Hertz and Doncel 1995); and (d) the historical reconstructions by Hertz: the introductory chapter 1 to *Electric Waves*, written in December 1891 for the *Untersuchungen*, and "Recherches sur les Ondulations Électriques," written in March 1889 for the *Archives de Genève*.

8 Buchwald and O'Hara in preparation. See also O'Hara 1988b.

9 These lengths were 100, 200, 250, 400 and 500 cm; see Hertz and Doncel 1995, 251–257, and its final Table I, which was successively published in papers Nº7_{Ak} and Nº7, and in EW 118. See, for this analysis, Doncel and Roqué 1990, 184–185, or Doncel 1991, figures 1 and 2.

10 See experiments 50–52 in Hertz and Doncel 1995, 259. These further data are tabulated in the first line of Table II on page 261, which corresponds to the first half line of the upper table in EW 120.

[11] The paper was sent to the *Akademieberichte* on January 21st, after the efforts of checking and editing the whole; see letters of January 1st and 15th in MLD 247–51.
[12] Hertz and Doncel 1995, 263–265 for the text and 267–269 for the figures; they are reproduced here (Figures 2 and 3) from their English redrawn version; previously we knew only the vague sentence in the diary of December 30th, MLD 247.
[13] Note that the translation of both *Inductions-* and *elektrodynamisch* by "electromagnetic" in *Electric Waves* even in the titles of these three papers, confuses matters almost beyond hope.
[14] "*...es ist zugleich die Wellenlänge der elektrodynamischen Wellen, welche die Maxwell'sche Anschauung als Wirkung der Schwingungen nach aussen voraussetzt.*" I have translated literally, retaining the original emphasis.
[15] "*Überlegungen über die Maxwellsche Theorie.*"
[16] "*Die aussichtsvollsten elektrischen Theorien*"; see diary of November 18th and letters of December 23rd, 1887 and January 1st, 1888 in MLD 239, 245 and 249.
[17] "*...une preuve directe déduite d'expériences sûres manquait cependant encore...,*" "*...vibrations électriques assez rapides pour en tirer une preuve des hypothèses de Maxwell. Ces hypothèses se trouvèrent pleinement confirmées.*" See Hertz 1889, 282–283.
[18] See diary of September 12th, 1888 and February 7th, 1889, and letter of December 5th, 1990 in MLD 259, 279 and 307–9; see also O'Hara and Pricha 1987.

SALVO D'AGOSTINO

HERTZ'S VIEWS ON THE METHODS OF PHYSICS: EXPERIMENT AND THEORY RECONCILED?

In this paper I argue that Hertz's epistemology in *The Principles of Mechanics* represents an attempt to generalize his earlier reflections on the relation between his experiments and the then current electrodynamic theories. These reflections appear in his collected papers on his discovery of electric waves (EW).

I find that his detailed discussion of the reasons for his preference for his own representation [*Darstellung*] of Maxwell's theory over that of Helmholtz has to be read in parallel with his discussion, in his Introduction to the *Principles,* of his preference for a space-time-mass (plus hidden quantities) conception of mechanics over Newtonian-Laplacian mechanics and Energeticism.

Hertz's contributions offer a privileged standpoint for an inquiry into the historical development of theoretical physics at the beginning of one of the most fecund periods for German and European physics. His achievements as both an experimentalist and a theoretician are especially suited for an inquiry into the burning question of the theory–experiment relationship in theoretical physics. Because of his almost unique position in the history of physics, Heinrich Hertz deserves special attention.

In my study, I am concerned with a critical analysis of some aspects of Hertz's epistemology. In the concluding paragraphs, I present an overview of Boltzmann's reaction to Hertz's positions because I consider this reaction to be one of the major pieces of evidence that Hertz's views on the theory–experiment relationship did not convince many of his contemporary physicists. I attempt an explanation of this situation from our modern point of view.

MAXWELL'S EQUATIONS ARE "THE COMMON SIGNIFICANCE OF DIFFERENT MODES OF REPRESENTATION OF MAXWELL'S THEORY"

Ever since its appearance, Hertz's celebrated dictum "Maxwell's theory is Maxwell's system of equations," has prompted comments from philosophers and historians of science. Most of them[1] do not give due consideration to the fact that Hertz's dictum is part of a whole passage dealing with a new significance for the term 'theory.' I argue that, in order to locate the meaning of Hertz's sentence properly, it must be inserted into the context of his *Bild*-conception of physical theory. I think the following interpretation is reasonable: although it is true that Maxwell's system of equations is identified with Maxwell's theory, it should also be noted that this system of equations is characterized as the *common and inner significance* [*gemeinsame Inhalt*] of a plurality of modes of representation [*Formen von*

D. Baird et al. (eds.), Heinrich Hertz: Classical Physicist, Modern Philosopher, 89–102.

Darstellungen] of the same theory. Hertz's conception of a theory's representation is then part of the whole picture; Hertz's dictum, taken in its context, appears as a statement which concludes the premise above on the system's significance within a mode of representation, and cannot be separated from it without distorting its meaning. The passage runs as follows:

[T]he representation [*Darstellung*] of the theory in Maxwell's own work, its representation as a limiting case of Helmholtz's theory, and its representation in the present dissertations [i.e., Hertz's theoretical papers] – however different in form – have substantially the same inner significance. *This common significance of the different modes of representation* [*Formen von Darstellungen*] (and others can certainly be found) appears to me to be the undying part of Maxwell's work. This, and not Maxwell's peculiar conceptions or methods, would I designate as "Maxwell's theory". To the question, "What is Maxwell's theory?" I know of no shorter or more definite answer than the following: Maxwell's theory is Maxwell's system of equations. (EW 21, emphasis added)

The equations represent, so to speak, the minimum denominator of the various *Formen von Darstellungen*. This point is again emphasized in the continuation of the passage above:

Every theory which leads to the same system of equations, and therefore comprises the same possible phenomena, I would consider as being a form or special case [*eine Form oder einen Specialfall*] of Maxwell's theory; every theory which leads to different equations and therefore to different possible phenomena, is a different theory. *Hence in this sense, and in this sense only*, may the two theoretical dissertations in the present volume be regarded as representations of Maxwell's theory. (EW 21, emphasis added)

As is clear from the above, Hertz's dictum is to be interpreted in the restrictive sense, implying that Maxwell's theory can be identified with the equations insofar as the equations are the common significance of the various modes of representation. The equations, being the common denominator of the various representations of Maxwell's theory, carry with themselves the property of defining the sense in which a theory is Maxwellian. Actually, for Hertz the term theory comes to have two rather distinct meanings: its meaning in the sense of the *Bild*-conception[2] of theory is not the same as that in Hertz's dictum above (theory = equations). Perhaps it would help to use two typographically distinct expressions, e.g. to use a capitalized 'Theory' to indicate the upper level in the *Bild*-conception (Theory = mathematics + *Darstellung*), and to reserve the lower case 'theory' for indicating what is common to different *Darstellungen*. To be consistent with the above proposal, Hertz's famous dictum should be written: *Maxwell's theory is Maxwell's system of equations*.

Through his *Bild*-conception, Hertz proposed a more comprehensive conception of the status of a physical Theory: by stating that the same empirical bases can afford different *modes of representation* and that some of them may have in common the same equations, he introduced a binary aspect into the Theory. On one side lie *the modes of representation* and, on the other, *the mathematics of the fundamental equations.* (This is also a definition of *fundamental equations*). However, the two components do not have an equal epistemological status. The fundamental equations, in their formal role, alone have perennial validity. However, only when these equations are interpreted through a *Darstellung* (mode of representation) do they become empirically meaningful, i.e. linked to the experimental basis. This linkage requires a *Darstellung*. Let us re-read Hertz's former passage in which interpreted equations are linked to the phenomenal basis:

> Every theory which leads to the same system of equations, *and therefore comprises the same possible phenomena*, I would consider as being a *special form* of Maxwell's theory; every theory which leads to different equations, and therefore to different *possible phenomena*, is a different theory. (EW 21, emphasis added)

Notice that, in the passage above, the system of equations is linked to a special form (i.e. to a form of representation) of Maxwell's theory. That means that a mode of representation linked to a set of equations constitutes the *physical content* (Hertz: *possible phenomena*) of the Theory. However, this linkage is diachronically and synchronically mutable. As stated above, different representations have been linked in the past and are linkable at present to the same equations; this is the case with Maxwell's equations, which can be linked with different *Darstellungen*. Only in this role of common denominators, i.e. in their formal role of uninterpreted symbols,[3] do the fundamental equations have perennial validity as "the undying part of the theory." Though reduced to a shadow, to a formal role, mathematics is promoted by Hertz to a highly significant role in physics.

In the same way that Maxwell's equations are the common significance of the various modes, the fundamental law of mechanics,[4] i.e. Lagrange's equations,[5] are common to various modes of representation and symbolize, at best, the axiomatic structure of mechanics.

In his Introduction to the *Principles*, Hertz lists various forms of representation of the principles of mechanics. He justifies his preference for a space-time-mass form of representation (*Darstellung*) over that of the Energeticists and over the common Newtonian-Laplacian forms of representation by listing the necessary requirements for an adequate theory: permissibility [*Zulässigkeit*], correctness [*Richtigkeit*] and appropriateness [*Zweckmässigkeit*]:

> By varying the choice of the propositions which we take as fundamental, we can give various representations of the principles of mechanics. Hence we can obtain various *images of things*; and these images we can test and compare with each other in respect of *permissibility, correctness and appropriateness*. (PM 4, emphasis added)

These requirements are essentially a generalization of those previously expounded in his Introduction to *Electric Waves*. In fact, when he prescribed his *Zulässigkeit* requirement for theory in the *Principles*, he analyzed a relationship of order and clarity similar to that described in his *Electric Waves*:

> Ideas and conceptions which are akin and yet different may be symbolized in the same way in the different modes of representation... [and in order to have] a proper comprehension of any one of these [representations] the first essential is that we should endeavor to understand each representation by itself without introducing into it ideas which belong to another. (EW 21)

This demand for order and clarity presented in *Electric Waves* can only be satisfied by axiomatic analysis, precisely the method that Hertz took up again in his *Principles.* There he defined the principles of mechanics as any selection of propositions such that "the whole of mechanics can be developed from it by purely deductive reasoning without any further appeal to experience" (PM 4).

I find that Hertz's interpretation of the meaning of Maxwell's equations in his famous dictum "Maxwell's theory is Maxwell's system of equations," is remarkably similar to his ideas on the *Principles of Mechanics* cited above.

HERTZ'S *BILD*-CONCEPTION OF THEORIES: DIFFERENT MODES OF REPRESENTATION (*FORMEN VON DARSTELLUNG*) OF A THEORY

In many of his passages Hertz stated that there exist different modes of representation [*Formen von Darstellung*] of his experiments dealing with the propagation of electric force [*Ausbreitung von elektrischer Kraft*] and with the related Maxwell theory. In his considerations in *Electric Waves*, Hertz presents three different modes of Maxwell's Theory: those found (1) in Maxwell's own work, (2) in Helmholtz's representation as a limiting case of Helmholtz's own theory, (3) in Hertz's two theoretical dissertations in his *Electric Waves*. According to Hertz, these three different modes of representation have in common (a) the same phenomena, i.e. the same empirical referent and (b) the same equations, i.e. the mathematics.

Hertz claims that his representation of Maxwell's theory is different from Maxwell's own representation. In fact, concerning his two theoretical dissertations, he remarked:

In no sense can they [the two theoretical dissertations in the present volume] claim to be a precise rendering of Maxwell's ideas. On the contrary, it is doubtful whether Maxwell, were he alive, would acknowledge them as representing his own views in all respects. (EW 21)

Notice that Hertz emphasizes the point that his representation in the two theoretical dissertations differs from Maxwell's own representation. I think that this emphasis was motivated by the experience with the theoretical and experimental process which led to the discovery of electric waves. In fact, the revelation that a new *Darstellung*, in contrast to the traditional but well established *Darstellung* of electricity and magnetism, was conceivable, and that it was presented by Faraday's and Maxwell's theories, suddenly struck Hertz during his December, 1887 experiments (Doncel 1991). In his own words:

But while I was at work it struck me that the center of interest of the new theory did not lie in the consequences of the first two hypotheses. (EW 7)

Elsewhere, I analyzed in some detail my interpretation of this revelation (D'Agostino 1993b, 51–56); here I present but a summary. I argue that Hertz was struck by a sudden understanding that his experiment could be much better explained through a new conception of polarizability; instead of the Helmholtzian view of polarizability as an induced charge (Poisson-type polarizability), he took ether polarizability as a primitive concept in his theory (EW 200, 210, 25; D'Agostino 1993b, 51). Since Poisson polarizability was the effect of an induced force acting-at-a-distance, Hertz's bold conception amounted to the abolition of the static electric force, a precedent for the complete abolition in his *Principles* of the old-established concept of force. Given these premises, Hertz's shift in 1887–88 from *Darstellung* (2) to (3) can be adequately described as a thematic change. I argue that, on this occasion, Hertz experienced *in vivo*, i.e. in the concrete process of discovery, how various modes of representation, widely different though they were, could all account more or less satisfactorily for the same set of phenomena. However mode (3), besides being the more adequate for explaining the experiments, was also distinguishable for its internal consistency, its logical permissibility. Hertz chose this *Darstellung* (i.e. his fourth stand-point in *Electric Waves*) as his own representation of Maxwell's theory.

Let me remark that Hertz's *Bild*-conception contradicted Helmholtz's and Kirchhoff's phenomenological conceptions both of which supported an indistinguishable unity of the mathematical apparatus with the physical content of theories (Chevalley 1991, 345 ff., 348; D'Agostino 1993b, 66).

In contrast to Helmholtz, Hertz found that there exists a multiplicity of representations consistent with the Helmholtzian requirement of a parallelism between concepts and perceptions (Helmholtz 1977, 122); i.e. for Hertz, Helmholtz's requirement does not unambiguously determine the choice of an appropriate theory:

> The images [*Bilder*] which we may form of things are not determined without ambiguity by the requirement that the consequents of the images must be the images of the consequents. Various images of the same objects are possible, and these images may differ in various respects. (PM 2)

Helmholtz's parallelism of laws not only does not suffice to determine the most appropriate theory, but does not even work if theory is limited to visible quantities (D'Agostino 1993b, 66). Only the introduction of hidden quantities allows parallelism to reach the status of a general principle:

> If we try to understand the motions of bodies around us, and to refer them to simple and clear rules, paying attention only to what can be *directly observed* [*was wir unmittelbar vor Augen haben*], our attempt will in general fail. We soon become aware that *the totality of things visible and tangible* do not form a universe conformable to law, in which the same results always follow from the same conditions. (PM 25, emphasis added)

Also relevant for my thesis is the fact that in the *Principles* Hertz refers to Maxwell's electromagnetism in support of his conception of concealed masses:

> Through Maxwell's labours the supposition that electro-magnetic forces are due to the motion of concealed masses has become almost a conviction. (PM 26)

A final point: In his 1890 theoretical papers Hertz referred to Heaviside as the first scientist who removed from Maxwell's theory the same symbol – I argue, the vector potential – that he, Hertz, removed (EW 196; D'Agostino 1975, 295); this

1890 rejection of the vector potential is echoed by his rejection of potential energy in the *Principles*.

HERTZ'S *BILD*: A BINARY CONCEPTION OF PHYSICAL THEORY

I argue that Hertz was led to distinguish between the mathematical uninterpreted form and the physical content of theory during the course of his attempts to interpret Maxwell's theory. His first attempt, in 1884, was highly regarded by no less than Max Planck (Planck 1894, 183) and yet has only recently received its due attention from historians[6] (D'Agostino 1975, 286–292; 1993b, 48; Cazenobe 1980; Darrigol 1993a). In his 1884 theoretical paper, Hertz derived Maxwell's equations from two formal (physically empty) principles: the independent existence of forces in space and the uniqueness of forces (Misc 273–290). For lack of a representation of the same forces by the state of a physical system (such as Helmholtz's polarization), the two principles were formal, i.e., rather empty of physical content and meaning. In 1888, the two formal 1884 principles[7] received physical content through Hertz's dielectric action and ether-polarization conceptions. Thus his 1884 derivation might have represented for him, by 1888, just a pure logical form, conforming to the permissibility requirement in his *Principles*.

As to the reasons why Hertz neglected to mention his 1884 essay in the following years, many theses have recently been advanced. I think I have shown that, although there was no explicit mention, Hertz returned to the basic 1884 concepts of the independent existence and uniqueness of forces in his 1888 and 1890 papers (D'Agostino 1993b, 50). In my view, Hertz's quite different 1887–88 approach (via polarizability) can be taken as a sufficient reason for his neglect. In his initial 1887 approach to his experiments, he was helped by Helmholtz's polarization theory (Helmholtz 1882, 3: 545–628, 798–822) in as much as it represented a tentative physical content for his 1884 formal theory. But, he soon abandoned it once he had succeeded in introducing an innovative interpretation of the independent existence principle through his new conception of ether polarization as a primitive concept of his theory (not, as he had thought before, a Maxwellian concept derived from primary charge and induction).

Hertz's derivation of Maxwell's equations in the 1884 paper shows that Maxwell's theory in that special form did not contradict traditional electrodynamics, i.e., in our terminology, the latter was in the correspondence area of the former. Of course, we can find flaws and even mistakes here and there in Hertz's 1884 derivation,[8] but this fact does not provide the basis for a valid historiographical argument that Hertz's neglect of this derivation was due to such flaws and mistakes.[9] Rather the evidence points to the fact that he simply changed his approach, not that he was then aware of the errors that, in hindsight, we now find in it.

Hertz's progression from a rather formal theory to a theory fuller in physical content may represent the experiential background for his binary *Bild*-conception of theory.

If mathematics seems to be privileged in its formal role of being the *common significance of the various modes of representation*, it is also clear that this role has strong limits: the similarity in mathematical structure can disguise deep differences in forms of representation (Heidelberger, this volume), i. e. in the physical content of two competing theories:

Considered from the mathematical point of view, this fourth mode of treatment may be regarded as coinciding completely with the limiting case of the third. But from the physical point of view the two differ fundamentally. [...] Now this fourth standpoint, in my opinion, is Maxwell's standpoint. (EW 25, 26)

The uninterpreted mathematics, like Kant's *Kategorien*, is certain and perennial but purely formal, i.e. empty of a physical content. Conversely, *modes (Formen) of representation* may vary when we pass from one theory to the other; as such, they are ephemeral. However, by providing an interpretation to mathematics they carry with them the physical content of a theory. In this role, *modes of representation* are indispensable components of Theory.

A HOLISTIC VIEW OF THE THEORY–EXPERIMENT CORRELATION

Hertz's *Bild*-conception excludes any kind of inductive term-to-term correlation between the empirical basis and the theoretical constructions. He clearly excluded this correlation in his 1889 essay "The Forces of Electric Oscillations, Treated According to Maxwell's Theory," when, presenting Maxwell's equations in ether as "the essential parts of Maxwell's theory," he denied that their correctness could be tested through observations:

These statements form, as far as the ether is concerned, the essential parts of Maxwell's theory. Maxwell arrived at them by starting with the idea of action-at-a-distance and attributing to the ether the property of a highly polarisable dielectric medium. We can also arrive at them in other ways. *But in no way can a direct proof [for each] of these equations be deduced from experience [Auf keinem Wege kann indessen bislang ein direkter Beweis für jene Gleichungen aus der Erfahrung erbracht werden]*. (EW 138, emphasis added)

For Hertz, just the opposite was true. A *fact* is deduced from a theory. In his "On the Fundamental Equations of Electrodynamics for Bodies at Rest" (1890), he writes:

I state in what manner the facts which are directly observed can be systematically deduced from the formulae; and, hence, by what experience the correctness of the system can be proved [*in welcher Weise die Tatsachen der unmittelbaren Wahrnehmung systematisch aus den Formeln abgeleitet werden können, durch welche Erfahrungen sich also die Richtigkeit des Systems erweist*]. (EW 197)

Note in passing that, in Hertz's view, experiments test the correctness, not the truthfulness of a theory.

On one hand, Hertz states that in no way can a direct proof for each of these equations be deduced from experience. On the other hand, he affirms that experience serves to prove the correctness of a theoretical system. And, that the physical interpretations of the equations are to be regarded as facts derived from experience, and experience must be regarded as their proof. The two statements appear, at first sight, to be contradictory; they are presented in the introductory passage of Hertz's theoretical paper referred to above and are thus worthy of an attentive reading. Here is the whole passage:

In the first part (A) I give the fundamental ideas [*Grundbegriffe*] and the formulae by which they are connected. Explanations [*Erläuterungen*] will be added to the formulae; but these explanations are not to be regarded as proofs [*Beweise*] of the formulae [because other *Darstellungen* may also be attached to the same formula]. The statements [i.e., elucidations] will rather be given as facts derived from experience [*Erfahrungsthatsachen*]; and experience must be regarded as their proof. (EW 197)

The contradiction between the affirmation of the concept's empirical meaning, on one hand, and the denial of any inductive empirical correlation for the same concept, on the other, can be reconciled by admitting a form of correlation different from the inductive term-to-term form. The holistic (today also called Duhemian) form of correlation assures empirical meanings without being committed to an inductive correlation. In the holistic correlation,[10] concepts are not related to experience term to term (as if experience consisted of isolated facts) but as a system-to-system form of correlation. This is exactly Hertz's idea and it is clearly expressed in the continuation of the passage above:

The statements [i.e., elucidations] will rather be given as facts derived from experience [*Erfahrungsthatsachen*]; and experience must be regarded as their proof. It is true, meanwhile, that each separate formula cannot be specially tested by experience, but only the system as a whole. But practically the same holds good for the system of equations of ordinary dynamics. (EW 197)

In Hertz's view, if facts are deduced from a systematic theory, no fact in isolation can be conceived (hence there is no basis for induction), and a system of concepts can be correlated only to a system of facts.

All this implies that, according to Hertz in his "The Forces of Electric Oscillations, Treated According to Maxwell's Theory," no experiment can be crucial for a single Theory, but experiments indicate which, among many Theories, is the most adequate. In Hertz's view Maxwell's Theory "has been found to account most satisfactorily for the majority of the phenomena" (EW 159).

The holistic-Duhemian conception of the theory–experiment relation is one of the main aspects of the *Bild*-conception. In it, a new relation is established between theory and experiment (D'Agostino 1989, 70 ff.). Through his *Bild*-conception Hertz aimed at reconciling the various difficulties pointed out in his criticism of the Helmholtzian third standpoint and, more generally, of the traditional conception of the theory–experiment relation.

HERTZ'S *BILD*: EXPERIMENT AND THEORY RECONCILED?

Though widely read and commented upon, Hertz's proposal for a new form for the foundational axioms of mechanics did not find favor with his fellow physicists at the close of the century. Boltzmann, for example, criticized Hertz's mechanics on account of its abstractness, which, in his opinion, lost in empirical efficiency what it presumably gained in axiomatic clarity (Boltzmann 1974, 225; D'Agostino 1990).

Boltzmann's ideas on Hertz's scientific method are expressed in his 1899 essay, "On the Development of the Methods of Theoretical Physics in Recent Times" (Boltzmann, 1974, 77–100). Boltzmann has by now read Hertz's *Principles*. Contrary to the traditional conception of theory as a true description of nature (Boltzmann's "complete congruence with nature") or as a best approximation of it, theory is now presented by Boltzmann "as a mere representation [*Bild*] of nature, a mechanical analogy as he [Maxwell] puts it, which at the present allows one to give the most uniform and comprehensive account of the totality of phenomena" (Boltzmann 1974, 90–91). Although this conception represents an undeniable failure from the perspective of the old descriptive conception of theories, at the same time, as Boltzmann himself comments, it has some advantages: the proliferation of theories, one of its consequences, is fruitful in "adding new and hitherto unknown phenomena." Boltzmann's second important conclusion in 1899 is that the conception of the plurality of theories has among its consequences the rejection of the old criterion for theory-testing: the "crucial experiment." Reciprocally, the experimental confirmation of a theory cannot be considered as a test of its "absolute correctness." In 1899, Boltzmann considered Hertz's philosophy as an advance in the direction opened by Kirchhoff and Maxwell. He believed that Hertz had deepened philosophically Maxwell's epistemological ideas (1974, 90–91).

In 1904, Boltzmann affirms that Hertz's *Richtigkeit* has to be preferred to *Zulässigkeit*, Hertz's non-empirical criterion of an "inner perfection." Boltzmann, by his own admission (1974, 111), renounced this inner perfection in his *Mechanics*, thus overturning Hertz's methodological advice. In 1904, what is given, "the empirical strength of data," seems Boltzmann's major if not sole criterion in deciding between theories. Consistent with his epistemological tenets, Boltzmann's *Mechanics* has an axiomatic foundation more akin to the traditional formulation. As a consequence, it eschews contiguous action, which according to Boltzmann "however a priori likely it may seem to some, still goes completely beyond the facts and to date remains well beyond what can be elaborated in detail" (1974, 119). To the non-sophisticated reader it might seem strange that, more than ten years after Hertz's celebrated experiment on the propagation of electromagnetic waves, Boltzmann still considers contiguous action as "completely beyond the facts" (1974, 111).

While Hertz believed that his mechanics with its hidden masses had opened a path for the solution to the problem of the mechanical explanation of electrodynamics (PM 26), Boltzmann acknowledged the contemporary failure of mechanical representations of electrodynamics, although he still hoped that such a representation could be reached in the future (1974, 119). Like Hertz, in 1904 Boltzmann considered logical clarity to be the only valuable criterion in theory construction, not to be compromised, for fear that our constructs will prove arbitrary, i.e. empirically empty, by a rush to bring in "experience too early." However in 1905, he has reached the conclusion that in theory construction, permissibility

[*Zulässigkeit*] has not to be preferred "per se." In other words, he does not consider axiomatic structure to possess any intrinsic value (a regulative role) in Hertz's sense. Unlike Hertz, Boltzmann concluded that "We shall start from what is given" and "heed only the aim of obtaining an adequate expression of what is given" (1974, 168). Boltzmann arrived at this last conclusion in his late years, overcoming those doubts on the crucial nature of experiments that he espoused in his 1899 essay.

On the whole, Boltzmann's criticism grew out of a certain uneasiness with Hertz's position on the correlation of theory and experiment. It might seem surprising that, in his papers, Hertz at times manifested the same uneasiness and even alluded to a kind of separation "de facto" between the two components. To this separation, Hertz alluded in the following passage:

What we here indicate as having been accomplished by the experiments [*Was wie hier als die Leistung der Versuche bezeichnet haben*] is accomplished *independently* of the correctness [*unabhängig von der Richtigkeit*] of particular theories. Nevertheless, there is an obvious connection between the experiments and the theory in connection with which they were really undertaken. (EW 19)

Again, in his paper, "On Electromagnetic Waves in Air and their Reflection," commenting on the relation between his experiment and Maxwell's Theory, he meant to describe the experiments:

without paying special regard to any particular theory [i.e., Theory]; and indeed, the demonstrative power of the experiment is independent of any particular theory [i.e., Theory]. Nevertheless, it is clear that the experiments amount to so many reasons in favor of that theory of the electromagnetic phenomena which was first developed by Maxwell from Faraday's views. (EW 136)

Let us interpret the independence above as a form of independence of the empirical level in respect to Theory. However, in his introduction to his collected papers on electrodynamics he concluded that "the object of these [his] experiments was to test the *fundamental hypotheses* of the Faraday-Maxwell theory, and the result of the experiments is to confirm the *fundamental hypotheses* of the theory" (EW 20, emphasis added).

I think there is some evidence for admitting that Hertz, in his rather faithful reportage of the often dramatic impact on him of the reality of experimentation (when he was "struck" etc.), tried to escape from some contradictory statements. I wish to add some additional comments on this delicate point.

To begin with, let us consider that one of the reasons for Hertz's rejection of the Helmholtzian electrodynamics was its inability to predict ethereal purely-transversal waves. Its defect was, at bottom, a limitation in predictability. On the other hand, Hertz's statement above: "facts are deduced from theory," is in no sense a claim that theory alone may suffice for a total forecast of observations (this could have been a kind of Einsteinian completeness). Hertz is very definite on this point: no theory could have foreseen the behavior of electric sparks that allowed him to detect exceptionally rapid electric oscillations and waves:

> Nor, indeed, do I believe that it would have been possible to arrive at a knowledge of these phenomena by the aid of theory alone. For their appearance upon the scene of our experiments depends not only upon their theoretical possibility [*theoretischen Möglichkeit*] but also upon a special and surprising property of the electric spark which could not be foreseen by any theory. (EW 3, cf. Helmholtz in PM xxxi)

This statement appears somehow to contradict the former holistic position of a correspondence between the system of concepts and the system of facts. It amounts to an admission that, in spite of the Theory's powerful grasp, phenomena are often unpredictable and only a post-factum Theory can account for them. The predictive power of Theories is here conditioned. However, Hertz's statement above indicates that no Theory can be considered totally immune from this sort of limitation. One *Darstellung* might be favored in this respect, others may not, but limitation in prediction is common to all. Hence, *limitation in predictability is an inherent feature of physical theories*. One could legitimately infer that no Theory is capable of predicting all the phenomena that are possibly relevant to theory. Seen from the other side of the coin, from the empirical side, this limitation can be seen as an *indifference to theory* on the part of the empirical level.

It is remarkable that Hertz derived the separation between theory and experiment from his holistic view: as we saw, in this view the concepts-facts relationship holds in a direction opposite to the presumed inductive process. The denial of the "crucial experiment" contributes to this separation. Let us now reinterpret this feature as the Theory's *partial autonomy* with respect to observations. If this seems to limit Hertz's holistic view of the theory-experiment relationship, one should also take into account that this view admits a certain amount of *theoretical independence* at the empirical level, an independence which represents the counterpart of the Theory's *partial autonomy* with respect to observations.

It seems to me that both the *theory's partial autonomy* and the *theoretical indifference* of the empirical level are landmarks of a crisis in the Hertzian search for an ideal theory-experiment fit. This crisis led to an awareness of an increasing split between experiment and the new form of theory.[11] Perhaps this awareness can be considered the intellectual component of a certain split "between experimenters and theorists in several branches of twentieth-century physics, which has become a significant part of the intellectual, social, and educational structure of the physics discipline."[12]

CONCLUDING REMARKS

I have shown how Hertz elaborated his new methodological orientation, beginning with his 1884 paper and his experiments in 1887–88, progressing to his two great theoretical papers in 1890, and developing its methodological and epistemological implications in the last work of his life, the *Principles*.

Through his logical analysis of the foundational axioms of Maxwell's *Darstellung*, Hertz aimed to unveil those defects of Maxwell's theory which he thought were responsible for its hostile reception by German physicists. Hertz concluded that the theory's foundational axioms lacked logical consistency.

At bottom, Hertz's emphasis on logical consistency expressed his concern for a better rationalization of the methodology of research in physics. He thought that in the past this research had proceeded through a hybrid mixture of empirical practices and of conceptual apparatus (PM 5; EW 28); a mixture which, on one side, was expected to be supported by the empirical basis while, on the other, was deemed to represent its justification. Though in the past this method had been successful in advancing science, Hertz felt that, in the new theoretical and experimental situation produced by Maxwell's theory and by his own experiments, this same methodology had proved somehow inefficient, and that an improved knowledge of the reciprocal interbreeding between theory and experiment was then required.

It can be argued that, as a completion of his program in the new mechanics,[13] Hertz intended to present Maxwell's equations as a set of equations describing the ethereal motions, thus again linking electrodynamics with mechanics, the aim of the old nineteenth century program, but this time in the context of his new conception of a *Bild*-theory. This part of his program remained a "torso," "less a doctrine than a program for establishing a doctrine" (Duhem 1980, 88; R. Cohen 1956, xii), and it has since been abandoned in the development of theoretical physics.

He aimed at a reconciliation between theory and experiment in the frame of his *Bild*-conception. However, it is my conviction that Hertz's proposals for this reconciliation opened new difficulties. Clearly, these difficulties originated in the binary aspect of the *Bild*-conception of theory. It is implicit in the incompatibility between the *Bild* request for an anti-historical permissibility (a perennial formal logical-grammar) and the historically contingent mutability of representations (Chevalley 1991, 561, 564–556, passim; Schiemann, this volume).

The supposedly unconstrained factual strength of experiments, a tenet in the positivistic views, certainly proved weakened after the philosophy of the experimentalist Hertz. I am inclined to argue that the consequent liberalization of Theory's role contributed to an increase in the scientist's trust in the power of pure thought.[14] His ideas were highly influential in "bringing physicists to the utmost critical re-appraisal of their intellectual tools" (Cohen 1956). Einstein showed that he learned Hertz's lesson in his unprejudiced choice of axioms for his 1905 relativity paper. Hertz's emphasis on the axiomatization of physical theories had a seguito in Einstein's Special Relativity which took Maxwell's laws and the Lorentz transformations as fundamental equations for the new relativistic mechanics.

On account of the progressive aspect of Hertz's *Bild* program, one has to recognize that his admission of a variety of possible modes of selecting the foundational axioms of theories gave theoretical physics a new freedom and power for its inventions.

Elsewhere I have shown that, from many aspects, Hertz's views were not in the shadow of Helmholtz. It is interesting to read how the contrast between Hertz's and Helmholtz's philosophies is seen by Helmholtz himself (D'Agostino 1993b, 65–66). Of course, this is not to imply that Hertz did not recognize his indebtedness to Helmholtz for many of his achievements.

In this work, I have stressed one aspect of Hertz's philosophy, the problems created by his attempted reconciliation of experiment and theory in the frame of his

Bild-conception. In casting a backwards glance over the whole of Hertz's achievements, I realize that, these problems apart, what makes Hertz stand out and is unquestionably his greatest success is the unmediated [*unmittelbare*] discovery of radio waves. It is true that, in one way or another, his Theory deserves credit for bringing to light this discovery – a presence in *nature*. But it would be a mistake to believe that its success and the waves' mere presence could have released Hertz from thinking he had to understand how and why the Theory did succeed, i.e. how theory and experiments were related. His "ethos" was not a "discovery ethos" but an "intellectual ethos," which aimed to associate science and philosophy in a single field of interest. On this, he was a true Helmholtz disciple and an illustrious representative of the European tradition. As he wrote in his *Electric Waves,* his discovery of the wave was, for him, mainly of philosophical importance (EW 19).

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NOTES

Expressions of gratitude go to Maria Zayac, who not only corrected my English but also attempted to improve my knowledge of her language.

I also wish to express my thanks to the Conference participants who actually contributed to bringing me a step further in my comprehension of Hertz's ideas.

1 I mainly refer to the Logical Empiricists, Moritz Schlick and Hans Reichenbach.

2 Controversies do exist concerning the English translation of the German word "*Bild*" (Chevalley 1991), which was originally used by Kant in his *Critique of Pure Reason*. Robert S. Cohen (1956) objects to rendering it with the English "image," noticing that Braithwaite uses "internal pictures." It is remarkable that Hertz, in describing *Bilder* as "representations," preferred to use consistently the word "*Darstellung*," rather than "*Vorstellung*" to indicate the active participation of the mind. Cf. Alan Janik and Stephen Toulmin, *Wittgenstein's Vienna* (New York: Simon and Schuster, 1973), p. 140.

3 Hertz's physical interpretation [*Deutung*] of the symbols, part of a *Darstellung*, is to be distinguished from purely psychological representations or "concrete sensual representations" (Heidelberger 1994). As rightly pointed out by various scholars (Chevalley 1991), the Hertzian holism is also to be accounted for.

4 Hertz's fundamental law of mechanics: every free system persists in its state of rest or of uniform motion in a straightest path.

5 The equations of motion of a free system adopted by Hertz are usually known as Lagrange's equations of the first form.

6 In their classic work, Jungnickel and McCormmach refer to my thesis on the importance of the 1884 paper and they also quote Planck's remarks on the subject (Jungnickel and McCormmach 1986, p. 47). O'Hara and Pricha refer to my evaluation of Hertz's 1884 essay and they also quote other theses of my 1975 work (O'Hara and Pricha 1987). Mulligan concords with my view of Hertz 1884.

7 In 1884, Hertz had in fact accepted contiguous propagation and derived Maxwell's equations without introducing dielectric action and the related polarization, founding his derivation on purely dynamical considerations of the effect of Faraday's induction on Ampère's action-at-distance forces, and adopting the two principles above. Hertz's original 1884 derivation of Maxwell's equations from the two principles above represented an apt mathematical and logical structure for the interpretation of his later experiments on the propagation of electric and magnetic fields in a Maxwell-type theory.

8 The complex soundness of Hertz' derivation has been recently confirmed by Zatzkis and Feynman, who use a Hertzian-type deduction to pass from stationary currents to the variable

currents regime and Maxwell's equations, cf. Zatzkis 1964, pp. 898ff. and Richard Feynman, Robert Leighton and Matthew Sands (eds.), *The Feynman Lectures on Physics* (Reading: Addison-Wesley, 1964), volume 2, chapters 18 and 32.

9 In an important and rather exhaustive 1993 paper, which only recently came to my attention, Olivier Darrigol presents the thesis that Hertz's 1884 paper suffers from important defects and that Hertz might have become aware of them some time after the publication. This should explain why he never referred back to it (Darrigol 1993a). Hertz's 1884 paper was examined with admirable depth and detail in Buchwald 1994, pp. 177–214. I was able to read it when the present paper had been completed. On another occasion I intend to deal with the problems raised by these works.

10 This conception of the theory–experiment relationship was to be named later as the Duhem–Quine hypothesis.

11 This split was openly recognized by Schrödinger (D'Agostino 1992, p. 347).

12 Peter Galison, *How Experiments End* (Chicago: University of Chicago Press, 1987), p. 12.

13 "[...] it is premature to attempt to base the equation of motion of the ether upon the law of mechanics until we have obtained a perfect agreement as to what is understood by this name" (PM xxxvii).

14 Albert Einstein, "On the Methods of Theoretical Physics" in *Ideas and Opinions* (New York: Crown Publishers, 1933), p. 313.

JESPER LÜTZEN

HEINRICH HERTZ AND THE GEOMETRISATION OF MECHANICS

THE GEOMETRY OF SYSTEMS OF POINTS AND ITS APPROPRIATENESS

In his analysis of his new *image* (*Bild*) of mechanics Heinrich Hertz (PM 29) distinguished between its physical content and its mathematical form. The physical content was characterized by the assumption that neither force nor potential energy are fundamental quantities of mechanics. When a mechanical system seems to be acted on by forces, it is, according to Hertz, because it is rigidly connected to another system of hidden masses whose fast cyclic motions to a first approximation have the same effect as forces in the traditional Newtonian-Lagrangian image of mechanics. The mathematical form of Hertz's mechanics was characterized by a geometric structure of configuration space that he called *the geometry of systems of points*.

In 1902 Hendrik A. Lorentz spoke on behalf of many physicists when he evaluated the importance of these two aspects of Hertz's mechanics as follows:

> From a physical point of view it is of the utmost interest to examine in how far the hypothesis of a hidden system, connected with the visible and tangible bodies, leads to a clear and satisfactory view of natural phenomena, a question which demands scrupulous examination, and on which physicists may in many cases disagree. On the contrary, it seems hardly possible to doubt the great advantage as to conciseness and clearness of expression that is gained by the mathematical form Hertz has chosen for his statements. (Lorentz 1937b, 36)

Therefore Lorentz devoted the paper 'Some Considerations on the Principles of Dynamics, in Connexion with Hertz's "Prinzipien der Mechanik"' (1902), from which I quoted above, to a reformulation of ordinary mechanics (including forces) into the mathematical form suggested by Hertz. The present paper, written by a historian of mathematics, will likewise concentrate on the mathematical form of Hertz's mechanics.

Let me begin with a brief outline of Hertz's *geometry of systems of points*. Hertz himself emphasized that contrary to traditional presentations his mechanics immediately embarked on a description of the motion of systems of points rather than single particles. In the configuration space of this system he introduced a Riemannian metric ds defined by:

$$\frac{1}{3}\left(\sum_{i=1}^{3n} m_i\right) ds^2 = \sum_{i=1}^{3n} m_i dx_i^2 \tag{1}$$

D. Baird et al. (eds.), Heinrich Hertz: Classical Physicist, Modern Philosopher, 103–121.

where $m_{3j+1} = m_{3j+2} = m_{3j+3}$ is the mass of the point mass with Cartesian coordinates $x_{3j+1}, x_{3j+2}, x_{3j+3}$. Hertz allowed his point masses to be bound together by constraints expressed by first order homogeneous differential equations of the form:

$$\sum_{i=1}^{3n} c_{ij} dx_i = 0, \qquad j = 1,2,...,k. \tag{2}$$

Some of these constraints can be holonomic, a term invented by Hertz to indicate that the corresponding differential equation (2) is integrable. This means that there exists a function f_j such that:

$$f_j(x_1, x_2, ..., x_{3n}) = c_j \tag{3}$$

where c_j is a constant. If there are κ holonomic constraints they can be used to reduce the number of independent coordinates to $\nu = 3n - \kappa$. Using these generalized coordinates $(q_1,...,q_\nu)$ the metric will be described by a general positive definite quadratic form:

$$ds^2 = \sum_{i,j=1}^{\nu} \alpha_{ij} dq_i dq_j. \tag{4}$$

In order to account for rolling motion such as a ball rolling on a plane, Hertz also allowed non-holonomic constraints, i.e. constraints for which the equation (2) is non-integrable. Such constraints cannot be removed by a suitable choice of coordinates.

In the Riemannian manifold with the line element defined by (1) or (4) Hertz introduced various geometric notions, especially the concept of *angles*. This in turn allowed him to define what it meant for a curve to be a *straightest possible* curve, subject to given constraints. With this geometric notion at hand he was able to formulate his only law of motion in an exceedingly simple way:

Every free system persists in its state of rest or of uniform motion in a straightest path. (PM §309)

He even added a Latin version of the law, probably in order to make the similarity to Newton's first law stand out more clearly.

It is well known that Hertz began his *Mechanics* with a philosophical Introduction about the *images* we make of things in nature, and in particular about how to evaluate different images as far as their *logical permissibility, correctness* and *appropriateness* are concerned. I shall here briefly analyze Hertz's mathematical form in this light. *Permissibility* of a mathematical formalism seems to correspond to what David Hilbert called its consistency. After Kurt Gödel – 1931 – we know that it is difficult or often impossible to decide if a mathematical formalism is consistent, but Hertz did not know of these problems and asserted:

To the question whether an image is permissible or not, we can without ambiguity answer yes or no. (PM 3)

There is no doubt that Hertz considered both his own mathematical formalism as well as the traditional purely analytical one to be consistent, so the question of *permissibility* does not distinguish between them.

An image is said to be *correct* if its predictions are in accordance with empirical results. *Correctness* therefore, does not seem to apply to the mathematical form of an *image*.

As described by Hertz *appropriateness* consists of *distinctness* and *simplicity* (PM 2). An *image* is more *distinct* than another *image* if it "pictures more of the essential relations of the object" (PM 2). This criterion also seems to be irrelevant to the mathematical form.

> Of two images of equal distinctness the more appropriate is the one which contains, in addition to the essential characteristics, the smaller number of superfluous or empty relations, – the simpler of the two. (PM 2)

Thus *simplicity* seems to be the ground on which one should decide between two equally *permissible* mathematical forms. However, if we interpret what Hertz wrote in a strict sense, his own geometric picture does not seem to fare well in comparison with a more traditional purely analytical presentation. In fact, Hertz could easily have left out all the geometric notions and retained only the analytical formulae without changing the physical content of the image. If he had done so he should have replaced his fundamental law with two laws stating that 1) a certain analytical expression (representing the velocity of an isolated system) should be a constant (Newton's first law) and 2) a certain other expression (representing the "constraint") should be a minimum (Gauss' principle of least constraints). Hertz mentioned this possibility himself (PM 31). Thus the geometry of systems of points seems to act as a superfluous idle wheel ("*leergehendes Nebenrad*" is Hertz's own characterization of force in ordinary mechanics; the English edition PM 11f. translates it as *sleeping partners* and thus avoids this Maxwellian term) which ought to be left out in order to increase the *appropriateness* of the *image*.

It is slightly unfair to use Hertz's general philosophical criteria on the mathematical form of his *image*. Indeed, it is clear that the requirements of *permissibility, correctness, appropriateness, distinctness* and in particular the characterization of *simplicity* by way of the minimization of "empty relations" (idle wheels) were primarily aimed at an analysis of the physical content of various *images* and in particular intended to disqualify the Newton–Lagrangian *image* with its superfluous concept of force. In his own defense of his *geometry of systems of points* Hertz therefore used three other arguments: 1) This mathematical presentation treats systems of points in complete analogy with the motion of a single point. 2) It allows a simpler formulation of the fundamental law of motion. 3) It places Hamilton's principle at the heart of the presentation and shows that it is a "purely geometrical method" (PM 32). Thus the criteria that count in favor of the geometry of systems of points seem to be something like intuitive clarity, elegance and beauty rather than the minimization of idle wheels.

In fact, in two places in the Introduction to his *Mechanics*, Hertz mentioned clarity and simplicity (in a broader sense) as essential elements of an image. First, he required that the *scientific presentation* of an image (the same as – or including – its *mathematical form*?) "should lead us to a clear conception of what properties are to be ascribed to the images for the sake of permissibility, what for correctness, and what for appropriateness" (PM 2). Secondly he criticized the energetic *image* of mechanics on the ground that its basic variational principle (e.g. Hamilton's principle) is not "simple" (PM 23f.). In this place "simple" does not mean free of idle wheels but, is used as the converse of "complicated" and "unintelligible to an unprepared mind." Thus "simplicity" in this broader sense seems to be a decisive if not explicitly mentioned part of *appropriateness*.

In conclusion of this section let me suggest that not only simplicity, but also intuitive clarity, elegance and beauty were decisive factors for Hertz's development of his *geometry of systems of points,* and indeed played a much larger role for his *image* of mechanics than he admitted in the Introduction. As we saw above, these characteristics of Hertz's mechanics were admired by his successors, in particular by Lorentz.

HERTZ'S AMBIVALENT VIEW OF THE NEW GEOMETRY

On November 25th, 1877 Hertz wrote to his parents:

> The entire new mathematics (from about 1830 on) is, I think, of no great value to the physicist, however beautiful it may be intrinsically, for I find it so abstract, at least in parts, that it no longer has anything in common with reality; for instance, the non-Euclidean geometry, which is based on the assumption that the sum of the angles in a triangle need not be always equal to 2 right angles, or the geometry dealing with space of four, five, or more dimensions etc. Even the elliptical functions are, I think, of no practical value. But perhaps I am mistaken. (MLD 71f.)

This was the judgment of a student who later became the first physicist to reformulate mechanics in terms of higher dimensional non-Euclidean (Riemannian) geometry. It is tempting to conclude that Hertz later discovered that his judgment of modern geometry was indeed mistaken. However, and this is the main point I want to make in this section, a closer reading of Hertz's *Mechanics* reveals that until his death he continued to express grave reservations about the use of Riemannian geometry in physics. This may seem to be an inconsistent attitude, but I shall argue that it can be understood in terms of his Kantian view of mathematics.

At first let us emphasize that, in contrast to Lorentz's later view, Hertz did not present his *geometry of systems of points* as a highly important part of his image of mechanics. He stressed that "the physical content [of his image] is quite independent of the mathematical form" but "they are so suited that they mutually assist one another" (PM 29). Though he did defend the new mathematical form of his mechanics, as explained in the previous section, the division into physical content and mathematical form clearly signals the relative insignificance of the latter. Moreover, though he did admit that "the development of this geometry [i.e. Hertz's *geometry of systems of points*] has a peculiar mathematical attraction," he hastened to add "but we only pursue it as far as is required for the immediate purpose of applying it to physics" (PM 30). It seems as though the conventionalist philosopher excuses his momentary fascination with mathematical beauty by stressing that this fascination did not mislead him to producing mathematics for its own sake.

Finally, as can be seen from the following two quotes, Hertz distinguished sharply between his *geometry of systems of points* and the Riemannian geometry of higher dimensional spaces as it had been developed by the mathematicians since the 1850s.

> Hence there arise [in the *geometry of systems of points*] many analogies with the geometry of space of many dimensions; and these in part extend so far that the same propositions and notations can apply to both. But we must note that these analogies are only formal, and that, although they occasionally have an unusual appearance, our considerations refer without exception to concrete images of space as perceived by our senses. (PM 30)
>
> It has long since been remarked by mathematicians that Hamilton's method contains purely geometrical truths, and that a peculiar mode of expression, suitable to it, is required in order to express these clearly. But this fact has only come to light in a somewhat perplexing form, namely in the analogies between ordinary mechanics and the geometry of space of many dimensions, which have been discovered by following out Hamilton's thoughts. Our mode of expression gives a simple and intelligible explanation of these analogies. It allows us to take advantage of them, and at the same time it avoids the unnatural admixture of supra-sensible abstractions with a branch of physics. (PM 32f.)

I shall return to the previous work done by mathematicians below. Here it suffices to say that they showed that trajectories of mechanical systems can be interpreted as geodesics in a certain Riemannian manifold. What is the fundamental difference between this explanation and that of Hertz? In order to answer this question it is helpful to wonder about Hertz's insistence that this *geometry of systems of points* only bears a "formal analogy" to n-dimensional Riemannian geometry. First, to a modern mathematician, Hertz's geometry seems to *be* such a Riemannian geometry. Second, trained as we are in Hilbert's formalist tradition, we may ask whether analogies between two mathematical systems can be anything but formal? Hertz's remarks seem to suggest that in his opinion it can, and therefore that mathematics, in particular the higher dimensional Riemannian geometry is more than just a formal system. In modern presentations Riemannian geometry is nothing but a formal analytical system to which are attached geometric names such as distance, map, atlas, curvature etc., very much as in Hertz's presentation of his geometry of systems of points. Bernhard Riemann on the other hand, introduced his geometry in a more philosophic context as an *a priori* possible structure of physical space.[1] This seems closer to the way Hertz conceived of the geometrical concepts of the mathematicians.

HERTZ'S KANTIAN VIEW OF EUCLIDEAN GEOMETRY

In order to understand Hertz's view of n-dimensional Riemannian geometry it is necessary to analyze his philosophical conception of mathematics and in particular of Euclidean geometry. He did not write anything about this in the Introduction to the *Mechanics*, but the very first sentence of the main text gives a strong clue:

> The subject-matter of the first book is completely independent of experience. All the assertions made are *a priori* judgments in Kant's sense. They are based upon the laws of the internal intuition of, and the logical forms followed by, the person who makes the assertions; with his external experience they have no other connection than these intuitions and forms may have. (PM §1)

The first book to which Hertz referred in this quote deals with "Geometry and Kinematics of Material Systems" whereas the second book deals with dynamics or "mechanics of material systems" as Hertz put it. The fundamental law of motion only appears in the second book, whereas the first book builds on mathematical (particularly geometrical) axioms alone (these are not explicitly spelled out but assumed to be known). Thus the quote above suggests that Hertz considered mathematics to be *a priori* in Kant's sense. This is corroborated by the beginning of his subsequent description of the geometry underlying kinematics:

> The space of the first book is space as we conceive it. It is therefore the space of Euclid's geometry, with all the properties which this geometry ascribes to it. (PM §2)

However the following addition problematizes a simple Kantian reading:

> It is immaterial to us whether these properties are regarded as being given by the laws of our internal intuition, or as consequences of thought which necessarily follow from arbitrary definitions. (PM §2)

This shows that Hertz was aware of the contemporary formalistic tendencies, according to which the axioms (I suppose this is what Hertz means by "definitions") of geometry are arbitrarily chosen. It would also have been hard for a student of Hermann von Helmholtz to overlook these new tendencies in the foundations of mathematics. Indeed Helmholtz had been actively spreading the gospel of the new non-Euclidean geometries. According to Helmholtz[2] and many other mathematicians, one cannot decide *a priori* which geometrical axioms apply to physical space. This is a question which depends on empirical facts (*Tatsachen*) such as the free mobility of rigid bodies and the angle sum in a triangle.

In the second book of his *Mechanics*, which was meant as an *image* of reality, Hertz echoes this empiricist point of view:

> Our statements concerning the relations between times, spaces, and masses must therefore satisfy henceforth not only the demands of thought, but must also be in accordance with possible, and, in particular, future experiences. These statements are based, therefore, not only on the laws of our intuition and thought, but in addition on experience. The part depending on the latter, in so far as it is not already contained in the fundamental ideas, will be comprised in a single general statement which we shall take for our Fundamental Law. (PM §296)

Of course he had to explain how one should transform experiences into statements in the image:

Rule 2. We determine space–relations according to the methods of practical geometry by means of a scale. The unit of length is settled by arbitrary convention. A given point in space is specified by its relative position with regard to a system of coordinates fixed with reference to the fixed stars and determined by convention.

We know by experience that we are never led into contradictions when we apply all the results of Euclidean geometry to space–relations determined in this manner. The rule is also determinate and unique, except for the uncertainties which we always fail to eliminate from our actual experience, both past and future. (PM §299)

Hertz did not explain how scales should be constructed (e.g. so that they follow light rays), but he did admit that the rule for determining space relations (as well as time and mass) were "obviously fortuitous" and that it probably did not "furnish true or absolute measures of ... space. ... Should we agree to use other measures, then the form of our statements would suffer corresponding changes, but in such a manner that the experiences, both past and future, expressed thereby, would remain the same'' (PM §304). It seems to me that in this quote he primarily had the precision of his measuring devices in mind. He never mentioned the possibility that with his measuring rules the correct image might turn out to be a non-Euclidean geometry, for example the hyperbolic geometry of Bolyai and Lobachevski. This would of course have caused problems for him. Indeed the geometry of the second book had to conform to our intuition, and therefore be Euclidean, as well as describe physical space correctly. The only way out would therefore be to determine a new rule of measurement (a different kind of scale) such that the geometry would be Euclidean. It is possible that Hertz had this in mind in the above quote. However, since he did not mention the problem at all, he seems to have been convinced that real space measured by accurate ordinary scales, would also in the future conform to Euclidean geometry.

The special *a priori* nature that Hertz bestowed on the geometric axioms is best contrasted with his different treatment of the one dynamic axiom. As we saw above this latter axiom is not *a priori* but is distilled from experience and its correctness is required from the start. If Hertz had considered axioms as arbitrarily chosen, it would have been more natural to treat all the axioms on an equal footing, i.e. to proceed in one of the following two ways: 1) The formalist mathematicians' way: all axioms are simply postulated under the only condition of being consistent. Mechanics as a whole would then become a mathematical system. Afterwards this system should then be checked against nature for correctness. 2) The empiricist way: all axioms are from the start chosen as correct statements in our image of nature. All their consequences would then necessarily be correct.

The fact that Hertz does not choose either of these symmetric treatments of axioms may simply reflect that he followed a long standing tradition, which was even followed by Helmholtz at the same time (Helmholtz 1898). However, Hertz's other Kantian statements make it rather clear that he did consider the two types of axioms as being fundamentally different, and that he did in fact consider Euclidean geometry as given *a priori*. His mention of "arbitrary definitions" must then be seen as lip service to the recent developments in mathematics if it is intended to mean "arbitrary axioms" at all.

THE EUCLIDEAN BASIS FOR THE GEOMETRY OF SYSTEMS OF POINTS

Given this Kantian view of mathematics, and in particular of geometry, it is clear that Hertz did not feel comfortable with high dimensional Riemannian geometry, and tried to dissociate his *geometry of systems of points* from it.

In the quote above (page 107), Hertz characterized higher dimensional geometry as a "supra-sensible abstraction." It is hard to see how this fits into the categories of the previous section. Strictly speaking, this geometry cannot be an *a priori* geometry of our intuition, because Hertz has reserved this category for Euclidean geometry alone. On the other hand it is hard to imagine that Hertz should have had a formalistic conception of Riemannian geometry. First, since no one had displayed a set of axioms for Riemannian geometry, Hertz probably would not have thought of it as a formal structure in this sense. Second, if he had thought of it as an analytic structure to which had formally been attached geometric terms, his distinction between it and his own *geometry of systems of points* would have been difficult to maintain. In fact, Hertz seems to have considered his own geometry as precisely such a formal "mode of expression" (see the quote on p. 107), in which all considerations despite their geometric garb "refer without exception to concrete images of space as perceived by our senses." Thus Hertz's geometry is simply a higher dimensional formal language created in analogy with usual geometry and having as its subject matter (*das Ding an sich*?) systems of points in ordinary Euclidean space. In contrast, therefore, higher dimensional geometry, as created by the mathematicians, seems to have been conceived by Hertz as more than a mere formalism in that it had a subject matter which was truly geometric. This would be in accordance with Riemann's own presentation.

Such an interpretation of Hertz's distinction between the approach of the mathematicians and his own *geometry of systems of points* becomes even more plausible when we notice that Hertz did not introduce the Riemannian line element *ds* (1) *a priori*, as was done by Riemann and his successors, but *derived* it from the line element of Euclidean space. This was done through a peculiar *image* of matter to which I shall now turn.

Definition 1. A *material particle* (*Massenteilchen*) is a characteristic by which we associate without ambiguity a given point in space at a given time with a given point in space at any other time. (PM §3)
Definition 2. The number of material particles in any space compared with the number of material particles in some chosen space at a fixed time, is called the mass contained in the first space. (PM §4)

This is Hertz's definition of mass. I shall pass over the problem how one might count "characteristics by which we associate … ." Instead it is important to notice that Hertz operated with *material points* which are "a finite or infinitely small mass conceived as being contained in an infinitely small space." Hertz wanted the masses even of the infinitely small *material points* to constitute a continuum, so he postulated that they contain infinitely many *material particles*. I.e., the number of *particles* in the reference space mentioned in Definition 2 is infinitely great of the second order, and the mass of the *material particles* is infinitely small of the second order. The reason for the introduction of the infinitely small *material points* is Hertz's desire to allow for a treatment (at least in principle) of fluid and continuum mechanics including the ether (an infinite number of infinitely small material points) in addition to the case he actually treated, namely that of finitely many points of finite mass.

According to Hertz, the displacement of a *material particle* is measured by the Euclidean distance between its initial and final position (PM §22). Moreover "the magnitude of the displacement of a system is the quadratic mean value of the magnitudes of the displacements of all its particles" (PM §29). Hertz applied these definitions first to finite displacements, and afterwards to infinitesimal displacements. If we allow ourselves to jump directly to the latter, the definitions imply that the displacement *ds* of a system of finitely many *material points* of finite mass can be expressed by the "Euclidean" metric:

$$ds^2 = \frac{1}{\omega}\sum_j (dx_j^2 + dy_j^2 + dz_j^2). \tag{5}$$

where the sum ranges over all the double infinity of particles contained in all the *material points*, and where ω denotes the number of these particles. If the system consists of *n material points* with masses $m_1, m_2, \ldots, m_n$, and coordinates (x_i, y_i, z_i), $i = 1, 2, \ldots, n$, and if η denotes the infinitely large number of *particles* in a unit mass, there will be $m_i\eta$ *particles* in the *i*'th *material point*. By grouping these together for $i = 1, 2, \ldots, n$, the expression (5) will be reduced to the finite sum:

$$ds^2 = \frac{1}{m\eta}\sum_{i=1}^{n} m_i\eta(dx_i^2 + dy_i^2 + dz_i^2). \tag{6}$$

where m is the mass of the system $m = \sum_{i=1}^{n} m_i$. Dividing through by η we get the Riemannian line element in configuration space:

$$mds^2 = \sum_{i=1}^{n} m_i(dx_i^2 + dy_i^2 + dz_i^2). \tag{7}$$

corresponding to (1). Thus Hertz derived the non-Euclidean line element of his *geometry of systems of points* from the line element of Euclidean geometry.

Let me point out that it would have been a simple matter for Hertz to avoid the notion of the infinitesimal particles. He could have defined a material point with mass *m* to be a tuple consisting of 1) a real number or a first order infinitesimal *m* and 2) "a characteristic by which we associate without ambiguity a given point in space at a given time with a given point in space at any other time" (just as in Definition 1 above). This definition would have allowed him to postulate the line element (7) or (1) in configuration space, and to proceed from there as he did in his mechanics. Indeed, Hertz only used the material particles to 1) define mass by counting and 2) derive the line element (7). For the rest the material particles only run as idle wheels in Hertz's mechanics.

We may therefore ask why Hertz did not increase the *appropriateness* of his *image* of mechanics by leaving out this notion of material particles? This is even more puzzling when we consider Hertz's negative view of atomism, which he considered as a necessary ingredient of the first Newtonian–Lagrangian *image* of mechanics (PM 18). To be sure Hertz's "atoms" have no properties except occupying a certain place at a certain time and being "countable" (not in the sense of cardinality), so they are not as problematic as the atoms in, e.g., Laplacian physics. Yet, one would have expected Hertz to prefer a foundation of mechanics such "that in the hypotheses of the problems, there only enter characteristics which are directly accessible to experience." Indeed he praised the second "energetic" image of mechanics for precisely that (PM 18).

Considering what Hertz used the *material particles* for, there seem to be two possible reasons why he introduced them. He may have thought it was more appropriate to define only one type of "elementary particle," and introduce the concept of mass by a simple counting procedure. However, since this virtue of his system is nowhere explicitly mentioned by Hertz, I consider it more likely that he introduced them in order to be able to derive the Riemannian metric from the Euclidean metric*. In this way he avoided "the unnatural admixture of suprasensible abstractions with a branch of physics" (see quote p. 107). If he had postulated the Riemannian metric as I suggested above, he would have been guilty of such an admixture.

To conclude, I have tried to argue that 1. Hertz all his life continued to view n-dimensional and non-Euclidean geometry with suspicion. 2. He did not think that he made use of such abstractions in his mechanics because 3. his *geometry of systems of points* was a formalism (not a true geometry) which was 4. built directly on Euclidean geometry. 5. He introduced his peculiar image of the constitution of matter, especially the material particle, exactly to deduce his geometry from Euclidean geometry.

MATHEMATICIANS' PREVIOUS GEOMETRISATION OF MECHANICS

As we saw in the quote on p. 107, Hertz referred to "mathematicians" who had considered Hamilton's principle from a purely geometric point of view. In the preface to the *Mechanics* Hertz was more explicit. Having referred to a paper by J. J. Thomson he continued:

> I might have derived assistance from this paper as well; but as a matter of fact my own investigation had made considerable progress by the time I became familiar with it. I may say the same of the mathematical papers of Beltrami and Lipschitz, although these are of much older date. Still I found these very suggestive, as also the more recent presentation of their investigations which Darboux has given with additions of his own. (PM xxxi f.)

I shall now give a brief account of the basic idea of these mathematicians.[3] At the center is Carl Gustav Jakob Jacobi's version of the principle of least action. It says that in passing from one configuration B to another one C, a system of point masses will choose the trajectory that minimizes the action integral:

$$A = \int_B^C \sqrt{2(U+h)\sum m_i dx_i^2}\,. \tag{8}$$

* This conjecture is corroborated by Hertz's own manuscripts concerning mechanics (note added in proof).

Here U is what the 19th century physicists called the potential function (equal to the negative of what we now call the potential energy), h is the total energy, that we assume to be conserved, and we have used the conventions mentioned below formula (1). If the configuration of the system can be described in terms of generalized coordinates $q_1, q_2, \ldots, q_\nu$ and if the kinetic energy can be expressed as:

$$T = \frac{1}{2} \sum_{i,j=1}^{\nu} g_{ij} \dot{q}_i \dot{q}_j \tag{9}$$

the action integral can be written:

$$A = \int_B^C \sqrt{2(U+h) \sum_{i,j=1}^{\nu} g_{ij} dq_i dq_j} \tag{10}$$

Basically, the geometrisation undertaken by the 19th century mathematicians consists in considering the differential form in the action integral:

$$ds^2 = \sum 2(U+h) \sum_{i,j=1}^{\nu} g_{ij} dq_i dq_j \tag{11}$$

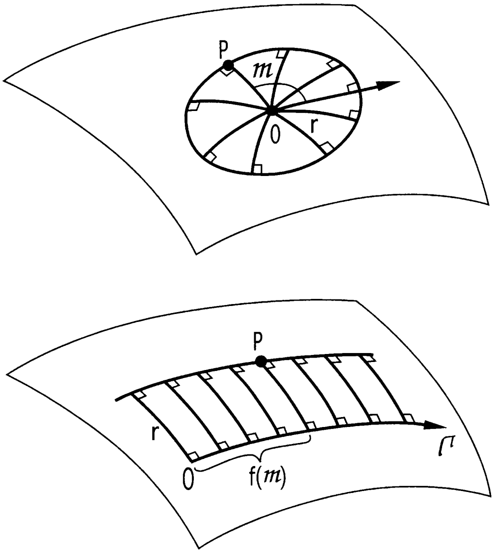
to be the line element of a ν-dimensional Riemannian manifold. The principle of least action then states that the trajectory of the system will minimize the integral:

$$A = \int ds \tag{12}$$

or rather, the variation δA will vanish. Thus the mechanical system will move along a *geodesic* (i.e., a locally shortest line) in this manifold.

This trick united research done by Carl Friedrich Gauss and Riemann in differential geometry with work done by William Rowan Hamilton and Jacobi in mechanics. In order to indicate how that happened we shall go back to the year 1828 when Gauss published his ‘Disquisitiones Generales circa Superficies Curvas’ and Hamilton published his ‘Theory of Systems of Rays.’[4] In these papers one can find analogous theorems which were later united through the geometrisation of mechanics.

It is well known that Gauss in the *Disquisitiones Generales* began the development of intrinsic geometry of surfaces, or, as he put it, the study of the properties that are independent of bending, without stretching, of the surface. His famous *Theorema Egregium* states that the Gauss-curvature is such an intrinsic property. For our purpose his extensive investigations of another intrinsic property, namely geodesics, are of more importance. He showed (Figure 1) that if on a given surface geodesics are drawn from a given point O and if equal distances (say r) are measured off along each geodesic, then the end points will make up a curve which will be *orthogonal* to all the geodesics. More generally if geodesics are drawn orthogonally to a given curve (Γ) (Figure 2), and if equal distances (r) are measured off on each geodesic on the same side of Γ, then the end points will make up a curve which will be *orthogonal* to all the geodesics. Gauss now chose to describe a point



Figures 1 and 2. Gaussian geodesics

P on the surface by coordinates (m,r) (see Figure 2) where r is the geodesic distance from P to Γ and m is the distance from a given point O on Γ to the point where the geodesic from P cuts Γ orthogonally (or a function of this distance). In Figure 1 one can think of Γ as an infinitely small circle around O and then the coordinates (m,r) are generalized polar coordinates. Expressed in these coordinates the line element has the simple form:

$$ds^2 = dr^2 + G(r,m)dm^2 \tag{13}$$

Indeed, the orthogonality result mentioned above states precisely that the mixed $dr.dm$ term will vanish from the metric. For this reason Gauss often used such coordinates.

He even showed that one could find such coordinate systems, i.e., such families of geodesics orthogonal to a common curve, by solving a certain partial differential equation. This is in fact the Hamilton-Jacobi equation for a geodesic motion (i.e., motion under the influence of no forces) on the surface, but Gauss could not make this connection since this equation did not turn up in mechanics until later.

The development leading to the Hamilton-Jacobi equation began with Hamilton's research in optics. In his 1828 'Theory of Systems of Rays' he took his point of departure in a theorem by Etienne Louis Malus. It states, that if rays emanating from a point are reflected in a mirror of arbitrary form, the reflected rays will make up a normal congruence. This means that through any point on one of the reflected rays there passes a surface that is orthogonal to all the reflected rays. Hamilton showed that contrary to what Malus had believed, this theorem holds after any number of reflections. Moreover, in three later supplements he generalized the theorem to refracted rays, and even to rays passing through a substance with varying refractive index, where the rays are, so to speak, continually refracted.[5] More specifically he showed how to determine the normal surfaces: He defined the so-called characteristic function:

$$V = \int_B^C v\, ds \tag{14}$$

where the integral is taken over the actual light ray joining B and C, and where v is the speed of light (if one adheres to the corpuscular theory of light, and equal to the inverse speed, if one adheres to the wave theory). If we now consider all light rays emanating from a point B and along each ray measure off not equal distances but equal amounts of V (14), then the points C reached in this way will constitute a surface which is orthogonal to all the light rays. This theorem is a three dimensional analog of Gauss's theorem mentioned above.

Hamilton proved that V satisfies two partial differential equations and that once V has been found as a solution to these equations, the behavior of the optical system is known.

A few years later Hamilton[6] carried these ideas over to mechanics. Here he defined the characteristic function of a system to be:

$$V = \int_B^C 2T\, dt \tag{15}$$

where the integral is taken along the trajectory connecting the configurations B and C. This is in fact the same as the action integral (10), when this integral is taken along the trajectory. In analogy with optics Hamilton showed that V satisfies two partial differential equations in the coordinates of B and C and that once one has found a solution of these two equations, the motion of the system is entirely known. Later Jacobi pointed out that it was enough to know a complete solution of one of these equations, the so-called Hamilton-Jacobi equation.

It is crucial for our story to notice that Hamilton carried only the analytical formalism but not the geometrical elements of his optics over to his mechanics. For example, the generalized version of Malus's theorem has no counterpart in Hamilton's mechanics. Why not? I think the most important reason is that a geometric description, in the manner of optics, of a system with n degrees of freedom would require an n-dimensional space, and this concept was problematic and almost non-existing at the time. In fact, as soon as Riemann's ideas became generally known around 1870 they were immediately used to geometrize mechanics.

However, before we turn to this geometrisation it is worth discussing the interactions between differential geometry and mechanics that did take place before 1870 despite the lack of a clear notion of n-dimensional geometry. When the system has three or fewer degrees of freedom a geometric interpretation does not lead to conceptual problems, and one can find several instances of such interactions. But even for systems with more degrees of freedom, one can find instances of interactions "by analogy." By that I mean that an analytical formalism is developed in analogy with an analytic-geometric study of systems with three or fewer degrees of freedom. I shall briefly mention some examples:

1. Following Jacobi's study of geodesics on ellipsoids,[7] geodesics on other surfaces were often studied in the same way, by considering them as paths of particles moving on the surface under the influence of no external forces. In this way one could benefit from the Hamilton-Jacobi formalism. As we saw above, this formalism had in fact been derived by Gauss for geodesic motion on a surface without any recourse to mechanics. This means, that from a modern point of view, there was no need to use mechanics in the study of geodesics, but it remains a historical fact, that around 1850 this was considered a forceful interaction between the two areas.

By analyzing the argument that made a solution of the equations of motion possible in the case of geodesic motion on an ellipsoid, Joseph Liouville found a large class of systems influenced by forces, whose equations of motion can similarly be solved by quadrature (i.e. evaluation of indefinite integrals). They are the so-called Liouvillian integrable systems. He did so first for a point moving on a surface, second for a point in space and third for a system of points. In this process, the last step illustrates what I called interactions by analogy.[8]

2. Jacobi's treatment of the Hamilton-Jacobi formalism was purely analytic,[9] but in 1848 Joseph Alfred Serret gave a geometric derivation of it which displayed the geometric meaning of the generating function V. His first paper was truly geometric, dealing with the motion of one point in space. The second paper dealt with systems of points and here Serret, like Jacobi, proceeded entirely analytically.[10] However, in contrast to Jacobi, Serret's derivation proceeded in complete analogy with his geometric derivation for one point.

3. The most interesting example of an interaction between geometry and mechanics before 1870, is probably Liouville's geometrisation of the principle of least action.[11] First, in an unpublished series of lectures of 1850–51 at the Collège de France, he made a remark very similar to the reduction (9)-(12) above, reducing the study of the motion of a point in a plane under the influence of forces to the study of the geodesics on a surface whose line element is described as above in terms of the potential function. This is probably the first explicit anticipation of a basic idea of the general theory of relativity to the effect that one can account for forces by incorporating them into the geometry.

Second, in a lecture of 1852–53 and in a publication of 1856 Liouville developed the analytical formalism inherent in this geometrisation also for higher degrees of freedom.[12] He showed that the problem of mechanics can be reformulated as the problem of finding a set of coordinates (V, φ_1, φ_2,..., φ_n) that will transform the differential form contained under the square root sign in the action integral (10) into the form:

$$dV^2 + g(d\varphi_1, d\varphi_2, \ldots, d\varphi_n), \tag{16}$$

that is, to the square of a total differential plus a quadratic form in the remaining differentials. In fact, as remarked by Liouville, V is a solution of the Hamilton–Jacobi equation.

The form (16) is a simple generalization of Gauss's line element (13). Liouville did not refer directly to Gauss, but he acknowledged that he had been inspired by a paper in which Ludwig Schläfli had generalized Gauss's line element (13) to higher dimensions. (In the published version, Schläfli proceeded analytically and not geometrically.)

Apparently without knowing Liouville's paper, Rudolf Lipschitz carried these ideas further in an important paper of 1872.[13] Like Liouville, he studied mechanics as a problem about transforming the differential form in the action integral, but he went on to introduce geometric notions in configuration space equipped with this metric. In particular he defined what it meant for a hypersurface to be orthogonal to a curve with respect to this form. With this notion at hand, he formulated the following generalization of Gauss's theorem:

Consider the family of trajectories of a mechanical system which cut a given hypersurface Γ orthogonally. On each trajectory and on the same side of Γ determine a point such that the action integral between Γ and this point is equal to a given constant. Then these points make up a hypersurface which is orthogonal to all the trajectories.

This is precisely the mechanical version of the generalized Malus's theorem that Hamilton did not formulate. In this way Lipschitz made a strong connection between Gauss's and Riemann's work on differential geometry and Hamilton-Jacobi mechanics.

In order to do justice to Lipschitz, I must reveal that as far as generality and motivation is concerned, the above outline is not representative of his work. In fact Lipschitz's aim was to investigate what dynamics would look like if the usual Euclidean geometry of space was replaced by a Riemannian or even more general geometry. He therefore set out by assuming that the point masses of his dynamical system were moving in an n-dimensional space whose line element was given by a p'th degree homogeneous differential form. By allowing p to be different from 2 Lipschitz complicated his theory considerably, but we do not have to dwell on these complications, in particular because his successors chose to limit the theory to the Riemannian case $p = 2$.

The interactions between differential geometry and mechanics before 1870 were scattered, but Lipschitz started a continued tradition. His most important successors were Gaston Darboux, who included a very clear account of mechanics in his

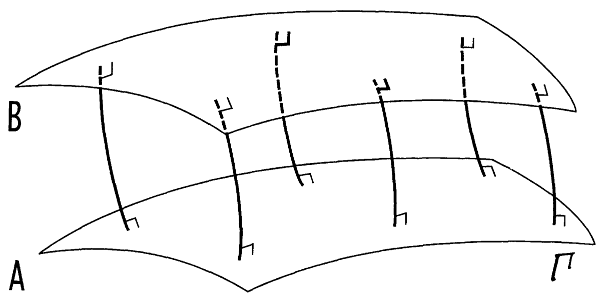


Figure 3. Lipschitz's generalization of Gauss's theorem

famous *Leçons sur la théorie générale des surfaces* (1888), and Gregorio Ricci-Curbastro and Tullio Levi-Civita, who treated mechanics as an application of their absolute differential calculus in (1901).[14] (Albert Einstein learned tensor calculus from this paper).

HERTZ COMPARED WITH THE MATHEMATICIANS

It is no wonder that Hertz considered Lipschitz's approach with suspicion. By starting with points in a n-dimensional non-Euclidean space, Lipschitz had clearly based his analysis on "supra-sensible abstractions." Darboux and his successors on the other hand, restricted the theory to systems of points in 3-dimensional Euclidean space, but as we saw above, Hertz also felt that their approach was "unnatural." I shall not return to his philosophical reasons for that; instead I shall compare Hertz's approach with that of the mathematicians, as far as its technical content is concerned.

First both parties shared the view that systems of points, rather than single points, are the basic objects of mechanics and that they should be described in terms of differential geometry. Second, both parties got rid of forces, but they did so in very different ways: Hertz, in a physical way by introducing the hidden masses, and the mathematicians in a geometric way by including the force function in the expression for the line element. From Hertz's point of view the latter method is only a geometrical reformulation of the second "energetic" image discussed in Hertz's Introduction.

Third, in both versions of mechanics, mechanical systems move along straightest lines. In the mechanics of the mathematicians, straightest lines are the same as geodesics, but in Hertz's mechanics this is not always the case. The reason for this difference is not the difference in the formalisms, but a difference in generality. In fact the mathematicians considered only holonomic constraints, so that they could reduce the number of coordinates to n freely varying quantities. Hertz, on the other hand, allowed non-holonomic constraints, and discovered, that in this case the principle of least action, or Hamilton's principle, did not hold. In Hertz's *geometry of systems of points* this is equivalent to saying that straightest lines are not always geodesics and vice versa. He illustrated this with a simple example: a ball rolling on a plane without external forces acting. He argued correctly that given two configurations B and C it is possible to roll the ball from B to C, i.e., to find a path from B to C that obeys the condition that the ball does not slip. The principle of least action now seems to suggest that the path from B to C which minimizes the action integral ((8) with $V = \text{const}$) will be the path followed by a ball rolling under the influence of no forces from B to C. However, Hertz equally correctly pointed out that there will be many configurations C that cannot be reached through a *free* rolling from a configuration B. Thus, the principle of least action (or equally Hamilton's principle) seems to pick out more paths than the system can physically follow.

This was somewhat strange because Hamilton's principle was usually considered to be equivalent to d'Alembert's principle, which did hold in Hertz's formulation of mechanics, even when there are non-holonomic constraints.

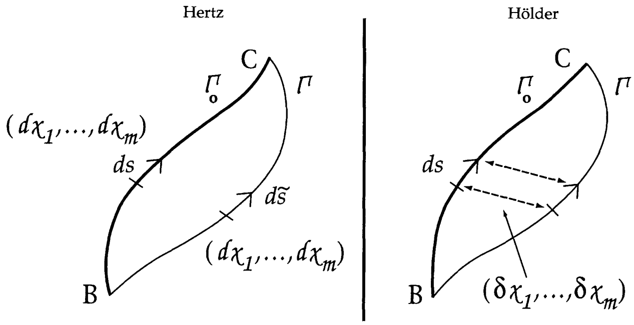


Figure 4. Hertz's and Hölder's variational methods

This very late confusion concerning the foundations of the variational calculus was completely clarified by Otto Hölder two years after the publication of Hertz's *Mechanics*.[15] He pointed out that in order to derive Hamilton's principle from d'Alembert's principle, one had to compare a path Γ_0 with other paths Γ that arise from Γ_0 by virtual displacements which are compatible with the constraints. That means that if the constraints are given by (2) the displacements δx must satisfy:

$$\sum_{i=1}^{n} c_{ij}\delta x_i = 0, \qquad j = 1,2,...,k. \tag{17}$$

Hertz on the other hand, compared the path Γ_0 with other paths Γ that were themselves compatible with the constraints, i.e. paths $(x_1(t), x_2(t),..., x_n(t)$ for which $(dx_1, dx_2,..., dx_i)$ satisfy (2). The problem is that if the constraints (2) are non-integrable, the curve arising from a virtual displacement satisfying (17) does not give a path compatible with the constraints. According to Hölder, Hamilton's principle is valid; Hertz only varied the path in the wrong way. Yet as Hölder admitted in a letter to Philip E. B. Jourdain, Hölder's type of variations does not correspond (even locally) to a usual variational problem of the kind: minimize an integral among a specified class of allowable curves.[16] It is therefore not unthinkable that Hertz realized that Hamilton's principle holds when virtual variations satisfying the constraints are performed, but that he rejected this because it was not a proper variational principle.

Be that as it may, Hertz considered Hamilton's principle and similar variational principles to be incorrect in case of non-holonomic constraints, and therefore chose the differential principle of straightest path as the fundamental law. His dislike for the philosophical and religious ideas that had been associated with the integral principles also influenced his choice.

As we saw in the quote on p. 112 Hertz claimed that he only learned of the works of the mathematicians late, but that he found them suggestive ("*doch konnte ich noch reiche Anregung aus denselben schöpfen*"). It is therefore possible that the geometric form of Hertz's image of mechanics was suggested to him by these papers at a time when the physical content of his image had been essentially worked out; however, I have not been able to illuminate how Hertz gradually developed his geometry of systems of points. Indeed, already the first of the four drafts of Hertz's *Mechanics* in the *Deutsches Museum* contains the geometric ideas and according to his own testimony, he never talked with any human being about his approach to mechanics (MLD 343).

Yet there is an entry in his diary which seems to cast doubt on his claim that he learned about the mathematicians' work at a late stage. This first note indicating an interest in the principles of mechanics is dated May 7, 1890. It reads: "Asked Lipschitz about the Hamiltonian principle." (MLD 301). Lipschitz, we should notice, was Hertz's colleague at Bonn University. What did Hertz ask Lipschitz about, and is it thinkable that the latter did not tell Hertz about his own geometric approach to this matter? One may conjecture that Hertz had discovered that Hamilton's principle did not apply to a ball rolling on a plane (or generally to systems with non-holonomic constraints) and wanted this paradox clarified by a mathematician. If this was why Hertz went to Lipschitz, he would probably not have paid close attention if Lipschitz had mentioned his geometrical formulation of the principle. It is therefore probable that Hertz had to be re-introduced to these ideas at a later time, when his own way of thinking about mechanics had made him more receptive.

CONCLUSION

In modern advanced textbooks' mechanics is often treated as a so-called symplectic geometry. This is a geometry of phase space. Geometry of a four dimensional phase space was explicitly used by Poincaré in his work on the three body problem in 1889,[17] and as we saw above other mathematicians like Lipschitz and Darboux had earlier used a Riemannian geometry of configuration space. Yet, it was Hertz who introduced such geometric ideas to the community of physicists and first gave the foundations of mechanics a geometric form. Today this is considered as an application of differential geometry to mechanics, a view that may have been shared by the 19th century mathematicians. Hertz, on the other hand, considered his *geometry of systems of points* as a formal theory distinct from, but built up in analogy with, high dimensional Riemannian geometry. This distinction between *geometry of systems of points* and *n*-dimensional differential geometry did not survive, but otherwise Hertz's *Mechanics* can be seen as the origin of the modern geometric treatments of mechanics. In this way the mathematical form of Hertz's image has proved more influential and long lived than its physical content.

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NOTES

1 Bernhard Riemann, 'Über die Hypothesen welcher der Geometrie zu Grunde liegen' (1854-lecture) in *Abhandlungen der Königlichen Gesellschaft der Wissenschaften zu Göttingen, Mathematische Classe* **13** (1867), pp. 1–20, also in *Gesammelte mathematische Werke* (Leipzig: Teubner, 1876), pp. 254–269.

2 Hermann von Helmholtz, 'Über die thatsächlichen Grundlagen der Geometrie,' (1868) in Helmholtz (1882), vol. 2, pp. 610–617; and 'The Origin and Meaning of Geometric Axioms' (1870) in Helmholtz (1971), pp. 246–265.

[3] A more comprehensive presentation can be found in Lützen (1995b).
[4] Carl Friedrich Gauss, 'Disquisitiones generales circa superficies curvas' in *Werke* (Göttingen: Königliche Gesellschaft der Wissenschaften, 1873), vol. 4, pp. 217–258, English translation *General Investigations of Curved Surfaces* (Hewlett: Raven Press, 1965); William Rowan Hamilton, 'Theory of Systems of Rays' in *Mathematical Papers* (London: Cambridge University Press, 1931–67), vol. 1, pp. 1–87.
[5] William Rowan Hamilton, 'Supplement to an Essay on the Theory of Systems of Rays' (1830), 'Second Supplement to an Essay on the Theory of Systems of Rays' (1830) and 'Third Supplement to an Essay on the Theory of Systems of Rays' (1832) in *Mathematical Papers, op. cit.* (note 4), vol. 1, pp. 107–293.
[6] William Rowan Hamilton, 'On a General Method in Dynamics' (1834) and 'Second Essay on a General Method in Dynamics' (1835) in *Mathematical Papers, op. cit.* (note 4), vol. 2, pp. 103–211.
[7] C.G.J. Jacobi, 'Note von der geodätischen Linie auf einem Ellipsoid und den verschiedenen Anwendungen einer merkwürdigen analytischen Substitution,' *Journal für die reine und angewandte Mathematik* **19** (1839), pp. 309–313.
[8] For a point moving on a surface, cf. Joseph Liouville, 'Sur quelques cas particulier où les équations du mouvement d'un point matériel peuvent s'intégrer; premier mémoire,' *Journal de Mathematiques, Pures et Appliquées* **11** (1846), pp. 345–378; for a point moving in space, cf. 'Sur quelques cas particulier où les équations du mouvement d'un point matériel peuvent s'intégrer; seconde mémoire,' *Journal de Mathematiques, Pures et Appliquées* **12** (1847), pp. 410–444; and for a system of points, cf. 'L'intégration des équations différentielles du mouvement d'un nombre quelconque de points matériel,' *Journal de Mathematiques, Pures et Appliquées* **14** (1849), pp. 257–299. See also Jesper Lützen, *Joseph Liouville 1809–1882: Master of Pure and Applied Mathematics* (New York: Springer, 1990).
[9] C.G.J. Jacobi, *Vorlesungen über Dynamik gehalten an der Universität zu Königsberg im Wintersemester 1842–43 und nach einem von C.W. Borchardt ausgearbeiteten Hefte* (supplementary volume of *Werke*, 1866, reprinted New York: Chelsea, 1969).
[10] Joseph Alfred Serret, 'Sur l'intégration des équations différentiels du mouvement d'un point matériel,' *Comptes Rendus de l'Academie des Sciences Paris* **26** (1848), pp. 605–610; for the paper on systems of points, cf. 'Sur l'intègration des équations générales de la Dynamique,' *Comptes Rendus de l'Academie des Sciences Paris* **26** (1848), pp. 639–643.
[11] For more details see Jesper Lützen, *Joseph Liouville, op.cit.*, (note 8), pp. 680–686 and 751–755.
[12] Joseph Liouville, 'Expression remarquable de la quantité qui, dans le mouvement d'un système de points matériels à liaisons quelconques, est un minimum en vertu du principe de la moindre action,' *Comptes Rendus de l'Academie des Sciences Paris* **42** (1856), pp. 1146–1154.
[13] Rudolf Lipschitz, 'Untersuchung eines Problems der Variationsrechnung in welchem das Problem der Mechanik enthalten ist,' *Journal für reine und angewandte Mathematik* **74** (1872), pp. 116–149.
[14] Gaston Darboux, *Leçons sur la théorie générale des surfaces*, 2. partie (Paris: Gauthier-Villars, 1888); Gregorio Ricci and Tullio Levi-Civita, 'Méthodes du calcul differentiel absolu et leurs applications,' *Mathematische Annalen* **54** (1901), pp. 125–201.
[15] Otto Hölder, 'Über die Prinzipien von Hamilton und Maupertuis,' *Nachrichten von der Königlichen Gesellschaft der Wissenschaften zu Göttingen, Mathematisch-Physikalische Klasse* (1896), pp. 122–157.
[16] P.E.B. Jourdain, 'On the General Equations of Mechanics,' *The Quarterly Journal of Pure and Applied Mathematics* **36** (1905), p. 75.
[17] K.G. Anderson, 'Poincaré's Discovery of Homoclinic Points' and J. Barrow-Green, 'Oscar II's Prize Competition and the Error in Poincaré's Memoir on the Three Body Problem,' both in *Archive for History of Exact Sciences* **48** (1994), pp. 133–147 and 107–131, respectively.

SIMON SAUNDERS

HERTZ'S PRINCIPLES

1. PHILOSOPHICAL INTRODUCTION

There are a number of reasons to be interested in Hertz's *Principles of Mechanics*. The Introduction is a classic in the philosophy of science, and was widely influential. The two Books that follow provide an axiomatization of mechanics in which *a priori* assumptions (Book 1) are sharply distinguished from the single "synthetic" postulate (Book 2). The work as a whole defends an alternative to the conventional formalism; the notion of "force" is derived from a more basic theory, based only on kinematic concepts together with the notion of "equations of connection" or constraints. Thereby "painful contradictions" in the conventional framework, as understood by Hertz, "will not have been answered; but our minds, no longer vexed, will cease to ask illegitimate questions" (PM 8). And questions of what exists are answered quite differently: the kinematical equations (and constraints) are supplemented by "hidden masses," in order to account for the appearance of distance forces in the usual theory; in place of fields of force we have a mechanical medium subject only to kinematical laws and the equations of constraint.

Evidently Hertz's mechanics illustrates some of the cardinal tenets of logical empiricism. It was cited by Wittgenstein (in the *Tractatus*) and by Carnap (in the *Aufbau*), and even where it was poorly received (by Poincaré and by Duhem) its influence was strongly felt. Apart from the familiar, and as Quine has put it, "disappointing" examples (due to Poincaré and others) in spatial geometry, it is the only systematic case of "equivalent descriptions" in classical physics, where the "equivalence" is neither trivial (as with notational change), nor systematic (as with "reconstrual of predicates," e.g. with change of coordinate system or representation).

This raises the question of how the claim of equivalence is to be *established*; in what sense can two systems be *seen* to be equivalent? Hertz claimed that his new system is "capable of exploring the whole content of ordinary mechanics, *so far as the latter relates only to the actual forces and connections of nature*, and is not regarded as a field for mathematical exercises" (PM xxxviii, emphasis mine); bearing in mind that the whole import of his new theory is that there is no such thing as "force" (but only accelerations produced by "connections"), the difficulty is clear enough.

We shall shortly look at his detailed arguments; but were Hertz justified in his claims presumably it would be a pragmatic question (depending on simplicity,

D. Baird et al. (eds.), Heinrich Hertz: Classical Physicist, Modern Philosopher, 123–154.

conservatism, and economy) as to which of the two systems of mechanics should be adopted. That was how it was seen by Carnap and Quine, and by Hertz as well. For Carnap and Hertz, though not for Quine, acceptance of either would carry with it certain "analytic" truths, perhaps different in each case, among them conventions on the procedures of measurement. The latter are supposed to have a quite different status from the physical principles *per se*. Quine denied that this distinction was of any real philosophical and *a fortiori* of any epistemological significance; at most what is involved is a "passing trait" in the development of the theory. But Quine did not cite historical evidence or concrete examples in science, despite the fact that it is *prima facie* a historical claim; Quine's arguments were bound up with questions of "meaning" *qua* mentalistic items, or else they focused on the adequacy of conventionalism (and of logicism) in the philosophy of mathematics. On the face of it neither is at issue in definitions or hypotheses *vis-à-vis* operational procedures in concrete physics – nor has anyone denied that such procedures are in fact necessary in science. What then of this dispute in the case of Hertz's principles?

As I shall argue, here Quine was quite right to hold that the "stipulation" of the so-called rules of coordination is on a par with theoretical hypotheses. Moreover, the difficulty with Hertz's approach is very similar to a difficulty encountered by Carnap in the *Aufbau*. Although in detail and rigor the *Aufbau* falls well short of the standards of the *Principles*, there is a methodological problem in both cases: the approach does not allow for the revisability of factual claims. As Quine puts it with respect to the *Aufbau*:

> Carnap achieved remarkable feats of construction, starting with sense data and building explicitly, with full Principia techniques and Principia ingenuity, toward the external world. One must in the end despair of the full definitional reduction dreamed of in recent paragraphs, and it is one of the merits of the *Aufbau* that we can see from it where the obstacles lie. The worst obstacle seems to be that the assigning of sense qualities to public place-times has to be kept open to revision in the light of later experience, and so cannot be reduced to definition. The empiricist's regard for experience thus impedes the very program of reducing the world to experience.[1]

What is the analogous "methodological problem" in the case of Hertz? This is the topic of Section 2. To summarize that discussion: in order to make determinations of motion precise, and in order that we can systematically revise and improve upon them, the *basic* concepts of the mechanical theory must enter into the operational procedures involved in concrete applications.

It is not only that mechanical concepts, in order to count as empirical, must have criteria of application; it is that such criteria must put in place a standard of revisability and systematic improvement in accuracy. The key difficulty for Hertz is the notion of "straight-line motion" or *inertial frame*; on the basis of his *Principles* there are no such criteria of application, with the result that the principle of inertia (Hertz's "fundamental law") becomes essentially a metaphysical notion (a "God's Eye view") that is in practice a rule-of-thumb (typically, that the laboratory bench is "good enough" as a frame of reference).

This is to be contrasted with Newton's principles, and the constructive procedure laid down in the *Principia* for determining inertial frames (or reference frames that are "as good" as inertial). This methodology is in turn reworked in Einstein's theory of general relativity; this, and the notion of "objectivity" that goes with it, has long played a central role in the history of modern philosophy. Hertz's principles, ignoring this methodology altogether, failed to provide any determinate sense to the notion of inertial frame, and for that reason they do not constitute a theory of mechanics in Newton's sense.

There are surely lessons here for philosophy, specifically the analytic tradition which Hertz helped to inspire. If we can view a system of mechanics as a "framework," in Carnap's sense – and surely we can – the crux of the matter is that what is required is a normative notion of objectivity which is stronger than the merely empirical. In view of the connections with Kant's notion of the synthetic *a priori* – to be more precise, the "qualified" a priority of the *Metaphysical Foundations* – the point bears on both sides of Carnap's contrast between "pragmatic" questions (the business of *decisions*) and "theoretical" questions, internal to a framework (a question of *truth*, including matters of fact). Carnap, then, will presumably acknowledge that there may be a certain pragmatic standard in place in dynamical physics, which rules out Hertz's mechanics as a viable framework of this sort; but granting this he must also acknowledge that this standard is closely tied to both his (Carnap's) notion of "analyticity" and to Kant's notion of synthetic a priority, and that the same standard is intact in both Newtonian mechanics and in contemporary physics.

Here Quine offers no more guidance than Carnap. As already remarked, our interest is not so much with the nature of mathematics *per se*, still less with "mentalistic" notions of meaning. We are concerned with what Quine would also call pragmatic questions: how are strands in the "web of belief" to be adjusted? What clusters of sentences are to acquire "critical mass," and how is this to be achieved so that conditional statements governing observable phenomena (what Quine calls "observational categoricals") can be held subject to a standard of accuracy and revisability? Quine speaks of simplicity and conservatism, but we can do better than that; here there is a criterion of "significance," intimately linked with concepts thought *a priori* by both Carnap and Kant, a criterion crucial to Newtonian mechanics and to relativity alike, which makes a difference as to how theory is to proceed.

If the issues raised by Hertz's *Principles* are anything to go by, the link between concept and experience that is so much at the focus of mechanical principles, and that has so much exercised philosophy, would appear to have a "constitutive" role in the notion of scientific objectivity, in something like the Kantian sense – so much so, that to abandon it is in essence to abandon the whole project of physics as it has been handed down to us. It is another question as to what difference this would make to broader questions in the philosophy of science. I do not deny that "pragmatic" rules can be adequate to some varieties of empirical research; of course they are, and since in fact the question of inertial frame makes very little difference to "most" electromagnetic phenomena produced in the laboratory – we know that the laboratory frame *is* approximately inertial for "most" purposes, on the basis of the principles of Newton and Einstein – it follows that Hertz would not in fact have been led astray "most" of the time. The important point is that there is no vantage point, on Hertz's principles, from which the qualification can be evaluated, in contrast to the situation according to Newton and Einstein. This is to say that mechanics according to Hertz has quite a different character to that with which we are familiar: as different, who knows, as theories of macro-economics, or cognitive psychology, or applied linguistics, which do not specify how they are to be applied so as to make determinate predictions, with a standard of precision and revisability already in place.

This is to deliberately advert to disciplines which make no contact with mechanical principles, unlike, for example, geology, meteorology, or molecular biology. The point about the latter examples is that whilst again they do not lead to statements of this kind, nevertheless, because of their connections with central concepts of dynamics, we understand why that is so and we understand (or at least we think we understand) what would be required for them to do so. The organizational role of dynamical principles throughout the core sciences is a subject that, for all the attention in philosophy of science to questions of "theory reduction," is clearly quite unsettled; the question of the broader epistemological significance of the existence of "correct" concepts in mechanics to other disciplines – a standard of "correctness" is just what is at issue – has scarcely been touched. For example, there are obvious implications for Kuhn's thesis or the arguments of Laudan *et al.*[2], and equally, for the various responses that have hitherto been made (as disparate as Hacking's *Representing and Intervening* and Kitcher's *Advancement of Science*).

To return to Hertz, I would like to make plain that my own view of the *Principles* is that Hertz intended to make a methodological proposal, and that he supposed that it would be given substance by a mechanical model of ether.[3] What he did not acknowledge is that failing such an application his mechanics did not even count as an alternative to Newtonian theory. The *Principles* has an outer dress of philosophy when it needed an inner one; it was because Hertz did not come to grips with "philosophical" difficulties in the concepts of space and time that he did not see that, stripped of the concept of force, the principle of inertia was no longer available to him as a *physical* principle. If Monk is correct to say that the passage from Hertz, cited in the opening paragraph, "was known by Wittgenstein virtually word for word and was frequently invoked by him to describe his own conception of philosophical problems and the correct way to solve them"[4] it may be that there is a more general lesson for philosophy as well.

2. PRINCIPLES OF MECHANICS

Hertz's mechanics is based on the idea that there is only inertial (straight-line) motion, according to Newton's first law, unless one (or more) mass-particles is connected with another by an equation of constraint. Then and only then do we have accelerated motions. If the constraints are integrable, e.g. as with rigid constraints, such as a bead constrained to move on a frictionless wire, then the energy and momentum of the total system (wire as well as bead) are conserved. But if we only consider a part of the total system, a "partial system" (e.g. the bead), ignoring some other part (the wire), the equations of condition will contain the time, and the partial system itself is accelerated.

It does not follow from this that the effects of equations of condition should always be understood in terms of the idea of "contiguous action," but Hertz clearly had this in mind. The theory of rigid connections is a part of mechanics stemming from the medieval tradition (the study of mechanical devices); both here and in statics, the connections are always thought of in terms of light inextendible threads, pulleys or levers. In every case there is no difficulty with contiguous action, for these auxiliary devices are themselves understood as bodies (their "parts" could be taken to mediate the action). But the last step is not itself reflected in the mathematical equations, or the form of the constraints.

It is worth observing that there was an alternative. Euler's hydrodynamics, where the fluid is incompressible, is a case in point. Here equations of constraint govern the displacements of neighboring volume elements of the fluid; going to the limit, we obtain first-order differential equations (e.g. the equation of continuity). And the passage to the continuum limit is not so foreign to Hertz's framework; after all, he works with infinite collections of "material points" making up each material particle, in order to have a "rational" basis for the comparison of masses.[5] But the appearance of second-order equations is strictly forbidden. We can see why if we look at the theory of elastic media, for the analog of the purely geometric equation for neighboring fluid elements is replaced by equations describing the forces acting between neighboring particles. This was the standard approach to the mechanics of the ether, as initiated by Fresnel and Cauchy, which Hertz judged to be at a dead end.

Hertz did not comment at all on the question of locality despite (or perhaps because of) the fact that it loomed so large in the debates over Maxwell's theory. It could be said that the notion of "connection" is attractive because we interpret it informally in terms of contiguous action, but it is also clear ("intelligible"), because as a system of first-order relations in the coordinates, it is a purely geometric idea, and needs no mention of forces. Hertz opted for clarity at the level of the mathematics, and tried to satisfy the intuitive demand for locality at a later stage of development.

Where Hertz did draw the line was at time-dependent constraints and inequalities. These he ruled out much as he did forces; if they were to appear at all, it would be as a consequence of integrating over some of the degrees of freedom. In terms of differentials of the coordinates q_j, $j = 1, 2, \ldots, n$ they were to be written:

$$\sum_{j=1}^{n} c_j \delta q_j = 0. \tag{1}$$

The coefficients c_j can be explicit functions of the q's, but not of the time. The differentials δq_j can be understood as the limit of finite displacements in the coordinates[6] q_i. Since the q's are functions of time, we can equally write this constraint as a first-order differential equation:

$$\sum_j c_j \dot{q}_j = 0 \tag{2}$$

In favorable circumstances (where the equation is integrable), we can find a function f of the coordinates with $c_j = \partial f/\partial q_j$, $j = 1, \ldots, n$. In this case (*holonomic* constraints) the motion is simply constrained to a subspace of the configuration space, as defined by:

$$f(q_1, q_2, \ldots q_n) = 0.$$

Choosing orthogonal coordinates, one of which is orthogonal to this subspace, the latter can simply be dropped, with the remaining degrees of freedom unconstrained. This is the standard procedure in treating motion on a frictionless plane. Hertz did not limit his mechanics to holonomic constraints, but they were clearly uppermost in his mind (his mechanics "assumes as the strictly invariable elements of nature fixed relations between the positions" [PM 41]).

As I have already remarked, locality or contiguous action was imposed informally. When Hertz came to discuss distance forces he supposed that ordinary space must contain innumerable "hidden masses." Connections between these (and with ordinary visible matter) must be responsible for the accelerations usually put down to gravity, the most important example of distance forces. Here Hertz explicitly draws a comparison between "invisible forces," introduced by the usual theory,[7] and "invisible masses and connections," postulated by his own. Hertz maintained that something like this is always necessary. "If we wish to obtain an image of the universe which shall be well-rounded, complete, and conformable to law, we have to presuppose, behind the things which we see, other, invisible things" (PM 25).

According to Hertz, the virtue of his approach is that the invisible things introduced can be just like the things that we see, and need only the concepts of motion and mass to express them. It seems that Hertz has a point, and that his scheme is in this sense the more conservative:

> We may admit that there is a hidden something at work, and yet deny that this something belongs to a special category. We are free to assume that this hidden something is nought else than motion and mass again, – motion and mass which differ from the visible ones not in themselves but in relation to us and to our usual means of perception. Now this mode of conception is just our hypothesis. We assume that it is possible to conjoin with the visible masses of the universe other masses obeying the same laws, and of such a kind that the whole thereby becomes intelligible and conformable to law. We assume this to be possible everywhere and in all cases, and that there are no causes whatever of the phenomena other than those hereby admitted. What we are accustomed to denote as force and as energy now become nothing more than an action of mass and motion. (PM 25f.)

Is the concept of connection really defined in terms of motion and mass? Here Hertz can point to the rigid body as the paradigm case of both "body" and "connection" (rigidity). Moreover, his notion of "material particle" was already linked to extension, because it is defined in terms of the number of "material points" in a given region of space. But it is all grist to my mill: there is an obvious similarity between Hertz and Descartes. In their images of the world they are almost exactly the same, and in motivation they are at one (cf. the remarks on page 144 below). The only real difference is that for Descartes the connection between body and extension is so tight that there is no room for the concept of void. With that and a correspondingly more explicit commitment to contiguous action, there is no real alternative to some sort of hydrodynamic ether, most obviously the vortex model.[8] Moreover Descartes, just like Hertz, could point to things already visible:

Let us assume that the matter of the heaven, in which the Planets are situated, unceasingly revolves, like a vortex having the Sun as its center, and that those of its parts which are close to the Sun move more quickly than those further away; and that all the Planets (among which we include the Earth) always remain suspended among the same parts of heavenly matter. For by that alone, and without any other devices, all their phenomena are very easily understood. Thus, if some straws are floating in the eddy of a river, where the water doubles back on itself and forms a vortex as it swirls: we can see that it carries them along and makes them move in circles with it. [...] Thus we can easily imagine that all the same things happen to the Planets; and this is all we need to explain all their remaining phenomena. [...][9]

Hertz did not refer to Descartes, but he did comment on Kelvin's model of the vortex atom ("an image of the material universe which is in complete accord with the principles of our mechanics"). In comparison to Descartes he was simply less specific:

And yet our mechanics in no way demands such great simplicity and limitations of assumptions. [...] We need not abandon our fundamental propositions if we were to assume that the vortices revolved about rigid or flexible, but inextensible, nuclei; and instead of assuming simply incompressibility we might subject the all-pervading medium to much more complicated conditions, the most general form of which would be a matter for further investigation. (PM 37f.).

Both were quite confident that some mechanism could be found (cf. Hertz: "thus there appears to be no reason why the hypothesis admitted in our mechanics should not suffice to explain the phenomena").

Prior to Newton, this sort of picture was extremely popular. The ancient atomists had also supposed that the world is built up out of rigid bodies and motion alone (various "shapes, sizes and motions"), i.e. in terms of rigid-body constraints.[10] The connection with Descartes is deeper only because Descartes went further along this road. He was the last to do so prior to Hertz; however Hertz was at a notable disadvantage, for unlike Descartes, he had no new concepts to bring to bear on the matter,[11] but only a new philosophy:

As to the details I have nothing to bring forward which is new or which could not have been gleaned from many books. What I hope is new, and to this alone I attach value, is the arrangement and collocation of the whole – the logical or philosophical aspect of the matter. According as it marks an advance in this direction or not, my work will attain or fail of its object. (PM xl)

But could the "logical or philosophical aspect" possibly be enough? Newton is thought to have done considerably better than Descartes; he is supposed to have made some important conceptual advance. We have a riddle: whether to infer that the Cartesian mechanics was in some sense equivalent to Newton's, or that Hertz's is not.[12]

* * *

But Newton's *Principia* did add something fundamental to mechanics. The answer to the riddle is that Hertz's mechanics cannot lead us back to the standard theory unless it also provides a method to distinguish between "true and apparent motion," what Newton's concept of force was designed to supply. The force concept – and by this I do not mean an intuitive *picture* (we might speak instead of "force functions" or gravitational potential for that matter) – is what is used to define both time and inertial motion (or departures from inertial motion), essential to Hertz's mechanics as well. Without it the equations all float on thin air.

But Hertz did provide for a notion of "effective" force, a *derived* notion, based on his theory of constraints and the hypothesis of "hidden masses." The details will take some unpacking, but evidently the question becomes whether these "effective" forces can be *used in the same way* as force-functions, particularly gravity, are used in the standard theory.

The basic idea is quite simple. We have already seen how constraints can be eliminated by reducing the number of coordinates. But both in the holonomic and in some of the more complicated cases we can include them directly in the equations, where they have the formal role of forces ("forces of constraint"). We start with Lagrange's equations, where T is the total kinetic energy and F are the external forces:

$$\frac{d}{dt}\left(\frac{\partial T}{\partial \dot{q}_j}\right)-\frac{\partial T}{\partial q_j}=F_j, \qquad j=1,2,\ldots,n.$$

These equations can be derived from Hamilton's principle, requiring that the variation in the integral along the physical path of the system, of the sum of the kinetic energy and virtual work done by the forces F under variations in the q's, be stationary. This yields:

$$\delta\int_1^2 T dt=\int_1^2 \Sigma_j\left(\frac{\partial T}{\partial q_j}-\frac{d}{dt}\frac{\partial T}{\partial \dot{q}_j}+Q_j\right)\delta q_j dt=0. \tag{3}$$

If the q's can be varied arbitrarily (so that the δq's can be arbitrarily chosen), we can conclude that each term in the summation vanishes separately, and we recover Lagrange's equations. But if there are constraints not all the coordinates will be independent. Lagrange devised a general and very powerful technique to deal with this complication. Given s constraints (for generality we here allow that the c's can depend on the time):

$$\Sigma_{k=1}^{n} c_{ik}\delta q_k + c_i\delta t=0, \qquad i=1,\ldots,s$$

we note that Hamilton's principle requires that the variation of the path is purely spatial, i.e. the coordinates are varied at constant time. δt is therefore zero and the constraints take the form of *Eq.* (1), the time-independent case. We further note that these s constraints will also be satisfied when each is multiplied by an arbitrary function of the time, denote $\lambda_i(t)$, $i = 1, 2, \ldots, s$. Adding the equations which result and integrating along the path from 1 to 2 we get:

$$\int_1^2 \Sigma_{ik}\lambda_i c_{ik}\delta q_k dt=0.$$

Combining this with Eq. (3) gives:

$$\int_1^2 \Sigma_{j=1}^n \left(\frac{\partial T}{\partial q_j} - \frac{d}{dt}\frac{\partial T}{\partial \dot{q}_j} + Q_j + \Sigma_{i=1}^s \lambda_i c_{ij} \right) \delta q_j dt = 0. \tag{4}$$

Among the (dependent) variations δq_j, a total of n-s can be made freely, with the other s variations then completely fixed by the constraints. But for these s coordinates we can require that the functions λ_i (so far completely arbitrary) satisfy the equations:

$$\frac{\partial T}{\partial q_j} - \frac{d}{dt}\frac{\partial T}{\partial \dot{q}_j} + Q_j + \Sigma_{i=1}^s \lambda_i c_{ij} = 0, \quad j = 1, 2, .., s.$$

In that case in all circumstances (whether we are varying independent or dependent coordinates) each term in the sum of the integrand of Eq. (4) must vanish separately, so we end up with the n equations:

$$\frac{\partial T}{\partial q_j} - \frac{d}{dt}\frac{\partial T}{\partial \dot{q}_j} + Q_j + \Sigma_{i=1}^s \lambda_i c_{ij} = 0, \quad j = 1, 2, .., n.$$

Given as initial data the positions and velocities at any instant, there are n+s unknowns (the $\ddot{q}_j$'s together with the λ_i's); the remaining s equations are the constraints. Given the initial data, the forces, and the constraints, the motions are completely determined.

If the forces vanish, then in rectilinear coordinates, for which

$$T = \frac{1}{2} \Sigma_{j=1}^n m_j \dot{q}_j^2,$$

we obtain the equations:

$$m_j \ddot{q}_j = \Sigma_{i=1}^s \lambda_i c_{ij} = f_j \tag{5}$$

Clearly the n functions $f_j(q,t) = \Sigma^s_{i=1} \lambda_i (t)\, c_{ij} (q_1, \ldots, q_n, t)$ could equally have been introduced into the Lagrange equation as the components of external forces, namely those that would be required to maintain the motion of the system were the constraints removed. The motions would be exactly the same; we can look on Eq. (5) equally as a force law, or, with Hertz, as the expression of a constraint. Hertz's radical move is to suppose that whenever we deal with an equation of this form, the RHS is to be understood as the expression of a constraint, even in the case of distance forces (and in particular gravity).

But so far we have no hidden masses. As I have already made clear, these are introduced to satisfy the intuitive demand of contiguous action. Moreover, introducing additional ("hidden") degrees of freedom, and writing out the equations only for these, the possible "effective" force laws are vastly increased.[13]

We can see this in detail as follows. We take the quantities q_k, $k = 1, \dots, n$ as the coordinates of the observed masses, supposing there are in addition N coordinates Q_j, $j = 1, \dots, N$ for the concealed masses. We group these together in the s constraint equations:

$$\sum_{k=1}^{n} c_{ik}\dot{q}_k + \sum_{j=1}^{N} C_{ij}\dot{Q}_j = 0, \quad i = 1,2,..,s \tag{6}$$

where the coefficients c_{ik}, C_{ij} may be functions both of the q's and the Q's (but not the time). We now suppose that the N coordinates Q_j are completely given as functions of the time. The s constraints are to be written with these functions of the time substituted for any occurrence of the Q's in the coefficients c_{ik} and C_{ij}. In this way we obtain s time-dependent constraints involving only the coordinates q_k and the time. Writing $\boldsymbol{q}$ for the n-tuple $q_1, \dots, q_n$, etc. they are:

$$\sum_{k=1}^{n} f_{ik}(t,\boldsymbol{q})\dot{q}_k + F_i(t,\boldsymbol{q}) = 0 \tag{7}$$

where $F_i(t,q) = \sum_j C_{ij}(\boldsymbol{Q}(t), \boldsymbol{q})\, \dot{Q}_j(t)$ and $f_{ik}(t,\boldsymbol{q}) = c_{ik}(\boldsymbol{Q}(t), \boldsymbol{q})$.

At this point we have to deal with "effectively" time-dependent constraints (that is why we treated this case in the foregoing). What is not so obvious is that using the constraints of Eq. (7) together with Hamilton's principle, applied only to he *visible* masses q_k, we will obtain motions consistent with what would be obtained treating the whole system. This does in fact follow; as a result, if we have the s equations of constraint Eq. (5), we can find another n equations in a total of $n+s$ unknowns, and any solution to these, supplemented by the Q's as functions of time, will give a solution to the equations for the complete system.

Given this, Hertz can cheerfully announce that the $n+s$ equations "do not now contain any reference to the unknown masses of the complete system; and as they are sufficient for the unique determination of the $n+s$ quantities $\ddot{q}_k$ and λ_i, they contain the solution of the stated problem" (§442). Quite so; but only if we are given the $s(n+1)$ functions f_{ik}, F_i as functions of the q's and the time. It is not even enough to know the "true" constraints (the c's and C's) as functions of all the coordinates, for we also need to know the Q's at all times. But Hertz explicitly remarks that these forces are to be *measured* (§541–45); it is obvious that he supposes this is how we must in practice go about things, whatever the theory, so he just as well as Newton can deduce the force function from the *observed* masses and motions (§543). We shall soon look at this claim in detail.

* * *

The remaining technical question is equally important. We must see how Hertz derives an ersatz law of action and reaction. As a first step it will help to recall Huygens' derivation of the third law in the case of collisions: since relative to any inertial frame, conservation of total momentum implies that the change of momentum of A as a result of a collision with B is the negative of the change of momentum of B due to the collision with A, it follows that the mean force $\bar{f}$ that acts on A due to B is equal and opposite to that acting on B due to A. Hertz surely has the momentum conservation law, for it is a part of his "fundamental law." So in this case, supposing Hertz's framework can handle impacts at all, the third law follows.

But the result is actually rather special; it does not follow that in the many-body case, the force of one body on another is symmetrically reciprocated. "Force" is here used in the sense of a vector field, where vectors are tied to points; it is true that given any partition of the total system into subsystems A and B, any change in the sum of the momenta of the particles of A is equal and opposite to the change in the sum of momenta of B, but neither of these are forces acting at a given point. The third law implies conservation of momentum, but is not equivalent to it.[14]

Hertz is in fact in difficulties with collision processes, because here the constraints take the form of inequalities. The clue to his treatment is to consider how forces at different points can lead to a net force acting at a single point in the ordinary mechanics. The simplest way is to suppose we deal with a rigid body, subject to a force at a given point of attachment, all treated using the theory of constraints. Intuitively, we can imagine that one coordinate q_k of one body is suddenly constrained to equal the coordinate Q_k of another (the indices can be paired by a suitable choice of coordinates), and the forces derived from the differences between the initial and final momenta. This is similar to a collision where two bodies are permanently joined on impact. Obviously the momentum change in the one equals the negative of the change in the other; the effect of the constraint is to set up equal and opposite effective forces on the two systems connected.

Hertz did not quite give the argument in this form, but it illustrates his technical derivation.[15] The argument is moreover very similar to Newton's discussion at Law III of the Axioms (the connection with momentum conservation is stated in Corollary III and IV shortly after). But at the close of that passage Newton makes it clear that the law "takes place also in attractions," referring to the following argument of the Scholium:

> In attractions, I briefly demonstrate the thing after this manner. Suppose an obstacle is interposed to hinder the meeting of any two bodies A,B, attracting one the other: then if either body, as A, is more attracted towards the other body B, than that other body B is towards the first body A, the obstacle will be more strongly urged by the pressure of the body A than by the pressure of the body B, and therefore will not remain in equilibrium: but the stronger pressure will prevail [...][16]

This new argument is now much more intriguing; if the third law did not hold, there could be no stable notion of "rigid body" (in this case a dumb-bell), because the mechanical connection will be subject to unbalanced forces. If it is a *light* connection rod, in the idealized sense (i.e. zero mass), its acceleration will be infinite, and the system necessarily flies to pieces. But the latter statement, if true in an inertial frame of reference, is true also in an accelerating frame of reference.[17] It seems the third law understood in this way has nothing to do with inertial structure, but with the notion of stability, in particular the stability of a body in a state of stress:[18] this can only be described in terms of forces if the third law is obeyed.

Newton himself did not draw this conclusion, for he did not consider the limit of small masses for the mechanical connection (the "obstacle").[19] In effect Hertz needs to prove a sort of converse: that any effective forces, generated by equations of constraint, *necessarily* satisfy the third law. But he could only draw this conclusion in the sense just stated, for pairs of effective forces associated with pairs of bodies rigidly connected. If the constraint is not there, there is no longer any effective force between the two bodies, and no longer a third law either.

Contrast Newton. The implication of his thought experiment is that whatever the forces of *A* on *B* and *B* on *A*, they are the same whether or not the obstacle is in place. Since in equilibrium they must be equal and opposite, that must also hold when the obstacle is removed and the masses fly together. The argument applies to the distance forces (the force functions) themselves, not the forces acting contiguously when the constraint is in place.

The argument is compelling, but it is unavailable to Hertz. He has no other way to say what it is for one body to exert a force on another save through the rigid constraint. But it is worse than that; for with the constraint removed, he can only suppose other constraints involving other masses come into play, by virtue of which *A* and *B* accelerate towards each other, so there is no reason to suppose the momentum of *A* and *B* alone is conserved. Hertz cannot suppose the bodies *A* and *B* are isolated (whereupon momentum conservation forces reciprocity of the net force), and when there are others masses and connections involved there is no reason to suppose momentum is conserved for *A* and *B* alone.

Hertz is clearly aware of the difficulty, but he is less than forthcoming:

> It is open to doubt, whether the extension of the application of the principle of reaction beyond what is contained in proposition 468 as to its form and content, can rightly be used as a fundamental principle of mechanics; or whether rather the actual and universally valid content of that principle has not been completely included in proposition 468.
>
> As far as the form is concerned, it is manifest that the statement of the law is not quite clearly determined when applied to action-at-a-distance. For when force and counter-force affect different bodies, it is not quite clear what is meant by opposite. For example, this is seen in the case of the mutual action between current-elements. (§470)

The action of current-elements, in the non-stationary case (where the currents are not closed), actually illustrates the problem very well: here the electromagnetic field carries away momentum. But that does not seem to illustrate any difficulty with "what is meant by opposite" when the force and counter-force affect different bodies (for they always affect different bodies[20]). Rather, *this* difficulty is specific to his system; given only mechanical connections, he can make no sense of a force *between* two bodies unless they are rigidly connected (the sum of the *net* forces will not in general be zero).

In fact Hertz has no choice but to acknowledge that his system is not equivalent to the Newtonian theory. It is also clear that he can turn this to his advantage; we recall the difficulties in the concept of ether:

> As far as the content is concerned, the application of the principle of reaction to actions-at-a-distance commonly found in mechanics manifestly represents an experimental fact, concerning the correctness of which in all cases people are beginning to be doubtful. For instance, in Electromagnetics we are almost convinced that the mutual action between moving magnets is not in all cases strictly subject to the principle. (§470)

* * *

The third law cannot be viewed as an experimental "given." It splits into two parts, the one bound up with constraints, independent of the law of inertia, whilst the other concerns distance forces. In the first case it is correlative to the general notion of stability and the analysis of wholes into parts; in the second case any direct test based on the observed accelerations depends on the assumption that the forces between the bodies are the net forces acting. Further, it presupposes that we know what is an inertial frame, or in an older language, how to distinguish "true" from "relative" motion. That takes us back to astronomy, and the Newtonian law of gravity, which of course directly incorporates the third law (and to this extent is based on it). We must see how Hertz's theory fares with gravity.

The difficulty should by now be obvious. Hertz has an ersatz force law, Eq. (5); if it is true that the inverse square law is "an experimental fact," then Hertz can surely incorporate the Newtonian theory of gravity into his own system. But again neither is the inverse square law *merely* an experimental fact. For whilst it is true that instances of it can be deduced from Kepler's laws – that is how it was discovered – Newton immediately proceeded to the hypothesis of universality, that just as the sun attracts the planets, and the earth the moon, so do the planets attract the sun, and the moon the earth. In no sense is this a consequence of Kepler's laws; on the contrary, the hypothesis directly violates these laws.[21] Nor was it implied by any other observational data; but it is implied by the third law.

In consequence, whilst Hertz along with Newton can conclude that for any central force that is a function of the coordinates of A alone (referred to the body B to which the force is directed), the area swept out by A is proportional to the time, Hertz cannot infer that A's frame of reference is not inertial (so he will not infer that the equations must be *corrected*). And whilst Newton can equally well appeal to conservation of momentum,[22] again Hertz may not, because by hypothesis there also exist the hidden masses and their motions.

Obviously Hertz's framework is pulling in exactly the opposite direction. If the moon is swept round the earth by a complex structure of connections and hidden masses, there is no obvious sense in which the third law is obeyed; in general we cannot even express the idea of "the force acting" (on one system due to another which is distant); if instead we appeal to the net forces, there are obvious reasons why they are not equal and opposite (consider Descartes' whirling straws); Newton's argument from stability is not available, as the vortex model again illustrates, for there is no reason to suppose the net forces acting are the same for the observed masses when they are static, as when they are in motion. Neither is there any reason to suppose there should be any such thing as a time-independent force law of universal validity in the first place. According to Hertz's derivation, on the contrary any force law, determined from observations alone, will have the components:

$$f_k = \Sigma_{i=1}^{s} \lambda^i(t) c_k^i(q_1,\ldots,q_n;Q_1(t),\ldots,Q_m(t))$$

where the Q's are the coordinates of the hidden masses. There is no hint that any observed force law will be more than approximately independent of the time.[23] Everything in Newtonian mechanics favors the hypothesis of universality, whereas everything in Hertzian mechanics counts against it.

I have labored the point only to get clear on the grounds on which Hertz's mechanics can nevertheless take over the law of gravity. The ground is this: its universality and reciprocity must be taken as purely *phenomenological*. They can be tolerated in Hertz's framework, for no obvious contradiction follows, but only as phenomenological hypotheses. In particular, they can have no fundamental role in the theory. For if they are used in this way, that is as *principles*, then we no longer deal with Hertz's *Principles*.

We have come to the crux of the matter; how then is Hertz to give content to the notion of *inertial frame*? As of the late 19th century, the theory of gravity was not one application of mechanics amongst others; on the contrary, it was the one application which made out a precise sense in which "true" and "apparent" acceleration could be distinguished. In order to do this – as an *empirical* discipline, in *practice* as well as "in principle" – the "principles" have to have a determinate application with a precision which can be subsequently improved – again according to the "principles."

The problem can be illustrated quite simply: if we are going to measure velocities at different times, we had better have a good clock. In order to be sure that we have a good clock, we had better be able to apply our theory of mechanics to the workings of the clock. We must, as it were, understand our clock to be good, in accordance with the basic concepts of the theory, for only in this way do we put in place a standard of accuracy. It is no mystery that this is possible in the case of Newtonian mechanics, for the theory was constructed with this end in mind. The clock in this case is the solar system.

The importance of "quantitative measurement" is rarely discussed in philosophy of science, if only because "precision" is in and of itself so obviously a good. Nor is its significance *vis-à-vis* the use of mathematics well understood; this is just as obscure as the notion that mathematics is in some sense the "correct" language for the description of nature. For all that, it is astronomy which first gave content to the concept of scientific precision, and which was part and parcel of the "mathematization" of phenomena, itself the engine of the Scientific Revolution. For this reason, any new proposal in the field of mechanics must also make out a notion of determinacy and accuracy in the application of its basic concepts. But on this Hertz has nothing to say; he makes use of a concept (inertial frame) to which he has given no determinate significance.

But there is the obvious thought that since the difficulty with gravity is that we have neither the equations of constraint nor the distribution of hidden masses, it is these that should be first investigated. This could be viewed as analogous to the phenomenological researches of Kepler (or Ptolemy for that matter). It is hardly surprising that Hertz's principles should require a model of the gravitational ether, when the whole point of his framework was to explain electromagnetic forces in terms of the electromagnetic ether.

The fact remains: Hertz had no such theory to offer and hence provided no principled basis for the application of his mechanics to concrete phenomena. Moreover, according to Hertzian principles, there is no reason to suppose that large-scale gravitational phenomena are suitable for this purpose. In fact it seems inevitable that

Hertz must look to the microscopic; the most promising avenue is to provide a detailed mechanical model of the electromagnetic ether.

There is no trace, in the *Principles*, of any worries on this score. Although he did say something of quantitative measurement, Hertz saw no problem in the appeal to the pendulum clock, the standard laboratory time-piece in his field. He did not mention that these were only useful for short time-scales (systematic errors were eliminated by astronomical calibration), but then short time-scales, and approximate time-keeping, were quite adequate to electromagnetic phenomena of his day; as Hertz, one of the leading experimentalists as of the late 19th century, was in a position to know. Likewise the laboratory frame was perfectly adequate as a rule-of-thumb reference frame; just for that reason electromagnetic phenomena gave no good answer to the question "how is an inertial frame to be determined," the business of astronomy.

Hertz's comments on time-keeping are worth citing. He begins with some basic insights of Kant's philosophy:

> Time, space, and mass in themselves are in no sense capable of being made the subjects of our experience, but only definite times, space-quantities, and masses. Any definite time, space-quantity, or mass may form the result of a definite experience. We make, that is to say, these conceptions symbols for objects of external experience in that we settle by what sensible perceptions we intend to determine definite times, space- quantities, or masses. The relations which we state as existing between times, spaces, and masses, must then in future be looked upon as relations between these sensible perceptions. (PM §297)

This is an admirable point of departure for the argument that the "definite experiences" in question had better be linked to fundamental theory. Instead we have the recommendation:

> Rule 1: We determine the duration of time by means of a chronometer, from the number of beats of its pendulum. The unit of duration is settled by arbitrary convention. To specify any given instant, we use the time that has elapsed between it and a certain instant determined by a further arbitrary convention.
>
> This rule contains nothing empirical which can prevent us from considering time as an always independent and never dependent quantity which varies continuously from one value to another. The rule is also determinate and unique, except for the uncertainties which we always fail to eliminate from our experience, both past and future. (PM §298)

There is no hint of the constraint that the accuracy of the time-piece must itself be revisable in accordance with dynamical principles. In the context of astronomy, neglect of this methodology is fatal to the claim of empirical adequacy: a pendulum clock, like any other medium-size periodic mechanical system, is quite inadequate to its needs. One wonders at what latitude of the Earth this pendulum clock is to be positioned, and with respect to which phase of the Moon. Both modify the net force which acts, and the second is varying in time. But well before systematic variations of this sort could come to light, most of the astronomical data obtained with such a clock would need revision. The pendulum swing must be periodically restored, for even with the best vacuum there will be heat dissipated at the point of attachment, and the period of oscillation is only independent of the amplitude to first order in the angle of swing. The best pendulum clocks of Hertz's day were accurate to the order of minutes per year; the proposal is quite alien to astronomy.

Here accuracy matters. The notion of precision operating in astronomy is very different from what is required in other fields (certainly as of the late nineteenth century). Already in antiquity Hipparchus was concerned with the discrepancy between the sidereal and tropical year (20 minutes); we know what trouble Kepler took with the orbit of Mars. Use of the pendulum clock for long time-base determinations, as opposed to interpolation, would have led to large perturbations to all the observed accelerations, and hence the estimates of mass; would Neptune still have been discovered on this basis?

Rule 2. We determine space-relations according to the methods of practical geometry by means of a scale. The unit of length is settled by arbitrary convention. A given point in space is specified by its relative position with regard to a system of coordinates fixed with reference to the fixed stars and determined by convention.

We know from experience that we are never led into contradictions when we apply all the results of Euclidean geometry to space-relations determined in this manner. The rule is also determinate and unique, except for the uncertainties which we always fail to eliminate from our actual experience, both past and future. (PM §299)

Reference to the fixed stars, as an absolute standard of motion, fell into disrepute from the time of Halley on, with the recognition that the stars are themselves in relative motion.[24] And in practice, this is at best a standard of rotational motion; we cannot define a measure of linear velocity (or relative velocities) in this way. Hertz concludes with the same claim that he made in the case of time: there always remain uncertainties. Of course that is true; but their step-by-step elimination is the trade-mark of astronomy.

* * *

At this point we should be clear as to what *is* adequate to astronomy. I have spoken of the determination of inertial coordinates, but this is actually too strong a demand. Newton's laws, and therefore ordinary mechanics, hold good for measurable quantities for frames in non-rotating but otherwise arbitrary linear acceleration, so long as it is only the relative distances and relative velocities that enter into the force laws. If we transform to a frame in an arbitrary state of linear acceleration:

$$\boldsymbol{r} \rightarrow \boldsymbol{r} + \boldsymbol{a}\,(t)$$

since all bodies are similarly accelerated, the equations for the relative distances and their rates of change with time will be completely unchanged. This observation is made in Corollary VI to the Axioms, right at the beginning of the *Principia*;[25] it then follows (assuming the proportionality of gravitational and inertial mass) that a uniform gravitational field can be transformed away, i.e. the equivalence principle. It was used in this way in the analysis of the orbits of the moons of Jupiter: obviously the entire system of Jupiter is accelerating towards the sun, but to a very good approximation this accelerative field is uniform; if the equations of motion that matter are the same whether or not they are referred to linearly accelerating frames, they will hold good for Jupiter's moons, and for the earth-moon system as well.

Newton actually needs the further assumption that the force laws are functions only of the relative coordinates (here it makes no difference if we also allow a dependence on the relative velocities). Hertz made no mention of Corollary VI, but it does follow from his principles as well – so long as the constraints depend only on the relative coordinates, *and so long as all the masses (including the hidden masses) are uniformly accelerated*. In other words, he can only apply this result to gravity, and in particular to the earth-moon system and the Jupiter system, on the assumption that the sun's gravitational field acts uniformly on the hidden masses. Exactly the mechanisms that Hertz invokes to account for the action of gravity must now be subject to it.

We see again how Hertz's principles are at odds with Newton's methods. On the vortex model the difficulty is obvious; there is no reason why the relative accelerations between Jupiter and its moons, due to the vortex in the vicinity of Jupiter, should be the same were there no vortex coupling Jupiter to the sun. As a result an approximation procedure, well-founded on the principles of classical mechanics, is not available to Hertz, unless it is to be another rule-of-thumb; a method which happens to work. The situation is exactly the same in electromagnetism; if there is a mechanical ether (as Hertz requires), there is no reason to suppose the laws are the same when referred to the ether frame, as to one in relative motion, unless the ether (as a material structure) is subject to the same transformations, just as in the case of sound or fluid mechanics.

But if we do have Corollary VI, the inertial structure that is needed for astronomy breaks into two parts: (i) the definition of a non-rotating coordinate system, and (ii) the definition of time. The latter is required if we are to compare relative velocities at different times, so that relative accelerations can be determined. This is what is fundamental to celestial mechanics; in practice this means that angles between celestial bodies, as subtended at observation points on the earth, must be precisely parameterized by the time.

Both (i) and (ii) are provided if we have access to a system of three or more force-free bodies whose trajectories intersect; the change of relative distance of two of these can be used as a clock, whilst the third yields a determination of rotation about their relative distance vector. Obviously both statements remain true if all three bodies are subject to the same linear acceleration;[26] therefore any such triple of non-interacting bodies, freely-falling in a spatially-uniform gravitational field, defines a frame of reference and a standard of time with respect to which Newton's laws will hold good. The problem is in principle solved – but only on the basis of Newton's principles. The crucial point against those of Hertz is that they do not license the appeal to Corollary VI.

Unfortunately the practical problem remains untouched even when we make use of Newton's principles, for in Newton's day no such bodies could be observed. But the next best thing was ready to hand; what was needed was a method to take into account the mutual gravitational forces between freely-falling bodies, that is, the system of the sun and planets, together with their moons.

I have already remarked on the first and most important step: given central forces, the area swept out is proportional to the time. This was Newton's clock, used to express the acceleration as a function of area. The practice goes back to Galileo and the analysis of projectiles. Assuming the horizontal forces vanish, the horizontal distance travelled is proportional to the time, and the vertical distance is expressed directly in terms of it. The elaboration of this method to more complicated mass distributions (taking into account the third law, and using the inverse square law), is the history of celestial mechanics; the time standard obtained is called *ephemeris time*; the general method is called "dynamical," because the time parameter is that according to which the equations of motion hold good of the observed motions.[27] We have *only* an iterative process, and it is only guaranteed to yield determinate results given the assumptions of the theory, including estimates on interplanetary matter densities and the rapid fall-off of tidal effects of stellar objects. In this sense there may come a point in the refinement in accuracy where observations of celestial phenomena no longer provide any significant test of the theory; instead we only obtain phenomenological data on the mass distributions. When the system under study is too complex, it is no longer basic principles that engage with observation, for there are too many parameters that can be adjusted.

These are not new considerations; on the contrary it is only to say that the notion of "test" in mechanics is in large part a question of consistency in application under systematically improving standards of precision. When phenomenological laws are involved – regarding, for instance, variation of viscosity with pressure or temperature – any conflict in evidence immediately translates into an investigation of the latter. The alternative is meaning holism as we enter Quine's web of belief; it is by building the fundamental concepts into operational procedures (in this case time-keeping) that they are brought to bear in the refinement of the phenomenological laws.

We are now in a better position to assess Hertz's principles: what is needed is an explicit model of a system which incorporates all of the masses (hidden or otherwise) and all of the connections, or otherwise licenses the use of an approximation in which some of the degrees of freedom are neglected; further, where the system in question is free from external influences, i.e. has no connections to other mass particles. Given this we could determine an inertial frame by appeal to conservation of momentum. So we could – had we only a "God's Eye View," some sort of direct acquaintance with systems of this kind. Unless and until human limitations are taken into account, the principle of inertia is either a Metaphysical Idea or it is a rule-of-thumb.

* * *

It is helpful to illustrate this argument with a concrete example. Consider the most ancient time-standard of all, the rotation of the earth. This is obviously vastly superior to any pendulum clock of Hertz's time (that is still true today), and it is odd that Hertz did not appeal to it. If only he could have made use of Corollary VI in the context of gravity, he would have had a promising escape route, for both standards (i) and (ii) are surely met by reference to the diurnal motion of the stars.

Moreover, as regards (i), this must be what Hertz intended by reference to the stars in Rule 2.

This is *sidereal time*; it is determined by star transits, the intervals between instants at which a given star crosses the meridian. No matter that the motion of the earth is not inertial, we have a rotating rigid body in a better vacuum than any that could have been made in the laboratory. Or so it might seem.

There is the minor point that the stars are in motion. This could be dealt with using transits of distant galaxies (let us waive the anachronism). There is also the problem of the precession of the orbit of the earth due to the moon and the sun, leading to the 20 minute discrepancy between the sidereal and the tropical year discovered by Hipparchus, but since this is uniform the effect concerns only an overall rate. The real problems arise, first, with nutation, due to periodic variations in the torques acting on the earth's equatorial bulge, and second and even more critically with the variation of latitude.

The principal contribution to nutation was discovered by Bradley, in the early nineteenth century, with an amplitude of about 9 seconds of arc every 18.6 years. This wobble of the earth's axis shows up in a periodic shift in all stellar coordinates (declination and right ascension), so as long as the variation is periodic corrections to sidereal time can easily be made. The difficulty is that there are many other harmonics (the dominant component discovered by Bradley is due to the regression of the nodes of the moon's orbit). To isolate these, there is no alternative to the use of Newton's law of gravity together with the third law. The variation in latitude is another matter. This effect concerns the variation in the mass distribution of the earth with respect to the axis of rotation; it leads to an erratic variation in the direction of the axis and, more important, to the rate of rotation. The latter is extremely serious, since it only shows up in the motion of celestial bodies (i.e. it does not effect stellar coordinates).

Obviously there is a well-known precursor to this problem, namely the slowing of the rate of rotation due to tidal friction. Astronomers had long been aware of this consequence of the Newtonian theory, but it first became a matter of observational importance in the middle of the nineteenth century. At this time Adams and Delaunay showed that the secular acceleration of the mean motion of the moon (due to perturbations) only accounted for about half of the determination of Dunthorne, Mayer, and Lalande in the preceding century (the latter was based on accumulated records going back to antiquity). In response to this, Ferrel and Delaunay made a systematic quantitative study of the tidal effect and the corresponding reaction (using the third law, or conservation of momentum together with the VI Corollary); the result agreed with the discrepancy between the theoretical and observed lunar accelerations.[28]

This is in itself not so serious a problem for Hertz's theory. Obviously it is a striking confirmation of Newton's laws (not founded on Hertzian principles), but it does not in itself force a dynamical definition of time. The reason is that the tidal friction effect is uniform on timescales comparable to the history of astronomy, and on short time-scales is anyway quite small (about 2 milliseconds per year). The correction to sidereal time itself could be made independent of Newton's law of gravity (say by appeal to energy conservation applied to the power dissipated in the tides).

But it made a difference to the way that sidereal time was viewed. Things came to a head with the construction of precise lunar tables in the 1880s. It was then clear that the long-term empirical terms in the moon's longitude, that were not accounted for by either perturbative or tidal corrections, invariably failed to represent subsequent observations, and that there were in addition irregular small-scale fluctuations. Simon Newcombe, whose work in the '80s provided the basis for the Paris Conference of 1896 (which unified the construction of astronomical ephemerides throughout the world), correctly attributed the departures to irregular variations in the rate of rotation of the earth. It was at this time too that Chandler discovered the effect that bears his name; the intersection of the axis of rotation with the surface of the earth wanders erratically over an area about 20 m in diameter, with a period of between 13 and 14 months. The final denouement to this story came later (with the theory of Brown in 1919 and the conclusive investigation by Spencer Jones in 1939), but the writing was clearly on the wall already in Hertz's time.

The difficulty for Hertz is that failing a mechanical model of a gravitational ether there is no reason why dynamical time should be taken as more fundamental than sidereal time; on Hertzian principles we are at a loss as to how to proceed. Both time-standards would involve complex and ill-understood phenomenology. But since there is no other standard of time-keeping underwritten by the theory there is no principled basis at all by which the two can be distinguished, hence neither their respective estimates as to what is motion. We have two rules of thumb, in this case conflicting, with no notion of how to proceed. It is in this sense that "the empiricist's regard for experience thus impedes the very program of reducing the world to experience."[29]

3. HISTORICAL REVIEW

In adopting the concept of "constraint" as fundamental, Hertz made a methodological decision as to which notions were properly "intelligible," thus fit for foundations. But it would be too quick to say, as do a number of introductory texts, that:

> ...the entire concept of constraints imposed in the system through the medium of wires or surfaces or walls is particularly appropriate only in macroscopic or large-scale problems. But the physicist of today is primarily interested in atomic problems. On this scale all objects, both in and out of the system, consist alike of molecules, atoms or smaller particles, exerting definite forces, and the notion of constraint becomes artificial and rarely appears.[30]

The theory of constraints is in fact a very important part of the modern theory of gauge fields and strings; but Goldstein's remarks do pin-point the crucial question of what is to be considered "complex," where all that can be obtained are phenomenological laws, and what is fundamental or "simple," where principles rule. The difficulty with Hertzian mechanics is that he did not go far enough; abandoning the notion of force, he must abandon the notion of a global inertial frame. That he did not know how to do.

He is in distinguished company. Newton too superposed a theoretical notion on his mechanics (Absolute Space), the very principles of which prohibited it from having a determinate application. Fortunately, in this case, the idle-wheel spins freely without any sound. The following remark of the historian Anneliese Maier (cited by Barbour) is perceptive, but we must add the comment that, following Newton, we can let go of the mythology, without retreating to naive empiricism (an "external" time and reference frame, Hertz's rules of thumb):

In general, the philosophers of late scholasticism behaved in the face of concrete physical questions just like the natural scientists of all times: when the all too abstract philosophical concepts became uncomfortable, they tacitly replaced these concepts by the naive empirical concepts of prescientific thinking and in practice worked with them. This explains why all kinematic problems were treated, not on the basis of the [...] Aristotelian space and time definitions, but rather on the basis of a purely descriptive determination in which *motus localis* [motion] is treated simply as a successive change of position, this change of position moreover being by no means relative to an *ultimum continentis* in the Aristotelian sense but relative to the empirical space of practical experience; this space is the same as the one Galileo meant and which then was finally introduced officially as "absolute space" into physics by Newton. And the change of position takes place in a time of which the same can be said.[31]

There are other historical comparisons that help to illustrate what is at stake. For example, it might be thought that Hertz was too quick in his appeal to hidden masses, that perhaps the motion of the planets can be directly described in terms of constraints among those observed.[32] Allowing for the "reification" of the constraint, there are precursors in abundance, from windmill vanes to a nesting of Platonic perfect solids. But even more to the point there is the ellipse itself; why not simply impose elliptical orbits as holonomic constraints, and proceed from there?

Why not indeed? We have returned to the style of astronomy familiar in antiquity, and as practiced by the medievals. The historical analogy might be made out like this: if we adopt Hertz's principles, we face the same problems as did Eudoxus and Ptolemy, with roughly the same resources and the same concept of progress.

A third comparison is even more helpful. Descartes too ran up against exactly the same problem. What can we learn from his theory of motion, and its reception by his contemporaries? The turn of events is worth recapitulating, although at times it has an air of comedy.

Descartes introduced the law of inertia in his *Système du Monde*, written in the years 1629–33; there it is quite clear that the earth "really" moved, and that the only "true" or natural motion is straight-line motion. But he was greatly disturbed by Galileo's imprisonment; his *Monde* did not appear; and more than a decade passed with his ideas unpublished. They saw the light of day in his *Principles of Philosophy* (1644). Again the law of inertia had pride of place, now supplemented with his rules of impact; but accompanying this, we have a new and radical theory of motion according to which the earth could after all be judged at rest.

Descartes performed the trick at the price of a certain inconsistency. The new theory of motion simply did not apply to his physics. Yet it was motivated by the physics; he asks whether the stars might move, "a supposition which will be shown below to be probable," and announces "we shall conclude that nothing has a permanent place, except as determined in our thought."[33] What he means by this becomes clear in section II.57:

Each body has only one proper motion, since it is understood to be moving away from only one set of bodies, which are contiguous with it and at rest. But it can also share in countless other motions, namely in cases where it is a part of other bodies which have other motions. For example, if someone walking on board ship has a watch in his pocket, the wheels of the watch have only one proper motion, but they also share in another motion because they are in contact with the man who is taking his walk, and they and he form a single piece of matter. They also share in an additional motion though being in contact with the ship tossing on the waves, they share in a further motion through contact with the sea itself; and lastly, they share in yet another motion through contact with the whole earth, if indeed the earth is in motion. Now all the motions will really exist in the wheels of the watch, but it is not easy to have an understanding of so many motions all at once, nor can we have knowledge of all of them. So it is enough to confine our attention to that single motion which is the proper motion of the watch.

We are back to Hertz and the methodological dilemma between a global (astronomical) or local (microscopic) definition of time – but with a twist. For Descartes' watch is not really an application of the law of inertia to its inner workings; it is not quite the same thing as a local dynamical time. In effect Descartes rejects from the outset the attempt to materialize an inertial frame of reference, whether by reference to the macroscopic or the microscopic: "it is not easy to have an understanding of so many motions all at once." Instead he appeals to the notion of "proper motion," according to which the relative velocities of contiguous bodies are what we should really consider as fundamental. No matter that the entire problem has disappeared from view (for this says nothing of how relative velocities at different times are to be compared), Descartes can now hold that the earth does not really move after all, no matter that (surely!) its motion is not inertial. To be more precise, the question reduces to the much less threatening query, of whether or not there is an "ether wind," as the earth is swept round in a vortex.

This inconsistency in Descartes' position is normally taken as rather worse than it actually is, for Descartes is quite right to insist on the pride of place of relative velocities. Single-time comparisons of these, along with relative distances, are what are directly observed (compare Hertz's Observation 3 at PM §304). The error is to suppose that we can quantify this notion without further ado, and thereby compare relative velocities at different times. Like Hertz, Descartes must actually build up his physics from the local, from the microscopic, using the law of inertia to materialize a standard of time along the way. Like Hertz, he does not know how this is to be done.

Newton, unsurprisingly, would have none of it. In his unpublished manuscript *de gravitatione*, written sometime around 1670, he was quite clear on the basic difficulty:

Truly there are no bodies in the world whose relative positions remain unchanged with the passage of time, and certainly none which do not move in the Cartesian sense; that is, which are neither transported from the vicinity of continuous bodies nor are parts of other bodies so transferred. And thus there is no basis from which we can at the present pick out a place which was in the past, or say that such a place is any longer discoverable in nature. For since, according to Descartes, place is nothing but the surface of surrounding bodies or position among some other more distant bodies, it is impossible (according to his doctrine) that it should exist in nature any longer than those bodies maintain the same positions from which he takes the individual designation. And so, reasoning as in the question of Jupiter's position a year ago, it is clear that if one follows Cartesian doctrine, not even God himself could define the past position of any moving body accurately and geometrically now that a fresh state of things prevails, since in fact, due to the changed positions of the bodies, the place does not exist in nature any longer.[34]

By the time he wrote the *Principia*, Newton understood better how the definition of motion, time, and inertial frame were linked to the dynamics. But the *Principia* poses a puzzle that we are unlikely to resolve. For all that Newton clearly established that not only could the center of mass of the solar system be in motion, but that it could actually be in a state of uniform acceleration, when it came to the Scholium to the Definitions (on space and time) not only do we find that there must exist an Absolute Space, but it is suggested that the true motions (with respect to this) will be determined in due course. In Proposition 11 of Book 3 it seems we have found it: the center of mass of the solar system, says Newton, is immovable.

Einstein said that we each have our Kant, and perhaps we each have our Newton too; for my part I believe Newton realized perfectly well that this will not do, but that he had no wish to make a fuss of the matter.[35] Concerning Absolute Space, that is something else; no doubt the notion of a class of inertial frames is too modern and abstract to be really intelligible as of the 17th century, even to its greatest intellectual figure.

* * *

Obviously the problem of identifying inertial motion had been well-rehearsed in the 17th century. These debates were so important to the development of mechanics that the most damaging criticism of Hertz's philosophical framework is not so much that the difficulty was ignored, but that it was *possible* that it could have been ignored. Of course Hertz was neither a philosopher nor a historian; the criticism is directed at those who saw in the *Principles* an advance in philosophy.

Something similar applies if we reflect on the methodological difficulties internal to Hertzian mechanics. A clear precursor, again in the work of Descartes, has recently been documented by Alan Gabbey.[36] It concerns a second major discovery of Descartes, namely the principle of virtual velocities (a precursor to D'Alembert's principle of virtual work). Descartes correctly saw that this was a universal principle adequate to the entire tradition of medieval mechanics, understood as the study of machines. Although Descartes argued for a view of physics which put this traditional notion of mechanics at center stage (whereas with Aristotle and the medievals these were "unnatural" motions since they involved human artifacts), he was not ready to base his physics upon it in the *Principles of Philosophy*. In fact the principle was never published in his lifetime.

The reasons are instructive. The principle of virtual velocities makes use of the concept of force, which is not of course a concept basic to his system. Descartes here is at a greater disadvantage than Hertz, because he does not have the technical tools to formulate even the ersatz force laws derived from the notion of constraints, or what Descartes would have called purely geometrical relations between the parts of matter. But in any case the most important applications at the time (in the theory of machines) depended on gravitational forces (weight); according to Descartes' principles this requires some kind of hydrodynamic model of ether, so in this respect, although Hertz allows for more general models, the two are in much the same boat. In effect the law of virtual velocities has a similar status for Descartes, as does the law of gravity for Hertz. As Descartes explains it to Marsenne, he cannot therefore set up the foundations of mechanics on the principle of virtual velocities, "without explaining what weight really is, and at the same time the whole system of the world." For this reason, in Gabbey's words, "the ultimate *principia* of Descartes' mechanics are not to be found in what he wrote on mechanics [...], and the single starting point from which the mechanics was developed, the [principle of virtual velocities], cannot do service as a *principium* of his physics in the same foundational sense as the three laws of nature."[37]

I doubt that the example is really to Descartes' credit. To suppress an important discovery because it cannot be judged a fundamental principle is strangely eccentric. But the contrast with Hertz is not favorable; insofar as motion is determinate according to Hertz's principles, it is because Newton's are used instead.

Finally we come to Kant. Here the philosophical questions are obviously much more focused, and some of them have already been touched on in Section 1 above; the *Critique* addresses exactly the problems of time-determination passed over in Hertz's *Principles*. The relationship between the *Critique* and Newton's *Principia* is of course very extensive; as Friedman demonstrates,[38] a significant fragment of Kant's epistemology hinged on the account of time in Newtonian theory. Given that time determinations are, for Kant, a necessary component to any objective experience, the principles involved thereby acquire both a synthetic and a necessary content. Time was to stand to Newton's mechanics roughly as space stood to Euclid's axioms; in both cases a certain "constructive" procedure was required.

It is unfortunate that Kant's philosophy of mathematics – to which this notion of "construction" was related – was not more carefully probed by the Vienna Circle, particularly in the light of Gödel's results and the debates over intuitionism. It is poorly made out in terms of "picturizability" (as Helmholtz understood Kant's appeal to intuition). Rather, Kant insisted on a standard of rigor that tied the mathematics directly to empirical applications – in the case of geometry, on compass and rule constructions.[39] In the case of time Kant's philosophy of mathematics is more subtle, but again we have a system of principles (as used by Newton) for the constructive (iterative) determination of a standard of rest.[40] But not even Kant could go so far as to say that Newton's laws were themselves a priori; rather, in the *Critique*, the *Metaphysical Foundations*, and particularly in the *Opus Posthumum*, we see evidence of repeated attempts to articulate a certain *kind* of a priority. One of Kant's most successful attempts in this direction can be found in the Analogies of Experience. The Analogies themselves (the principle of permanence, the law of causality, and the principle of communion) are clearly modeled on Newtonian principles (conservation of mass, the second law, the third law), generalized so as to be more plausibly understood as self-evident truths. In the *Metaphysical Foundations* a certain three-step process (the Postulates of Empirical Thought), immediately following the Analogies, is taken over as the basis of his "foundation" for Newton's laws. Of the Analogies themselves he says:

They are simply principles of the determination of the existence of appearances in time, according to its three modes, viz. the relation to time itself as a magnitude (the magnitude of existence, that is *duration*), the relation of time as a *successive* series, and finally the relation in time as a sum of all *simultaneous* existence. This unity of time-determination is altogether dynamical. For time is not viewed as that wherein experience immediately determines position for every existence. Such determination is impossible, inasmuch as absolute time is not an object of perception with which appearances could be confronted. What determines for each appearance its position in time is the rule of the understanding through which alone the existence of appearances can acquire synthetic unity as regards relations of time; and that rule consequently determines the position (in a manner that is) *a priori* and valid for each and every time.[41]

* * *

Kant was certainly wrong to claim that Newton's principles could not be bettered, but there seem to be good reasons why the concept of time must be locked into a set of dynamical principles, in something like the Kantian sense. Moreover, I doubt that this can be considered a scientific claim *per se*, despite the fact that it is standard wisdom in relativity; not only did it first arise in the Newtonian context, but it is *prima facie* an epistemological thesis. That is, it is not an *empirical* discovery. It cannot be "naturalized," or treated as a "hypothesis," in any ordinary sense of these terms.

To summarize again our conclusions, a fundamental time standard must be dynamical. If there is a general lesson for operational definitions, it is that exactly what is *not* required of an operational definition is something independent of theory. More than that, that a "good" operational definition is one that incorporates within it the most fundamental concepts available. The fundamental objection to Hertz's mechanics is that it made use of a notion (the principle of inertia) to which it could attach no definite sense. What is required is a dynamical determination of time and motion, which given Hertz's framework could only be made out at the microscopic level – most obviously, in the context of electromagnetism. Hertz needed a local electromagnetic clock and a local materialization of an inertial frame. On the basis of his principles, indeed the latter should be the rest frame of ether.

Einstein's solution does not quite fit into this mold, but it is closely related. The fundamental difference is that although he too looked to a deeper level of description than that provided by Maxwell, he did not see any reason why this should be described by the principles of classical mechanics.[42] I shall not attempt to summarize the problem situation as he saw it, but let it be granted that he was prepared to take Maxwell's equations, and the electromagnetic phenomena that they described, rather more at face value. That led him to a view of motion rather similar to Descartes: the motion of the conductor relative to the magnet, and of the magnet relative to the conductor, should differ only in sign, no matter that both were at sea inside Descartes' watch.

But this is to jump a step, and to look ahead to the general theory of relativity. As of 1905 Einstein had still to appeal to a mythological standard of rest (because just like Hertz he was to undercut Newton's laws, and thereby astronomy), and hold that these symmetries only obtained in frames moving at constant velocity with respect to this (what he called "the resting frame"). But there was now a difficulty;

Maxwell's equations could not be valid in a moving frame of reference if valid in this rest frame, for according to standard kinematics the light-speed should not be invariant. Conversely, holding that light-speed is to be invariant, Einstein could at a stroke implement his guiding inspiration, that electromagnetic phenomena should depend only on relative motions.[43]

Given that Maxwell's equations are locally defined it was natural to examine the implications of this given a *local* standard of time-keeping. If the light-speed principle is *used* in the determination of simultaneity – which otherwise proceeds exactly as in Newtonian theory[44] – the relativity of simultaneity follows immediately. Indeed, if the light-speed principle is used again in the definition of the clock (most simply a periodically reflected light-pulse), the time-dilation follows by inspection, and thereby length-contraction as well (for only then can light-speed be constant).

But proceeding in this way we do not obtain a materialization of inertial frames, nor a dynamical determination of time. The difficulty is that all of this presupposes that Maxwell's equations are referred to an inertial frame, which is entirely free-floating (for as with Hertz's mechanics, it is cut off from the inertial structure as determined by astronomy, using Newton's principles). Einstein, no more than Hertz, can appeal to the inverse-square law (or any other force-law) as anything but a rule of thumb. In fact Einstein at this point is exactly in Descartes' predicament, for whilst his concept of time and motion is purely local and relational, he yet needs a global principle of inertia (in the sense in which it is built into Maxwell's equations, which hold only for inertial frames).

At this point Einstein's thinking takes on a character that can still strike one as uncanny. It was already obvious that a global uniform acceleration is a difference that makes no difference; that there is no change in the physics (and that correspondingly such a gravitational field can be "transformed away"). But from this he drew the extraordinary conclusion that *there is no such thing as uniform linear acceleration*; that there is no "container space" with respect to which this notion makes sense. And this relates directly to non-uniform accelerations, since given that they are smoothly varying, then locally (that is in a sufficiently small neighborhood) the acceleration is constant. As the local physics goes, any freely-falling frame will do; we do not need to define an inertial frame in order to determine the gravitational acceleration, for there is no such thing.

This goes beyond Newton's Corollary VI, and of course it is quite independent of Newton's principles; it is the Principle of Equivalence. Given any triple of sufficiently close freely-falling bodies, of sufficiently small mass, an inertial frame is materialized directly, and with it a dynamical (here we should perhaps say "kinematic") determination of time. With this the problem of principle is completely solved, although once again the practical problem is untouched.

The subsequent development of general relativity, and its application to the solar system, resembles Newton's procedure; from a methodological point of view, Newton's inverse square law is used in much the same way that Newton appealed to Kepler's laws. The result is again a dynamical determination of time, although we no longer obtain even an approximation to a global inertial frame.[45]

It is important to see that the solution "of principle" also puts on a principled footing Einstein's entire procedure in the development of the special theory. The local application of Maxwell's equations is valid to a very good degree of approximation in the laboratory frame; for atomic length and time-scales the approximation is completely negligible. This means that we have in principle a second dynamical determination of time, if only there exist sufficiently small systems which can be accurately modeled by Maxwell's equations in an experimentally controlled way. Einstein had already initiated this approach in 1905, although it was Bohr who made the crucial breakthrough with his model of the atom; and it was quantum electrodynamics, not classical electromagnetism, which was the theory at issue.

I hope, with this brief survey, to have made clear what it would have taken for Hertz to finally set his mechanics on a principled footing. For I should emphasize at the last that Hertz was in many ways right; to return to the opening remark of this section, his appeal to the concept of constraints was not so far off the mark when it comes to electromagnetism: Maxwell's equations, in terms of the E and B fields, on the one hand, and the D and H fields, on the other, are in effect equations of constraint, and can be expressed in terms of the exterior derivative alone (they are therefore completely independent of both the affine structure and the metric). The one exception is the pair of equations relating the two kinds of fields (in vacuum, using Heaviside units with $c = 1$, these are $D = E$ and $B = H$); only here does any metrical structure enter (the "law of inertia" as it figures in Maxwell's theory). In particular Hertz was right to propose that the foundations of the physics to come should be built on the local and hence the microscopic, and to ignore the use of distance-forces in the determination of time and frame of reference. Although he had nothing to put in its place, this move was absolutely essential, as an intermediary step (the special theory), to Einstein's final solution.

* * *

We have seen that in principle we have a second dynamical determination of time, if only we are given a detailed application of Maxwell theory to atomic systems. That we now have, in its quantized form. As of 1976 the fundamental definition of time refers to atomic clocks. The *SI* second (Système International) is the time taken for the emission of 9,192,631,770 wavelengths in the transition between two hyperfine levels of the ground state of the caesium-133 atom. Dynamical (ephemeris) time is no longer to be used as the basis for the calibration of clocks. In its place we have Temps Atomique International (TAI), based on a free-running, data-controlled timescale (Échelle Atomique Libre or EAL) formed by combining data from all available high-precision atomic clocks (principally caesium beam standards and hydrogen masers).[46]

In fact we have two quite independent standards of time, each founded on quite different but equally fundamental dynamical theories. This was exactly the situation that Hertz really required to make sense of his concluding statement in the *Introduction*; moreover, it concerns exactly the two fields, astronomy and electromagnetism, macroscopic and microscopic, that he too saw as the ultimate ground of contest, no matter that the crucial concepts in both cases had moved on. It is worth quoting at length:

> Now, if we could perceive natural motions with sufficient accuracy, we should at once know whether in them the relative acceleration, or the relative relations of position, or both, are only approximately invariable. We should then know which of our two assumptions is false; or whether both are false; for they cannot both be simultaneously correct. The greater simplicity is on the side of the third [Hertz's] image. What at first induces us to decide in favour of the first [Newton's] is the fact that in actions-at-a-distance we can actually exhibit relative accelerations which, up to the limits of our observation, appear to be invariable; whereas all fixed connections between the positions of tangible bodies are soon and easily perceived by our senses to be only approximately constant. But the situation changes in favour of the third image as soon as a more refined knowledge shows us that the assumption of invariable distance-forces only yields a first approximation to the truth; a case which has already arisen in the sphere of electric and magnetic forces. And the balance of evidence will be entirely in favour of the third image when a second approximation to the truth can be attained by tracing back the supposed actions-at-a-distance to motions in an all-pervading medium whose smallest parts are subjected to rigid connections; a case which also seems to be nearly realized in the same sphere. This is the field in which the decisive battle between these different fundamental assumptions of mechanics must be fought out. (PM 41)

I hope it is clear how the text should be modified to apply directly to the present situation. The absolutely crucial additional demand – that each field must separately underwrite its own standard of time-keeping – should by now be quite clear. Since that is our situation, we can indeed proceed roughly as Hertz envisages. At present the two dynamical time standards are in agreement to within ten microseconds in the period 1977–1990, or about one part in 10 per year, which is within the theoretically estimated error bounds in the two systems of time-keeping. In the words of the *Explanatory Supplement to the Astronomical Almanac*:

> In principle, the time in the equations of motion of the Sun, moon, and planets could diverge from the time determined from observations of phenomena of terrestrial physics. At the present time, observational determinations are not sufficiently accurate to indicate such a systematic difference. If a true difference were detected, the scientific community would have to decide how to accommodate that difference. An ideal dynamical time can be determined only by analysis of observations over an extended period of time.[47]

The patience of astronomy is legendary; in the meantime there is no guidance, from this source, as to how the two are to be related, the business of quantum gravity. The problems of the late 19th century have only deepened with 20th century developments; whereas then classical mechanics (macroscopic, astronomical) stood in an unknown relation to electromagnetism (microscopic, terrestrial), now it is general relativity and quantum field theory that need to be reconciled. Moreover, one of the most difficult obstacles is exactly the question of how time is to be defined in a quantized reparameterization-invariant theory.

Hertz's problems and a part of his vision are still with us. In the centennial year of Hertz's death, it is a nice tribute that the favored standard of the IAU for all fundamental empirical determinations is now in effect an electromagnetic frequency; time as measured in Hertz. I would myself take the continuation of the above quotation, the final words of the *Introduction*, as Hertz's philosophy, as the statement of what he tried to achieve, and as the goal of every physicist of his or any other time:

But in order to arrive at such a decision it is first necessary to consider thoroughly the existing possibilities in all directions. To develop them in one special direction is the object of this treatise, – an object which must necessarily be attained even if we are still far from a possible decision, and even if the decision should finally prove unfavourable to the image here developed. (PM 41)

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NOTES

1 Willard van Orman Quine, *Theories and Things* (Cambridge: Belknap Press, 1966), pp. 84f. Cf. Rudolf Carnap, *The Logical Structure of the World* (Berkeley: University of California Press, 1967); Carnap cites Hertz's Introduction to PM in section 161, p. 256f.

2 Cf. Simon Saunders, 'To What Physics Corresponds,' in Steven French and Harmke Kamminga (eds.), *Correspondence, Invariance, and Heuristics* (Dordrecht: Kluwer, 1993), pp. 295–325.

3 There is no doubt here that Hertz held back from the dogmatic claim of underdetermination with respect to *all possible* evidence; he explicitly declares that the two theories cannot both be correct in *this* sense (PM 40f.).

4 Ray Monk, *Ludwig Wittgenstein: The Duty of Genius* (New York: Free Press, 1990), p. 26.

5 Since ratios in the cardinality of infinite sets are undefined, Hertz in effect simply takes mass-density as a primitive notion. Hertz has in effect two quite different ideas: the mechanics, dealing with finite (or at most countable) degrees of freedom, and field theory. His uncountable infinities of mass points were a half-hearted attempt at a synthesis. Lützen is surely right to trace this back to Hertz's underlying commitment to the geometric continuum, but wrong I think in his claim (1995b, pp. 61f.) that the real reason is to do with an additivity condition on the curvature, for this involves only the material particle masses. The problem is how to take the internal structure of material particles into account, assuming the mass distribution is smooth, and it is at this level that Hertz's notion of infinite collections of "material points" is supposed to come into play.

6 In modern terminology they are better understood as differential forms. The point just made, that constraints are a "purely geometric" notion, carries over to the fact that the exterior algebra of differential forms depends only on the manifold structure, independent of the affinity and metric altogether. This could do as a summary of Hertz's entire program, as also its shortcomings.

7 Hertz made only two criticisms of the standard theory, apart from the concluding paragraph of the Introduction (which hinted at its inadequacies in the field of electromagnetism). One of them focused on the fact that in the usual mechanics it was necessary to introduce "hidden" forces, all of which eventually canceled. This was linked to a specific criticism of the Newtonian theory of gravity, namely that it inevitably leads to the microscopic (because every part of each body is subject to gravitational attraction to every part of every other body). I shall come back to the other criticism later.

8 There are, however, serious difficulties in reconciling this with Kepler's laws, as soon pointed out by Huygens. For further background I refer to the beautiful and accessible study by J. Barbour, *Absolute or Relative Motion?* (Cambridge: Cambridge University Press, 1989), volume 1. My debt to Barbour's work will become quite obvious in what follows.

9 René Descartes 'Principles of Philosophy,' in J. Cottingham, R. Stoothoff, and D. Murdoch (transls.), *The Philosophical Writings of Descartes* (Cambridge: Cambridge University Press, 1985), section III. 30.

10 The important difference, *vis à vis* Galileo and Descartes, concerned the concept of vacuum and the notion of free motion: for the early atomists, "falling"; for Galileo, "uniform circular motion"; for Descartes, "uniform rectilinear motion." As we shall see, the early atomists were nearer the mark.

11 His geometric approach had certain original features, in the detailed mathematical development, but the real thrust of these lay more in the transition to Riemannian geometry, an aspect that Hertz played down (in contrast to e.g. Zöllner, or Clifford). Within the more limited context of mathematical methods (in particular differential geometry), Darboux was the more successful, and has historical priority. For formal aspects of Hertz's work, I refer to the comprehensive account of Lützen 1995b.

12 The similarities between Hertzian and Cartesian mechanics have been pointed out before (Mach 1960, R. Cohen 1956), but this implication went unremarked.

[13] No one has succeeded in generating the inverse square law by the former method (with all masses "visible"); neither has anyone made progress with the latter method, but certainly there is more leeway given "hidden" masses.

[14] The two are sometimes conflated in introductory texts; the tradition goes back at least to the time of Hertz, although he himself carefully distinguished the two (Poincaré did not; in his putative derivation of the 3rd law from the principle of relativity and conservation of energy, he actually derived the law of conservation of momentum; see Henri Poincaré, 'La Théorie de Lorentz et le Principe de Rêaction,' *Archives Néerlandaises des Sciences Exactes et Naturelles*, 2nd series **5** [1900], pp. 252–278).

[15] Hertz considered r rigid constraints $\delta q_j - \delta Q_j' = 0, j = 1, \ldots, r$ and remaining time—independent constraints $\Sigma_k c_{ik}\ \delta q_k = 0, i = 1, \ldots, s \geq r, \Sigma_j C_{kj}\ \delta Q_j = 0, k = 1, \ldots, S \geq r$. Treating the system as a composite, for the coordinates not rigidly connected we obtain the equations of motion $m_i\, \ddot{q}_i = \Sigma_{k=1}^{s}\ \lambda_k c_{ki}, i = s{+}1, \ldots, n$ and $m_j \ddot{Q}_j = \Sigma_{k=1}^{s}\ \Lambda_k C_{kj}, j = S{+}1, \ldots, N$. For those subject to the rigid constraints as well, we have $m_i\, \ddot{q}_i = \Sigma_{k=1}^{s}\ \lambda_k c_{ki} - \mu_i$ and $M_i \ddot{Q}_i = \Sigma_{k=1}^{s}\ \Lambda_k C_{ki} + \mu_i$, i.e. with the same undetermined multiplier μ_i in each, but with opposite sign. Using the same interpretation as before, these are identified with the forces of action and reaction.

[16] Isaac Newton, *Mathematical Principles of Natural Philosophy* (Berkeley: University of California Press, 1934), p. 25.

[17] Likewise the argument from collisions is independent of the inertial structure, supposing the time of impact can be as small as is desired.

[18] It is when the two bodies (A and B) are strictly contiguous, and the "interposed obstacle" is an infinitesimal volume element which contains their common boundary, that the mass can properly be taken to zero. If the forces are non-zero, they had better cancel if there is to be any such thing as a stable body, to even the crudest approximation.

[19] The passage cited concludes "[...] and will make the system of the two bodies, together with the obstacle, to move towards the parts on which B lies; and in free spaces, to go forwards *in infinitum* with a motion continually accelerated; which is absurd and contrary to the first Law." This is also correct, but now depends on the first law and hence the inertial structure.

[20] For his second example of a difficulty in the standard theory, Hertz (PM 5f.) gave the embarrassingly bad argument that when a stone is whirled in a sling, although the centrifugal force is supposed to be fictitious, it is nevertheless one and the same as the force of reaction acting on the hand (which is perfectly real). Mach kindly commented that mistakes like this do appear in "logically defective expositions, such as Hertz doubtless had in mind from his student days" (1960, 319); the centrifugal force not only acts on a different body (the stone), but is relativized to a different frame of reference (the co-rotating frame).

[21] This is the subject of Karl Popper's classic 1957 paper 'The Aim of Science,' reprinted in his *Objective Knowledge* (Oxford: Clarendon, 1972), pp. 191–205; for a balanced appraisal, see I.Bernard Cohen's 'Newton's Theory vs. Kepler's Theory and Galileo's Theory,' in Yehuda Elkana (ed.), *The Interaction Between Science and Philosophy* (Atlantic Highlands: Humanities Press, 1974), pp. 299–338.

[22] This step was made in Section XI of Book 1 of the *Principia* (previous sections assembled results on central forces referred to a fixed origin). In the introductory remark Newton appealed explicitly to both the third law and Corollary IV of the Axioms (conservation of momentum).

[23] The dependence of the force law on the mass is a more complicated matter; according to Hertz, this will enter at the level of the undetermined multipliers, the λ's (since the c's are functions only of the coordinates); whether it is possible to obtain independence of acceleration from mass on Hertzian lines, as required by Galileo's arguments, appears to depend on the details of the model, which of course we do not have. Using Newtonian concepts this step amounts to assuming constant proportionality of gravitational and inertial mass, a *consequence* of the equivalence principle as formulated by Einstein.

[24] Ptolemy (and before him Hipparchus) had doubted the constancy of stellar motions; it was on the basis of his observations that, fifteen centuries later, Halley could report that Acturus and Sirius had moved southwards by one minute of arc.

[25] Cf. Newton, *op.cit.* (note 16), Axioms, Corollary VI; unfortunately the argument is marred by a reference to "equal accelerative forces," rather than the linear acceleration of the relative space.

[26] Lange actually proved a slightly more subtle result, bearing on whether any frame at all could be found in which given arbitrary motions would count as uniform straight-line

motions, at least for short timescales. The answer is that it cannot, so long as there are more than three non-collinear bodies. Hertz did not refer to either Lange's work or to that of Neumann, of which it was an extension. In the preface he does refer to Mach's *Science of Mechanics* ("in a general way I owe very much to Mach's splendid book," PM xl), but to the first edition of 1883. Here Mach was dismissive of Neumann's work, and obviously made no mention of Lange.

[27] A theory formulated in this way is called "reparameterization-invariant," where there is no "external" time parameter tacked on to it. Just as all the known classical theories of dynamics can be written in a generally-covariant form, likewise they can all be formulated as reparameterization-invariant, so long as the time and frame of reference is dynamically determined using the theory in question. The same is in principle true for Hertz's theory.

[28] For further background to the history see B. L. Gurney and R. v. d. R. Woolley (eds.), *Explanatory Supplement to the Astronomical Ephemeris and the American Ephemeris and Nautical Almanac* (London: Her Majesty's Stationary Office, 1961), section 3D.

[29] Quine, *op.cit.* (note 1), p. 85, replacing "experience" by "motion".

[30] H. Goldstein, *Classical Mechanics* (Reading: Addison-Wesley, 1950), p. 14.

[31] Anneliese Maier, *Die Vorläufer Galileis im 14. Jahrhundert*, 2nd edition (Rome: Edizioni di Storia e Litteratura, 1966), p. 23.

[32] This actually seemed to be what FitzGerald had in mind in his review (FitzGerald 1895) when he enquired, without a trace of irony, whether all the various rigid connections might not foul one another in the motion of the planets.

[33] Descartes, *op.cit.* (note 9), section II.13.

[34] A. Rupert and Marie Boas Hall (eds.), *Unpublished Scientific Papers of Isaac Newton* (Cambridge: Cambridge University Press, 1978), p. 130.

[35] I count my Newton by numbers. The *Principia* consists of 110 Theorems, 82 Propositions, and 47 Lemmas. There are also the eight Definitions, the three Axioms, and the six Corollaries. Apart from the four Rules of Reasoning and the six Phenomena – essentially Kepler's laws – we have a large number of Scholia. There are exactly two Hypotheses, both in Book 3. The second was the rather obscure suggestion that did the earth consist only of a ring of the same mass, at the equator, then the motion of the equinoctial points would be the same if the ring were fluid as were it rigid. The first is the single sentence: "That the center of the system of the world is immovable." It is followed by the sole remark: "This is acknowledged by all, while some contend that the earth, others that the sun, is fixed in that center. Let us see what may from hence follow." Immediately on this we have Proposition 11, that the center of mass of the solar system is immovable – after which he had no more to say on the matter.

[36] Alan Gabbey, 'Descartes's Physics and Descartes's Mechanics: Chicken and Egg?' in Stephen Voss (ed.), *Essays on the Philosophy and Science of René Descartes* (New York: Oxford University Press, 1993), pp. 311–323.

[37] *Ibid.*, p. 320.

[38] Michael Friedman, *Kant and the Exact Sciences* (Cambridge: Harvard University Press, 1992).

[39] This is to be contrasted with Friedman's purely formal notion of geometric construction, which is clearly at variance with Kant's intended use of construction in *singular* intuition. (I am indebted to Friedman himself for this observation.)

[40] Here Kant was rather more explicit than Newton, and strongly emphasized that there could be no limit to the process that begins with the earth-moon system. In the "General Observation on Phenomenology" in the *Metaphysical Foundations* he attempts a reconciliation of Cartesian relativism with Newtonian theory. Whatever the muddles, there are genuine insights as well, which in certain respects went deeper than Mach's.

[41] Immanuel Kant, *Critique of Pure Reason*, A 215/B 262.

[42] Recall that the light-quantum paper of 1905 *preceded* his discovery of special relativity. For further discussion, see Simon Saunders and Harvey Brown, 'Reflections on Ether' in S. Saunders and H. Brown (eds.), *The Philosophy of Vacuum* (Oxford: Clarendon Press, 1991).

[43] More precisely, consider: (R) Relative velocities of source and observer alone are what matter; (S) Light-speed is independent of the motion of the source, with the observer at rest; (O) light-speed is independent of the motion of the observer, with the source at rest. The logical relations among these are as follows: $R \rightarrow [S \leftrightarrow O]$, $[S \;\&\; O] \rightarrow R$, whilst S & O is light-speed invariance. S is true on the ether theory but R is false, whilst S is false on a

ballistic theory but R is true. Einstein's actual postulates were R and S, yielding O and hence light-speed invariance.

44 There is no difficulty in giving an operational definition of Absolute simultaneity according to Newtonian principles. The radar method, with ballistic signals defined in terms of perfectly elastic collisions, is an example. There is no need to appeal to infinite particle velocities; what would happen in practice is that discrepancies would eventually arise as relative velocities become large in comparison to light-speed (the "absolute" simultaneity obtained in this way, given slow relative velocities – as with planetary and lunar motions – is approximately the same as the simultaneity associated with any one of the rest frames of the particles involved, using Einstein's principles).

45 We do obtain a global coordinate system, but the conditions on this are not quite so simply expressed as in the Newtonian case. One of them is that the line element takes on the Schwarzschild form, including in the mass a tidal potential for bodies outside the solar system; but the 1991 resolution of the IAU (International Astronomical Union) also recommended that spatial grids of the barycenter of the solar system show no global rotation with respect to a set of distant extragalactic objects; for a summary see P. Kenneth Seidelmann (ed.), *Explanatory Supplement to the Astronomical Almanac: A Revision to the Astronomical Ephemeris and the American Ephemeris and Nautical Almanac* (Mill Valley: University Science Books, 1992), pp. 45–48. For this reason the coordinate system is not completely dynamical.

46 The standard of length has long been fixed in terms of time, so that space and time standards are now both determined through counting operations.

47 Seidelmann, *op.cit.* (note 45), p. 63.

ALFRED NORDMANN

"EVERYTHING COULD BE DIFFERENT": THE *PRINCIPLES OF MECHANICS* AND THE LIMITS OF PHYSICS

In his lectures on "Science as a Vocation" Max Weber portrays the scientist as an ascetic whose heroism is one of self-denial.

> This is the fate and indeed the *point* of scientific work [...]: every scientific "fulfillment" implies new questions and *wants* to be "surpassed" and become obsolete. All who want to serve science have to resign themselves to this. [...] We cannot work without hoping that others achieve more than we did.[1]

While seeking the fulfillment of discovery, Weber's scientists place their personal accomplishments into the wider perspective of history and community. The conviction of having gotten it right and the celebration of a privileged access to nature have no permanent place in science but play only an ephemeral and transient role. Any expedition into unknown territory will and must eventually return into the highly constrained and qualified disciplinary fold.

Behind Weber's sketch may lie a full-fledged conception of the dynamics of inquiry or of the motivational fabric of scientists. His view may serve as a sociological complement to epistemological accounts of science.[2] For present purposes, however, it typifies only the ascetic heroism of a scientist whose social and philosophical background was very close to Weber's own. It will be shown that Heinrich Hertz's view on physics as a vocation, his conception of the dynamics of inquiry, his epistemology, and perhaps even his motivational fabric uphold Weber's ascetic ideal. Especially in his famous Introduction to the *Principles of Mechanics* Hertz exemplifes the interplay of advance and retreat, of physical intuition and epistemological restraint, of realist impulse and conventionalist qualification. He uses the Introduction to temper the straightforward empirical claim that, as opposed to his predecessors, he may simply have gotten the principles of mechanics right. Instead of speculating beyond the known limits of physics he deliberately contains his proposal within these limits. This public display of temperance recommends his view also by recommending its author: Hertz's *Principles of Mechanics* ought to be taken seriously insofar as Hertz himself strikes such a careful balance between a claim to novelty and an appreciation of tradition.

Aside from everything else that Hertz accomplishes in this Introduction, his exemplary exhibition of self-mastery also reinforces a particular conception of science.[3] On this conception, discovery is vital for progress and the advancement of science; as such it constitutes the true goal or meaning of science. At the same time, however, discovery is threatened by theory and thus by science itself: insofar as

D. Baird et al. (eds.), Heinrich Hertz: Classical Physicist, Modern Philosopher, 155–171.

science is inescapably theoretical, any discovery can give rise to a variety of theoretical representations, and the negotiation of these representations draws physicists into sterile debates, detracting from the goal of discovery.[4] In presenting his own view of mechanics as a possible choice from among a variety of empirically adequate images, Heinrich Hertz thus shoulders the fate of physics.

1. PHILOLOGICAL VS. PHILOSOPHICAL ASPECTS OF SCIENCE

Soon after he arrived in Berlin to work in the laboratory under his teacher Hermann von Helmholtz, the student Hertz wrote to his parents about the joy of learning "directly from nature." Even though he worried that "this pleasure will soon turn into habit and will then be no more," he would rather learn for himself and for others directly from nature than learn only for himself from what others have already said: "As long as I work only from books I cannot rid myself of my feeling that I am a wholly useless member of society" (MLD 97–99).

These sentiments are echoed almost ten years later in another letter to his parents. In the fall of 1887 and the spring of 1888 Hertz had finally achieved a major experimental breakthrough. He showed electromagnetic effects propagating through air and thus provided experimental proof for Maxwell's theory. He had left the world of books behind him and found himself "alone with nature":

> In my work I now have the comfortable feeling that I am so to speak on my own ground and territory and almost certainly not competing in an anxious race and that I shall not suddenly read in the literature that someone else had done it all long ago. It is really at this point that the pleasure of research begins, when one is, so to speak, alone with nature and no longer disputes about human opinions, views, and demands. To put it in a way that is more learned than clear: the philological aspect drops out and only the philosophical remains. (MLD 255)

Hertz defends this vision of his accomplishment in his 1889 lecture 'On the Relations between Light and Electricity.'

> Our experiments were carried out at the summit of the arch which, according to [Maxwell's] theory, connects the domain of optics with that of electricity. It was only natural to move a few steps further and to attempt the descent into the known domain of optics. There may be some advantage in putting theory aside. There are many lovers of science who are curious as to the nature of light, but to whom Maxwell's theory is nevertheless a book with seven seals. The economy of science, too, requires of us that we should avoid roundabout ways when a straight road is available. If with the aid of our electric waves we can directly exhibit the phenomena of light, we shall have no need for theory as an interpreter; the experiments themselves will clearly demonstrate the relationship between the two fields. (Misc 324)[5]

As in the letter to his parents, Hertz presents his accomplishment as an experimental exhibition which leaves theory behind and follows a straight road on which he can move without mediation or interpretation. His experiments allow him to exhibit the phenomena directly without recourse to disputable human opinions.

After more than ten years of laboratory work Hertz had reached the point where "the pleasure of research" begins. Indeed, he savored the sense of being "alone with nature" as a rare and privileged moment in his career. Books and human opinions would soon catch up with him and theoretical considerations would contest the ground which he had just claimed for himself. Even before he left Karlsruhe in the spring of 1889 and throughout his last years in Bonn, Hertz would complain that his experimental successes were creating a new set of obligations which stood in the way of further experimental work.

> The next task is to follow up victory, and that is more difficult than the victory itself, where good luck certainly plays a major part. The work itself is a hard taskmaster; I am expected to elaborate the thing once more, to lecture about it, to demonstrate the experiments to others; formerly these things were done with and out of the way once they were published. (MLD 283)

Not long thereafter the privileged moment dissipated entirely as Hertz found his experimental conclusions explicitly contested by Marie Alfred Cornu. Hertz's student Vilhelm Bjerknes describes this as an unpleasant, but inevitable set-back which apparently took a heavy psychological toll on Hertz (Bjerknes 1923, xvf. and xxif.). According to Bjerknes and Hertz himself, it was not the substance of Cornu's criticism which constituted this setback. After all, the criticism was based upon the phenomenon of "multiple resonance" which had been observed and set aside by Hertz himself and which had gained prominence in the work of Eduard Sarasin and Lucien de la Rive. Hertz "regarded the phenomenon as a consequence of the rapid damping of the primary oscillation – a necessary consequence and one that could be foreseen." However, although Sarasin "received my explanation with the readiest goodwill, we did not succeed in coming to a common understanding as to the interpretation of the experiment" (EW 16f.). Cornu's criticism was possible once competing interpretations of Hertz's experimental phenomena appeared. Hertz was thus reminded that in the dispute of human opinions, views, and demands one is never alone with nature but in a community of contestants who stand at a remove from nature. He formulates this notion in his 1891 tribute to Helmholtz.

> Within our consciousness we find an inner intellectual world of conceptions and ideas; outside our consciousness there lies the cold and alien world of actual things. Between these two stretches the narrow borderland of the senses. (Misc 335)

The epistemological metaphor of nature and mind as two separate worlds is deeply at odds with the previous exultation of being alone with nature and of the experiments themselves exhibiting directly the relations between electricity and light.[6] This is not to say, however, that Hertz was in any way philosophically confused. A post-Kantian epistemological conviction allowed him to anticipate and account for the inevitable fate of his discoveries. They were destined to ultimately figure as some among many items in the cold and alien world of actual things, a world to which there is no immediate access but which needs to be interpreted from the inner world of conceptions and ideas through the borderland of the senses. All the discoveries of physics share this destiny, and knowledge of that serves as an intellectual restraint on the naive realism which Hertz indulged in his experimental pursuits. Inversely, the experimental scientist had to restrain or set aside his own epistemological convictions in order to perform successfully and to savor the liberating joys of discovery.[7]

Vilhelm Bjerknes devotes much of his acute intellectual profile to the two "principles of self-mastery" [*Prinzipien der Selbstbeherrschung*] which he had learned from his teacher Hertz. Both principles demand that for some limited time the experimentalist should put certain worries aside, worries about the expediency of the experimental setup or worries about the theoretical framing of a problem. Within these limited parameters one should push for results. If they are achieved "you will feel liberated and relieved and you will have fresh strengths for the next task." At the same time, "you can now assess whether it is desirable to repeat the whole work with improved methods" and you can now worry about the theoretical considerations that had previously been excluded (Bjerknes 1923, xif.).

Allow yourself to be a naive realist for the limited purposes of experimentation and discovery, Hertz appears to suggest, but beware of straying from the proper path of science by failing to regain a qualifying epistemological perspective on your accomplishments. The mastery of nature requires the self-mastery of the scientist, the duty to discovery has to be checked by the willingness to transform novelty into an object for theoretical negotiation. While discovery holds the promise of letting nature speak directly for some theories and against others, that same discovery will soon give rise to a variety of theoretical interpretations as the resourceful world of ideas and conceptions quickly catches up with the world of actual things. Only circumspect self-mastery allows the scientist to first indulge a wishful naive realism and then exercise a physically sterile epistemological restraint. It prevents scientists from blindly following the "deeply human drive of 'on and on'" through the "labyrinthine ducts of the unknown until one no longer knows where one is" (Bjerknes 1923, xi). The sobering return to the world of conceptions, ideas, and competing theoretical interpretations serves also to keep the community of scientists together, to ensure and maintain the ways of doing science. Hermann von Helmholtz therefore praises the "self-denying diligence" of the scientist,[8] and so does Max Weber. Heinrich Hertz exemplifies it most dramatically in his Introduction to the *Principles of Mechanics*.

2. ORDINARY MECHANICS

Vilhelm Bjerknes speculates what the fate of the *Principles of Mechanics* might have been had Heinrich Hertz lived longer:

> No one who ever had any insight into his ways of thinking can doubt that we here suffered irreparable losses. For nothing was more foreign to him than to work in a formally logical manner, no matter how well he was able to do this. His expansive imagination always worked with concrete images of which he had an inexhaustible store. This concretely working imagination was at the source of his successes, in the domain of theory no less than in the domain of experiment. (Bjerknes 1923, viii)

As Bjerknes suggests, the *Principles of Mechanics* is universally considered an exercise "in a formally logical manner." And yet one need not speculate about the work left undone by Hertz himself in order to see how this book, too, results from his "expansive imagination" working "with concrete images." The *Principles* is testimony to an imagination which was getting ahead of itself and "might have posed a danger" had Hertz not checked it by framing its accomplishments within a conventionalist conception of theory-choice.[9] While Hertz's particular proposal simply sets out to provide a physically more intuitive account of mechanics, he recommends it not as an admirable product of his imagination, but humbly submits it to a process of theoretical negotiation which respects the historical and epistemological sensibilities of science as he conceives it.[10]

On this reconstruction, the origin of the *Principles* can be traced back as far as January 1878, soon after Hertz chose to become a scientist and even before he took up his studies with Helmholtz in Berlin.

> I also require much time to ponder over the matters themselves, and particularly the principles of mechanics (as the very words: force, time, space, motion indicate) can occupy one severely enough; likewise, in mathematics, the meaning of imaginary quantities, of the infinitesimally small and infinitely large and similar matters. (MLD 77)

Whether or not this early puzzlement inaugurated Hertz's systematic reflections on the principles of mechanics cannot be determined from the available documentary evidence. The primary reason for this lack of evidence is explained almost sixteen years later in one of Hertz's last letters to his parents.

> As [the *Principles*] is about to go to the printer, I have very much the feeling of "God protect the house." The book could easily reduce my by and large good repute to rack and ruin, "if the casting fail." Even a minor fault can make the whole sound wrong. And it is after all a somewhat anxious feeling to come out with something that I have never talked or consulted over with any human being. (MLD 343)

That Hertz never discussed this subject with anyone speaks to the intellectual isolation of a researcher who first attempts to answer Helmholtz's demands for discovery in the area of electromagnetics and who then becomes a fairly solitary physics professor in Kiel, Karlsruhe, and Bonn. However, it also suggests that his thoughts on mechanics are not immediately tied to the current research interests of his peers.

Starting with Hermann von Helmholtz (PM xxxvi), many of Hertz's readers have suggested that Hertz was working towards a theory of the ether. There are various indications, however, that Hertz was following up on his early puzzlement of 1878, concerning himself rather with the more elementary problem of how to clarify the basic concepts of ordinary classical mechanics.

In the first place, his preface to the *Principles of Mechanics* begins with the assertion that "it is premature to attempt to base the equations of motion of the ether on the laws of mechanics until we have obtained unequivocal agreement as to what is designated by this name" (PM xxxvii). His book is explicitly devoted to that latter, preliminary task. As such it cannot promise a theory of the ether; at best it introduces constraints which such a theory will have to satisfy.[11]

Secondly, in his 1889 lecture 'On the Relations between Light and Electricity' Hertz considers the ether as the medium of motion, a substance "which is able to support waves." Towards the end of the lecture he asks "whether all things have not been fashioned out of the ether." He declares this to be one of the "ultimate problems of physical science," and expresses faith "that future undertakings will achieve success" in their exploration (Misc 314, 326f.). The *Principles of Mechanics* introduces concealed masses as the hypothetical smallest constituents of systems, i.e., they are considered neither as the medium of motion, nor as a substantial substrate of bodies. Indeed, as opposed to the ether, the concealed masses are considered unobservable in principle. Whatever the current limits of observation, of weighing or manipulating masses, the concealed masses elude by definition all weighing and manipulation (PM §301). They are therefore not subject to exploration even by physical undertakings of the future.[12]

If the *Principles* does not propose a theory of the ether and if it is not immediately tied to the research interests of Hertz's peers, Hertz's expansive imagination may indeed have revolved entirely around the conceptual problems of ordinary classical mechanics. On November 27, 1891 Hertz insists on this in a letter to Emil Cohn:

> What you have been hearing about my work by way of Halle is unfortunately without any basis and I don't know how this opinion originated. I haven't worked on the mechanics of the electrical field at all, and haven't found out anything about the motion of the ether. This past summer I reflected a lot about ordinary mechanics, but I don't remember speaking about this in Halle at all. Here I would like to put some things in order and to determine the order of concepts in such a manner that one can see more clearly what is definition and what empirical fact, e.g., in the concept of force, of inertia, etc. I am already convinced that great simplifications are possible here. For example, as to what a mechanical force is, I have only now clarified this in a manner which satisfies me. But I have neither written these things down, nor do I know whether they would later satisfy others, too. At any rate it is a matter which can only mature slowly.[13]

3. ON THE CONSTRUCTION OF MECHANICAL EXPLANATIONS

Hertz's Introduction to the *Principles of Mechanics* provides a comparison of three "images" of mechanics, the Newtonian image, a "Hamiltonian" image,[14] and Hertz's own proposed image. But this comparison of images does not pertain to theories or descriptive accounts of all motions in the universe. Instead it compares the principles from which Newton, "Hamilton," and Hertz might derive theoretical accounts of, e.g., planetary motion in the solar system. While numerous concrete phenomenological laws serve to articulate Newton's fundamental principles, Hertz's own account has remained unarticulated and, as it stands, fails to yield descriptive theories.[15] What all three sets of principles have in common, however, is that (i) each is necessary for deriving "the whole of mechanics [...] by purely deductive reasoning without any further appeal to experience" (PM 4), and that (ii) each is sufficient for the purpose of "exhaustively representing the contribution of experience to the general laws of mechanics" (PM §736).[16] The three images of mechanics are therefore treated and compared insofar as each embodies a set of instructions on how to construct a complete mechanical explanation.

Hertz gives some examples of what is wrong with the manner in which Newtonians and "Hamiltonians" devise mechanical explanations. These examples show how their conceptions run counter to his physical intuitions. He conceives with great facility that there might be "rolling without slipping" in nature and holds this against the "Hamiltonian" image, according to which one must show that "all so-called rolling without slipping is really rolling with a little slipping, and is therefore a case of friction" (PM 20). Similarly, Hertz's physical intuitions rebel against the Newtonian construal of planetary motion as analogous to the whirling of a stone on a string in a circle around the body. The forces which hold planets in their orbits "have never been the objects of direct perception [...] Nor do we expect in the future to perceive the forces" (PM 12). By construing the planetary orbits as analogous to the case where the forces "seem to be real," the Newtonian image ascribes to the mechanics of motion what belongs to the physiology of perception.[17]

Elsewhere in this volume Jesper Lützen suggests that on a non-Euclidean geometry "it would have been a simple matter" for Hertz to avoid the notion of infinitesimal material particles. However, Hertz would rather introduce the hypothesis of concealed masses than abandon the intuition that physical space should be conceived as Euclidean space (pages 111f above). Moreover, Hertz's assumption that there are unobservable masses which differ from observable bodies only in the one respect of being unobservable is more closely attuned to physical treatments than the customary, but even more costly alternative of introducing and proliferating occult or "hidden" forces.[18]

By thus referring to his physical intuitions, Hertz goes beyond a consideration of principles and the sorts of mechanical explanations which derive from them. Indeed, towards the very end of the Introduction he abandons the principles of mechanics altogether and looks directly at the derivations which he attributes to nature itself.

> The first [Newtonian] image can be said to assume as the final constant elements in nature the relative accelerations of the masses with reference to each other: from these nature occasionally derives approximate, but only approximately fixed relations between their positions. But the third [Hertz's] image can be said to assume as the strictly invariable elements of nature fixed relations between the positions: when the phenomena require it, nature derives from these approximate, but only approximately invariable relative accelerations between the masses. If only we could study the motions of nature with sufficient precision, we would know right away, whether the relative acceleration or the relative positions of masses or both are only approximately invariable in these motions. We would then also know immediately which one of our assumptions is false or whether both are false, for they cannot both be simultaneously correct. (PM 41)

In this way Hertz begins to treat the two images of mechanics as competing empirical research programmes, as testable theories of nature which will be found right or wrong in the course of further discovery:

> This is therefore the field on which the decisive battle must be fought between the various basic suppositions of mechanics which we considered here. (PM 41)[19]

Hertz's Introduction to the *Principles of Mechanics* thus concludes with an emphatic vision for the future of mechanics. Far from being an exercise "in a formally logical manner," the *Principles* recommends as physically more intuitive an account according to which the fundamental invariable elements in nature are not relative accelerations but fixed relative positions. Indeed, once this is considered the heart of the matter, the Newtonian system of mechanics can be subjected to ridicule:

We see a piece of iron resting upon a table, and we accordingly imagine that no causes of motion – no forces – are present there. Physics, which is based upon [Newtonian mechanics], teaches us otherwise. Through the force of gravitation every atom of the iron is attracted by every other atom in the universe. But every atom of the iron is also magnetic, and is thus connected by fresh forces with every other magnetic atom in the universe. But bodies in the universe also contain electricity in motion, and this exerts further complicated forces which attract every atom of the iron. And in so far as the parts of the iron themselves contain electricity, we have yet again different forces to take into consideration; and in addition to these various kinds of molecular forces. Some of these forces are not small: if only a part of these forces were effective, this part would suffice to tear the iron to pieces. But, in fact, all the forces are so adjusted among each other that the effect of this immense arsenal is zero; that in spite of a thousand existing causes of motion, no motion takes place; that the iron, after all, simply rests. Now if we place these conceptions before unprejudiced persons, who will believe us? Whom shall we convince that we are still speaking of actual things and not of fabrications by a freewheeling power of imagination? [...] [T]here can be no question that a system of mechanics which does avoid or exclude [these conceptions] is simpler, and in this sense more appropriate, than the one here considered. (PM 13)

4. EPISTEMOLOGY CONTROLS INTUITION

The *Principles of Mechanics* is not known as an articulation of Hertz's concrete physical intuitions, much less as a research proposal. Even its immediate critics praised it for everything but its practical import or its ambition to alter the course of physical research.[20] This is due in part to Hertz's emphasis on the principles of construction for a complete mechanical explanation. While this emphasis has practical implications for the conduct of physical research at the limits of observation, it has no practical import in all those situations where a complete explanation is not at issue, i.e., in regard to the "practical" needs of humankind:

[W]e have only spoken of appropriateness in a special sense – in the sense of a mind which endeavours to embrace objectively the whole of our physical knowledge without considering the contingent position of humans in nature, and to set forth this knowledge in a simple manner. The appropriateness of which we have spoken has no reference to practical applications or to the needs of humankind. (PM 40, cf. 10)

This disclaimer was easily conflated with the notion that Hertz provides a purely theoretical and epistemological reflection on mechanics. And this notion was reinforced, of course, by Hertz's manner of framing the issue. He presented his proposal only as one among three possible images of mechanics. Instead of appealing to the realist impulse towards discovery which presupposed a sympathetic audience, he cautiously prepared the ground by appealing to a vague but widely held neo-Kantianism which was a defining feature of the scientific community in Germany.[21]

Hertz set out to recommend his view not on the empirical ground of "correctness" but on the logical or conceptual grounds of "permissibility" and "appropriateness." To do so, he had to put the three images on a par with respect to their correctness, i.e., their empirical adequacy in regard to the known phenomena of motion. Thus he notes for the Newtonian image of mechanics, "No one will deny that within the whole range of our experience up to the present the correctness is perfect" (PM 9). For his own image he makes the similar claim that it "correctly represents [...] all natural motions without exception [...] in the sense that no definite phenomena can at present be mentioned which would be inconsistent with the system" (PM 36). In both cases he expressly limits the consideration of empirical correctness "to the range of previous experience: as far as future experience is concerned, there will yet be occasion to return to the question of correctness" (PM 9, cf. 36f.). These provisos indicate that the adopted mode of comparison will eventually be abandoned for an empirical comparison of research programmes. In light of Hertz's physical intuitions, this postponement of empirical considerations also shows that the adopted mode of conventionalist comparison exacts a price of self-denial and artifice. This price apparently becomes nearly impossible to pay in the case of the "Hamiltonian" image of mechanics. Though Hertz needs to say that its correctness is on a par with that of the other two images, he casts serious doubts on its empirical adequacy. If only for the purposes of his strategy of comparison he finally rules by *fiat* and against his intuitions that his doubt "is one which affects the appropriateness of the system, not its correctness, so that the disadvantages which arise from it may be outweighed by other advantages" (PM 21).[22]

Having with some effort construed all three images as empirically correct, Hertz compares them for their "permissibility" and "appropriateness." While the Newtonian image appears to be neither permissible nor appropriate, the "Hamiltonian" image despite its shortcomings fares well overall on the criterion of "appropriateness." By meeting both conditions, Hertz's image emerges as superior to both predecessors. Again, the conventionalist mode of argument exacts a price which the physicist Hertz apparently finds difficult to pay. For all his willingness to ridicule the Newtonian image and to expose its internal contradictions, he falls short of explicitly denying the permissibility of an image which, even as he writes, still shapes ordinary thought and talk about mechanics.[23]

Hertz's conventionalist strategy for recommending his image of physics takes up 39 1/2 pages of his 41-page Introduction. In effect and for posterity it established the intent of his book as a formal, academic exercise, namely as an epistemological evaluation of competing images of mechanics, devoted to showing the permissibility and appropriateness of the one proposed by Hertz himself. At the same time, this preponderant concern of his Introduction established his philosophical reputation and influence. His physical intuitions and empirical proposal were now so successfully controlled by an epistemology of choice, that the claim of new discovery at or beyond the limits of observation went unnoticed. After those 39 1/2 pages the long-deferred shift of gear to the direct empirical comparison appears as a sudden, unconnected, and inconsequential afterthought.[24] It is only through this last-minute shift of gear that the emphatic voice of Hertz, the experimentalist and discoverer announces itself:

In conclusion, let us glance once more at the three images of mechanics which we have brought forward, and let us try to make a final and definitive comparison between them. After what we have already said, we may leave the second ["Hamiltonian"] image out of consideration.[25] We shall put the first and third images [i.e. Newton's and Hertz's] on an equality with respect to permissibility [...]. We shall also put both images on an equality with respect to appropriateness [...]. We shall then have as our sole criterion the correctness of the images: this is determined by the things themselves and does not depend on our arbitrary choice. (PM 40)[26]

5. RESORTING TO CHOICE

So far, the epistemology of choice which governs Hertz's Introduction to the *Principles of Mechanics* has been described as a carefully qualified retreat from the more immediate claims of the research programme. Certain unresolved tensions in his text were said to indicate Hertz's ambivalence about this. A further exploration of this ambivalence reveals that Hertz does experience this retreat as a "loss of world" which counters the impulse towards discovery. It will also show, however, that he retreats to a philosophical position which is entirely his own and which he fully embraces. The conventionalist epistemology of choice imposes a limit on the power and expanse of physics. While the classical experimental physicist Hertz may wish to run up against this limit, the modern philosopher Hertz uses this delimitation to arrive at a definition of the scientist in the historical process of the "disenchantment of the world."

Max Weber introduces the notion of the "disenchantment of the world" as he elaborates the fate and destiny of the scientist who is bound to promote the progress of rationalization. In his letters to his parents, Hertz exhibits an acute awareness of this historical process. Very soon after becoming a student of physics in Munich, he writes,

I am burning with impatience to reach the frontier of what is already known and to go on exploring into unknown territory; but the road is terribly long [...] As one goes on, more and more questions arise and fewer answers [...] But that is the beauty of it, for discovery brings joy, nature explained seems almost less beautiful than the unexplained. (MLD 71)

Hertz here anticipates the disillusionment which he was to experience in the wake of his electrodynamic researches. The consummate moment of discovery and explanation is also a moment of disenchantment. The joyful exploration of unknown territory inevitably destroys the beauty, charm, uniqueness of the unknown and unexplained. In so far as the joy of discovery derives from a sense of wonder at the unknown, Hertz regrets the diminution of wonder that is effected by the progress of science:

[w]hat would be a greater miracle – a gnat one yard long, or a whale 1000 yards long? *num detur realiter* (!) *culex* [could there really be a gnat] whose one wing could cover the whole earth? Could there be a cold so great that words would freeze together? [...] Sometimes I really regret that I do not live in those days, when there was still so much that was new; to be sure, enough is yet unknown, but I do not think that it will be possible to discover anything nowadays that would lead us to revise our entire outlook as radically as was possible in the days when the telescope and the microscope were still new. (MLD 81)

Hertz's sense of regret was soon to be amplified, not only by his initial failures to make substantive discoveries[27] but also by his first encounter with the notion that physicists have to negotiate a variety of empirically equivalent "images." This first encounter concerned his 1881 manuscript 'On the Contact of Elastic Solids' (Misc 146–162) which was returned to him with extensive annotations by one of his teachers, Gustav Kirchhoff. In effect, Kirchhoff had rewritten much of Hertz's paper in a different mathematical notation.

> At first I was surprised and even flattered that Kirchhoff had gone over it so thoroughly, but apart from a wrong sign that I had indeed overlooked, his comments seemed only to say the same thing (and by no means better) that was in the paper. In part the points were expressed in a manner peculiar to Kirchhoff which I do not like at all, and which I should be very unwilling to have imposed on me. (MLD 147)

In spite of his growing annoyance, Hertz ended up "substituting his formulation for mine [...] although I do not believe that the paper will be any better for it" (MLD 149).[28] Eight weeks later Hertz is pleased to report that "in looking over my paper, I found that Prof. Kirchhoff himself had made the main error of which he had accused me (and which was only a matter of some unclarity in my presentation) and I have shown him that" (MLD 149).

This first encounter with two empirically equivalent modes of representation proved sterile and unproductive. Accordingly, it is unlikely that Hertz would willingly involve his peers in reflections on theoretical constructs which appear empirically undecidable. When he does so, after all, for the first time in his 1884 paper on the relation between Maxwell's and "the opposing" electromagnetics, he backs into this kind of theoretical deliberation only reluctantly. That paper begins, after all, with a section in which he derives "from generally accepted premises" (Misc 278) a theorem which is testable in principle but which "may not be capable of experimental verification" since the predicted actions "lie at the limits of observation" (Misc 276f.). The second section is devoted to a rather different, but related project. He wants to see how these predicted actions can be incorporated "into the usual system of electromagnetics." Since all researchers are implicitly committed to these actions, one's system of electromagnetics ought to predict them even if that requires making certain corrections to the usual system (Misc 278). After introducing these corrections, Hertz arrives at two sets of equations, and the system of forces given by these equations "is just that given by Maxwell" (Misc 288). It would seem, then, that he has derived Maxwell's system "as the most fitting from the standpoint of the usual system of electromagnetics." And this would indicate that Maxwell's equations can be derived "starting from premises that are generally admitted in the opposing system of electromagnetics" (Misc 289).[29] At this point, however, Hertz shifts gear again and abandons his suggestion that the one system can assimilate the other. Having arrived at an untestable prediction, having provided an incomplete demonstration of Maxwell's system from the opposing standpoint, he now frames the issue as a matter of choice: "[I]f the choice rests only between the usual system of electromagnetics and Maxwell's, the latter is certainly to be preferred" (Misc 289).[30] Hertz adduces three reasons in support of this choice. The first two are reasons of appropriateness (distinctness and simplicity), but the third characteristically returns to empirical implications. Maxwell's system is to be preferred because it predicts outright the previously identified and as yet unobserved action at the limits of observation. Hertz has thus arrived at a choice between two systems which, for the time being, he must consider empirically equivalent. But the overall development of the article makes clear that the issue of choice arises only after other strategies fail and in the absence of definitive empirical indicators. It marks a state of uncomfortable *stasis*, a state which condemns physicists to engage in epistemological evaluations.[31]

Hertz did not include this paper in his volume on *Electric Waves*. After he made the requisite discoveries at the limits of observation, this earlier approach became obsolete. It now represented a period of false starts, frustration, and a gratuitous appeal to epistemology.[32]

6. THE LIMITS OF MECHANICS

Hertz's spirit of discovery moves to explore the unknown and is at the same time concerned to limit the expanse of physics and to preserve a sense of wonder. When he runs up against a choice between empirically equivalent theoretical representations, he finds the business of physics limited to epistemological negotiations, and at the same time finds the territory of the known clearly delimited from the unknown that lies beyond the current limits of observation. In the case of the *Principles of Mechanics* this duality of aspects presents itself not as ambivalence or ambiguity; each aspect is realized and each increases the merit of his work. The choice between images of mechanics points towards the consideration at the very end of the Introduction, i.e., towards discovery and more accurate observations of natural motion. At the same time, the presentation of a choice among empirically equivalent images reinforces the notion that theoretical physics remains on one side of an unbridgable epistemological divide which leaves the wonders of nature largely untouched. It is this latter dimension of Hertz's argument which finally needs to be established.

Paragraphs 427 and 428 of the *Principles of Mechanics* emphasize a special feature of Hertz's account. Once hypothetical masses which are unobservable in principle are admitted into his system, it becomes "impossible to carry our knowledge of the connections of natural systems further than is involved in specifying models of the actual systems." This limitation renders his mechanics anti-realist. The systems considered in mechanics can agree with the real systems in nature only in one respect, namely "that the one set of systems are models of the other." Nature and mind are again conceived as separate worlds:[33]

> The agreement between mind and nature can therefore be compared to the agreement between two systems which are models of one another. And we can even account for this agreement if we wish to assume that the mind has the capacity to form and work with dynamic models of things.

Once hypothetical and hidden masses are admitted, knowledge of the world is limited to knowledge of the formal agreement between systems. The same holds whenever hypothetical and hidden forces are admitted into physics.[34] Thus a multitude of theoretical systems emerges, each agreeing with the system of nature. The mind cannot penetrate nature and the choice between theoretical systems can therefore be made only on the grounds of permissibility and appropriateness.[35] And what appeared as a special feature of Hertz's own image of mechanics becomes a feature of all three images considered as three competing models of nature.

Hertz's mechanics draws attention to conceptual confusions in opposing systems of mechanics, it develops physical intuitions to suggest an area of research at the limits of observation, and it finally limits all images of mechanics by drawing them into an epistemology of conventionalist theory-choice. Hertz presents this limitation as a virtue, especially in respect to the boundary between the problems of physics and the problem of life.

While it is not usual to treat of the problem of life in the customary representations of mechanics, "the complete vagueness of the forces introduced leaves ample latitude. One tacitly reserves the right to stipulate later on, for example, the contrast between forces in animate and inanimate nature" (PM 38). By avoiding "force" as a fundamental concept, Hertz's mechanics is explicitly restricted to inanimate nature and does not permit speculations about the relationship between, for example, inertial force and *elan vital*.[36]

> It seems to me that this is not a disadvantage, but rather an advantage of our law. Precisely because it allows us to survey the whole of mechanics, it thereby shows us the limits of this whole. Precisely because it only renders a fact without attributing to it the appearance of necessity, it thereby lets us know that everything could be different. (PM 38)

Hertz's mechanics provides a model of inanimate nature and thereby preserves a sense of wonder at the animate.[37] And since it draws other models into a conventionalist epistemology of choice, any system of mechanics becomes a contingent representation of contingent fact. Hertz's own mechanics and his comparison of different images of mechanics show that "everything could be different." This allows him to propose an alternative mechanics which agrees with his physical intuitions, and also divorces the problems of physics from the problem of life. Following his intuitions and pushing the limits of observation, Hertz runs up against the limits of physics characterized by the recurrence of sterile epistemological negotiations. But, though physically sterile, the comparison of systems that are models of one another also delimits physics in a positive manner by establishing the boundaries between nature and mind, between mechanics and the problem of life.

Max Weber quotes Tolstoy when he asks about the sense [*Sinn*] of science as a vocation:

> "It is senseless [*sinnlos*] since it does not give an answer to the only question that is important to us: 'What shall we do? How shall we live?'"[38]

And once science has clarified, for example, the notion of the divine across various cultures, "the discussion in the lecture-hall and by a professor has simply arrived at an end, whereas the implied huge problem of *life* [*Lebensproblem*] has, of course, not at all reached its end."[39]

This recognition is yet another aspect of Hertz's philosophical legacy. Under the impression of Tolstoy and of Hertz's reflections on mind and nature as dynamical models of one another, Ludwig Wittgenstein would note in remark 6.52 of his *Tractatus*:

We feel that even if all *possible* scientific questions have been answered, our problems of life [*Lebensprobleme*] have still not been touched at all.

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NOTES

[1] Max Weber, *Soziologie, Universalgeschichtliche Analysen, Politik* (Stuttgart: Kröner, 1973), pp. 315f.

[2] Mertonian sociology of science and Popperian philosophy of science can be said to explicate and universalize the ascetic heroism of Weber's scientist.

[3] This may sound like a "social deconstructionist" approach to Hertz and is definitely informed by such approaches. However, it also serves the purpose of "rational reconstruction" in that it reconciles otherwise incongruent aspects of Hertz's writings. I will argue that Hertz's introduction to the *Principles* brought to "literary life" a particular conception of physics.

[4] Jed Buchwald identifies Hertz's conception of physics with "what one might call the Berlin edict: 'Go forth and discover'" (1994a, 125). He does not explore the tension between theory and discovery.

[5] The experiments alluded to are the ones by which Hertz shows that his electric waves follow the same laws of reflection as do light waves.

[6] In his tribute to his teacher, Hertz sets out to describe Helmholtz's physiological researches as an attempt to explore the "borderland of the senses." However, it is clear to Hertz that such explorations cannot bridge the worlds of consciousness and of actual things. On his view the purpose of Helmholtz's researches is to keep these worlds "carefully defined and well divided", preventing us from making "the mistake of referring anything which belongs to [the borderland of the senses] to one or the other of the worlds it separates" (Misc 335f.).

[7] On Hertz's conception of science there is a time for theory and a time for discovery, i.e., Hertz does not view science in terms of a close and productive methodological interaction between theory and experiment. Cf. his letter of August 10th, 1889, to Oliver Heaviside: "Theory goes much further than the experiments, for the experiments hardly come to tell in a whispering voice what theory tells in clear and loud sentences. But I think in due time there will come from experiment many new things which are not now in theory, and I have even now complaint against theory, which I think cannot be overcome until further experimental help [sic]." Heaviside's response exhibits a rather different conception of science according to which there is a far more subtle relation between theory and experiment (O'Hara and Pricha 1987, 68 and 72).

[8] According to his essay on "The Aim and Progress of Physical Science," scientists in a "work-loving, frugal, moral" nation like Germany will sharpen their senses, train their hands, even display "the courage and the coolness of a soldier" as they learn to confine themselves within a highly specialized field of study. Most of all, German scientists are supposedly characterized by "a total lack of fear over the implications of the knowledge of the complete truth" (Helmholtz 1971, 245 and 223–225). Max Weber also emphasizes the manly willingness of scientists to accept the "uncomfortable" consequences of their research (*op. cit.*, note 1, p. 328). Helmholtz employs a revealing phrase when he comments on Hertz's death in his preface to the *Principles of Mechanics*: "All who regard human progress as consisting [...] in the dominion of the intellect over both natural passions and antagonistic forces of nature, must have learned with the greatest sorrow of the death of this highly gifted prodigy" (PM xxiii).

[9] "Expansive imagination" is a translation of Bjerknes's "*vorauseilende Phantasie*," i.e., literally of an imagination rushing ahead. On page xii Bjerknes notes that this wonderful imagination "might have posed a danger had he not worked out for himself the two principles of self-mastery."

10 What Gregor Schiemann describes in this volume as a "loss of world" emerges here as a strategy of legitimation; a neo-Kantian vantage-point which makes no extravagant claims on the world is epistemologically validated (and those who assume this vantage-point are in appropriate critical control of their intellectual passions).

11 The strongest evidence for this view arises from the conclusion of Hertz's Introduction. Hertz outlines the course of researches which may bring about an empirical validation of his conception of mechanics. Its final step will consist in "reducing the supposed actions-at-a-distance to motions in an all-pervasive medium whose smallest parts obey rigid connections" (PM 41).

12 But cf. Schiemann's contribution in this volume.

13 The letter is in the collection of the Deutsches Museum in Munich (# 3206). The entire paragraph reads as follows in German: "Was Sie über meine Arbeiten via Halle gehört haben, ist leider ohne alle Begründung und weiss ich nicht wie die Meinung entstanden. Über die Mechanik des elektrischen Feldes habe ich gar nicht gearbeitet und über die Bewegung des Aethers gar nichts herausbekommen. Ich habe in diesem Sommer viel über die gewöhnliche Mechanik nachgedacht, ich glaubte aber gar nicht davon in Halle gesprochen zu haben. Hier möchte ich gern etwas ordnen und die Anordnung der Begriffe so treffen, dass man viel klarer sieht, was Definition und was Erfahrungsthatsache ist, z.B. in dem Begriffe der Kraft, der Trägheit, etc. Ich bin auch bereits überzeugt, dass hier grosse Vereinfachungen möglich sind und z.B. was eine mechanische Kraft ist, habe ich mir erst jetzt in mich befriedigender Weise klargemacht, aber weder habe ich die Sachen aufgeschrieben noch weiss ich, ob sie nachher auch andere befriedigen würden. Jedenfalls ist es eine Sache, die nur langsam reifen kann. Experimentell habe ich in der letzten Zeit fast gar nicht gearbeitet. Die Arbeiten des Herrn Bjerknes haben mir viel Freude gemacht."

14 In the second image "we make use of one of the integral principles of ordinary mechanics which involve in their statement the idea of energy. It is not of much importance which of these we select; we can and shall choose Hamilton's principle" (PM 16).

15 Cf. the contribution by Simon Saunders in this volume.

16 Book I of the *Principles* is devoted to (i), book II shows that Hertz's conception of mechanics can satisfy (ii).

17 Cf. note 6 above. Not only Hertz's physical intuitions rebel against the Newtonian and "Hamiltonian" images of science. He also charges that their conceptions give rise to intellectual confusion. Hertz therefore uses the case of the stone on a string also to expose a tension between Newton's second and third laws of motion. While the second law requires us to consider "force" as the cause of changes in motion, the third law considers "forces" as resultant features in a system of mutual dependencies (PM 5f.). For another reading of Hertz's "embarrassingly bad argument" cf. Saunders in this volume, note 20.

18 This point was first made in Poincaré's review of Hertz's *Principles* (1952, 247). The introduction of concealed masses whose properties are like those of manifest bodies has a respectable pedigree which can be traced back to Descartes and beyond. Indeed, there are striking similarities to the physics of Lucretius.

19 Here and elsewhere throughout this paper, the English translations have been corrected. While I am not aware of any previous discussions of this passage, this volume affords a comparison with two other readings, namely those by Simon Saunders and Gregor Schiemann. For a further step in Hertz's suggested course of research see also note 11 above.

20 Cf. Joseph Mulligan's contribution to this volume. However, it is not altogether true that Hertz's *Principles* lack examples, cf. Boltzmann 1899 and Brill 1900b, Paulus 1916.

21 In one of the four extant drafts of the *Principles* (in addition to a corrected manuscript), Hertz explicitly refers to Kant (# 2845 at the Deutsches Museum in Munich, 43). Evidence for a general, albeit vague neo-Kantianism can be found in the works of Helmholtz, Kirchhoff, Hertz, Boltzmann, Mach, and many others who, according to Hertz, rejected the "false" and "fictitious natural philosophy" which had dominated science in Germany for the first half of the 19th century (Misc 332).

22 The advantage of the "Hamiltonian" image is its superiority in other aspects of "appropriateness" vis-a-vis the Newtonian image. Hertz suggests that a disadvantage in "appropriateness" can be canceled by an advantage elsewhere. In regard to deficiencies of "correctness" Hertz implicitly denies here that they could be canceled by other kinds of "advantages." He is thus doing the "Hamiltonian" image an immense [*entgegenkommend*] favor when he decides that "we should prefer to admit that the doubt is one which affects the appropriateness of the system." The doubt in question concerns the requirement of the "Hamiltonian" image that the process of rolling without slipping does not occur in nature.

According to Hertz, this requirement should not be construed as "only approximately realised in nature" but leads "to results which are entirely false." But when he chooses to view the matter as one of appropriateness, he suppresses this intuition and chooses, after all, to view the Hamiltonian requirement as approximately realised in nature. When Hertz finally leaves the mode of conventionalist theory-choice and embarks on his comparison of empirical correctness, he is no longer constrained to consider the "Hamiltonian" image as possibly correct but readily dismisses it (PM 40).

23 The Newtonian image confuses our ideas (PM 6), even gives rise to "painful contradictions" (PM 8, cf. also 10): "I fancy that Newton himself must have felt this embarrassment when he gave the rather forced definition of mass as being the product of volume and density" (PM 7). Since Hertz defines as inadmissable "all images which implicitly contradict the laws of our thought" (PM 2), this would indeed throw "such strong doubts upon the permissibility of this image that it might appear to be my intention to contest, and finally to deny, its permissibility" (PM 8). However, Hertz does not wish to go as far as this: "all indistinctness and uncertainty can be avoided by suitable arrangement of definitions and notations, and by due care in the mode of expression" (PM 9, cf. also 40). Hertz's artful wavering on this issue gives rise to conflicting interpretations (cf. e.g. section 3.1 and note 81 of Klaus Hentschel's contribution to this volume).

24 The problem was slightly compounded by the English edition and all reprintings of Hertz's introduction. In corrections of his manuscript (# 2853 at the Deutsches Museum in Munich) he had requested that each of the three main sections and the concluding remarks be set off from the preceding text by 1/4 to 1/3 blank page. His conclusion was to stand separately in that it introduced an entirely different consideration of the issue. In the English edition the concluding section runs on as yet another paragraph. The translation of "*endgültigen Vergleich*" as "conclusive" rather than "definitive" comparison also shrouds the shift of perspective and suggests that the last pages present a (somewhat incongruent) synoptic summary.

25 Cf. note 22 above.

26 Here follows the passage about relative accelerations and absolute rest which was quoted above, cf. also note 19.

27 These are detailed in Buchwald 1994a, e.g. 124–130.

28 Buchwald 1994a, 108f. reproduces a page from Hertz's original and a sample of Kirchhoff's corrections. Buchwald notes that a large proportion of the published paper was actually formulated by Kirchhoff.

29 Buchwald 1985, 187–193 has shown that the two systems are incommensurable and cannot be amalgamated. At best, Hertz's paper represents his attempt to work from Helmholtzian "interaction physics" towards Maxwell's field-theory. On Buchwald's reading, the body of the *Principles* may represent another step in the same direction.

30 The meaning and importance of this statement is controversial. Salvo D'Agostino, for one, interprets it as a commitment for Maxwell's and against the "usual" electromagnetics (which includes the unamended formulations by Helmholtz). On his interpretation, Hertz is here adopting a research agenda which would eventually lead him through his electrodynamic experiments to his "proof" of Maxwell's theory (cf. 1975 and his contribution to this volume, cf. also e.g. Joseph Mulligan's editorial comments in Mulligan 1994a). According to Manuel Doncel (this volume) and Jed Buchwald (1990, 1994a, and 1994b) Hertz discovered the Maxwellian implications of his electrodynamic researches only very late in the course of experimentation. On their interpretation, the present statement has to be read as a counterfactual. In light of what he had done in section 2 of his paper, there is no longer a choice "only" between the "usual" and the Maxwellian electromagnetics: Hertz had already chosen a Helmholtzian framework, (mistakenly) satisfied that he had established its convergence to Maxwell's equations. Cf. also O'Hara and Pricha 1987, 3 and 8–11; Gerhard-Multhaupt 1988.

31 This case underscores Klaus Hentschel's claim that the epistemology of choice is activated at particular points of theoretical impasse (cf. his contribution to this volume). It is less clear that the choice between images of mechanics also represents a response to crisis.

32 For a discussion of Hertz's subsequent disregard of his 1884 paper, cf. Salvo D'Agostino's and Manuel Doncel's contributions to this volume.

33 Cf. page 157 above.

34 Cf. again Poincaré 1952, 247.

35 Cf. Gregor Schiemann's contribution in this volume.

[36] Hertz's fundamental law "is too simple and narrow to account for even the lowest processes of life" (PM 38). Paragraphs 320 to 322 discuss the relation between animate and inanimate nature. Hertz suggests that since no contradictions between animate and inanimate systems are known (animate systems also obey the laws of motion, for example), it may always be possible to insert [*unterschieben*] an inanimate representation into an animate system such that, for given mechanical problems, the inanimate system can substitute for the animate (PM §321). This discussion figures prominently in a study by Richard Manno on Heinrich Hertz, mechanics, and freedom of the will (Manno 1900).
[37] "The same feeling which impels us to exclude from the mechanics of the inanimate world as foreign every indication of an intention, of a sensation, of pleasure and pain, – this same feeling makes us hesitant to deprive our image of the animate world of these richer and more colorful conceptions" (PM 38, cf. §320). For reasons of maintaining appropriate boundaries Hertz declines in 1891 Oliver Lodge's invitation to become a member of the Psychical Society (O'Hara and Pricha 1987, 98). Cf. also Hertz's previously mentioned reluctance to follow Helmholtz's reflections on the physiology of the senses (note 6 above).
[38] Max Weber, *op.cit.*, note 1, p. 322.
[39] Max Weber, *op.cit.*, note 1, p. 329.

JOSEPH F. MULLIGAN

THE RECEPTION OF HEINRICH HERTZ'S *PRINCIPLES OF MECHANICS* BY HIS CONTEMPORARIES

INTRODUCTION

For present-day physicists Heinrich Hertz's *Principles of Mechanics* is a neglected, almost forgotten, book. For example, few recent textbooks on mechanics make even passing reference to Hertz's fundamental law [*Grundgesetz*] of the straightest path, which is the foundation of his *Principles of Mechanics*.[1] Since Hertz's *Electric Waves* had received an enthusiastic reception from physicists when it first appeared in 1892, and is still important today, it is difficult at first to understand this lack of interest in his book on mechanics, which appeared just two years later. This paper suggests that a major factor in the neglect of Hertz's *Mechanics* was the unenthusiastic and often quite negative response to his book by some of the most important physicists of his time.

To support this thesis we have chosen as our critics four eminent theoretical physicists whose lives overlapped Hertz's brief 36-year span, and whose comments were made at various times during the decade following the first German printing of Hertz's *Principles of Mechanics* in 1894.

THE REACTIONS OF FOUR LEADING PHYSICISTS TO HERTZ'S *MECHANICS*

I. FitzGerald

Our first reviewer is the Irish physicist George Francis FitzGerald, whose life occupied the half century from 1851 to 1901. FitzGerald spent his entire career at Trinity College, Dublin, and acted as the leader of the Maxwellians, an informal group of physicists from the British Isles who, after Maxwell's death in 1879, vigorously defended and advanced Maxwell's electromagnetic theory.[2] Hertz met FitzGerald for the first and only time when he travelled from Bonn to London to receive the Rumford Medal in 1890. There they met for dinner with Oliver Lodge (1851–1940), another Maxwellian who was also a great admirer of Hertz's electromagnetic work.

FitzGerald's review of Hertz's *Mechanics* appeared in the journal *Nature* on January 17, 1895. Three-quarters of the review are devoted to Helmholtz's Preface and Hertz's own Preface and long Introduction to his book. Only the last quarter of the review covers the more-than-200 pages of the text itself, and most of this is confined to a discussion of what Hertz means by the path of a system of points. This was a crucial element in Hertz's fundamental law – that every free mechanical

D. Baird et al. (eds.), Heinrich Hertz: Classical Physicist, Modern Philosopher, 173–181.

system persists in its state of rest or of uniform motion along its straightest path, i.e., along the path of least curvature.[3]

FitzGerald's overall judgment of Hertz's book is contained in the last two sentences of his review: "It is most philosophical and condensed, and gives one of the most – if not the most – philosophical presentation of dynamics that has been published. It is worthy of its author; what more can be said?" (1895, 285).

But this favorable (if somewhat ambiguous) comment is tempered by pointed criticisms scattered throughout the review. There are three of particular significance:

Hertz claims that all of mechanics can be deduced from his fundamental law. But FitzGerald notes that this law is really only a postulate that must be confirmed by the experimental verification of its predictions in the universe around us.

Again, Hertz attempts to explain "force" (a concept he always had misgivings about) as the effect of kinematic constraints that link the observed motion of a physical system to hidden moving masses in that system. These constraints are imposed by rigid connections that link the real masses of the system to the hidden masses. But FitzGerald was of the opinion that Hertz had not faced the problem of these connections becoming tangled. He writes: "Analytically a postulate that the points of two different bodies that act on one another are in contact is easily expressed, but it does not follow that when we come to invent actual rigid connections to produce the observed effects, they will do so for any length of time without jamming" (1895, 284). For FitzGerald this tangling of the connections was a very serious difficulty.

Finally, FitzGerald does not share Hertz's difficulties with the concept of "force." He notes that the law of gravity can be perfectly well described without any reference to the notion of force, by merely stating it in the following kinematic form: every particle of matter moves toward every other particle in the universe with an acceleration inversely proportional to the square of their distance apart. The principal reason for introducing forces, according to FitzGerald, is to account for a particle acting where it *is not*; and this is tied to our lack of distinct ideas about where a particle *is*. It is possible without contradiction, FitzGerald asserts, to consider each particle as existing everywhere it acts, that is, throughout the whole of space. Then all action-at-a-distance difficulties would vanish without the need to postulate hidden mechanisms to explain the gravitational attraction of particles that appear to us to be spatially separated.

These and additional criticisms by FitzGerald indicate that he doubted the usefulness of Hertz's mechanics for solving practical problems in physics. This seems to have diminished considerably his appreciation of the many important insights – both physical and philosophical – that he found in Hertz's book.

II. Mach

Our second critic is Ernst Mach, the Austrian physicist and philosopher, who lived from 1838 to 1916. Hertz refers favorably to Mach's writings on mechanics both in his Author's Preface and his Introduction to the *Principles of Mechanics*. In his

Preface he admits his debt to Mach: "In a general way I owe very much to Mach's splendid book on the *Development of Mechanics*" (PM xl).

Hertz only met Mach once, at the 1891 *Versammlung* of the *Gesellschaft Deutscher Naturforscher und Ärzte* in Halle. Before this meeting Mach wrote to Hertz expressing his eagerness to meet the author of the 1884 paper comparing other theories of electromagnetism with Maxwell's theory (Misc 273–290), and of the two 1890 theoretical papers on the electromagnetic equations for bodies at rest and bodies in motion (EW 195–268). These latter two papers Mach described as approaching the ideal of a physics freed of all mythology (Thiele 1968, 132).

To the third German edition of his *Die Mechanik in ihrer Entwicklung historisch-kritisch dargestellt*, which appeared in 1897, Mach added an 8-page section entitled "Hertz's Mechanics." (1960, 317–324).[4] In this section, Mach points out that in his first edition he had outlined "an extremely general program for a future system of mechanics." He now suggests that "Hertz's book must, in fact, be read by everyone interested in mechanical problems," since, in Mach's opinion, "his novel views represent a great step forward." But then Mach launches into an array of more negative comments, including the following:

Hertz's reasons for removing "force" as a fundamental concept of physics are not justified, being based on logically defective expositions that Hertz remembered from his student days, not on the clear accounts of Huygens and Newton. Although Mach had his own difficulties with the idea of force, he found the concept, as normally used in physics, much more acceptable than Hertz's "hidden masses" and "hidden motions," which, in Mach's opinion, served only to reintroduce mythology into physics.

Although Hertz's fundamental law of mechanics is new in form, in Mach's opinion its content is identical with Lagrange's equations in mechanics. This is because the rigid connections that Hertz postulates between particles are really the same as what are conventionally called "forces."

Finally, according to Mach, in the beautiful ideal form that Hertz has given to mechanics, its physical contents have shrunk to an almost imperceptible residue. Descartes would have seen in Hertz's mechanics the realization of his own ideal – the reduction of all mechanics to what Martin Klein has called "a mechanics from which dynamics would be eliminated, and which would consist exclusively of kinematics" (Klein 1972, 74).[5]

On the basis of these criticisms, Mach concludes: "As an ideal program Hertz's mechanics is simpler and more beautiful, but for practical purposes our present system of mechanics is preferable, as Hertz himself, with his characteristic candor, admits" (Mach 1960, 324). Mach is here referring to Hertz's statement in the Introduction to his *Mechanics* about his third, force-free representation of mechanics: "The appropriateness [*Zweckmässigkeit*] of which we have spoken has no reference to practical applications or the needs of mankind. In respect of these latter it is scarcely possible that the usual representation of mechanics [i.e., the conventional one in terms of space, time, mass and force], which has been devised expressly for them, can ever be replaced by a more appropriate system" (PM 40). This idea Hertz repeated in a letter of 19 May 1893 to Professor Éduard Sarasin in Geneva, in which he writes that his book on mechanics " ... unfortunately has a purely theoretical interest and no practical interest at all" (quoted in Jungnickel and McCormmach 1986, 142). These statements of Hertz were often exploited by his critics, and led to a general impression among physicists that Hertz's book was beautiful, but useless.

III. Boltzmann

Our third physicist is Ludwig Boltzmann, who lived from 1844 to 1905 and who, like Mach, was an Austrian and a philosopher of considerable repute. He probably never met Hertz, since during most of Hertz's professional life Boltzmann was professor in Graz and Munich and therefore somewhat remote from the center of physics activity in Germany. Despite this, he had great admiration for Hertz, based on the quality and importance of Hertz's research. On 6 January 1894, just five days after Hertz's death, Boltzmann wrote to Helmholtz:

> One should emphasize the extraordinary import of Hertz's discoveries in relation to our whole concept of Nature, and the fact that beyond a doubt they have pointed out the only true direction that research can take for many years to come. (Koenigsberger 1902–03, 3:100)

But when Hertz's *Mechanics* finally was published at the end of that same "black year,"[6] which saw the death not only of Hertz but of Helmholtz and August Kundt as well, Boltzmann was less enthusiastic. His reactions are to be found in his address to the *Naturforscherversammlung* in Munich on 22 September 1899, and in lectures delivered at Clark University in Worcester, Massachusetts during that same year (1974, 88–91 and 108–113).

Boltzmann was very impressed by Hertz's Introduction, in which he had set forth a cogent epistemological foundation for physics. He also liked Hertz's new representation of mechanics, referring to it as "extraordinarily simple and beautiful," one that "has a certain inner perfection and obviousness, and contains very few arbitrary elements."

But when it came to the practical utility of Hertz's mechanics, Boltzmann was, again like Mach, more skeptical. He points out that he had been unable to find hidden masses that would lead to acceptable solutions when Hertz's methods were employed, even for very simple gravitational problems. Boltzmann concluded that even in the simplest cases Hertz's new approach leads to the greatest complications and, therefore, that: "as long as even in the simplest cases no systems or only unduly complicated systems of hidden masses can be found that would solve the problem in the sense of Hertz's theory, the latter is of purely academic interest" (Boltzmann 1974, 90).

This unwieldiness of Hertz's mechanics dissuaded most other physicists from attempting to apply it to real physical problems, and led to Boltzmann's well-known statement: "I have often heard Hertz's mechanics praised, yet never seen anyone pursue the path he indicated" (Boltzmann 1974, 88). This view had to be slightly modified in 1904 when Paul Ehrenfest (1880–1933), a student of Boltzmann, took up his mentor's challenge and did successfully apply Hertz's method to derive the equations of motion for an incompressible fluid, and also for a rigid body moving in an incompressible fluid – but these were problems that could have been solved more easily by conventional methods.[7]

Boltzmann was always a kind-hearted critic, however, and feared he was being unfair to Hertz; he therefore admitted the possibility that, if Hertz had lived, he might have been able to respond to some of Boltzmann's criticisms. "Unfortunately," he goes on, "at that precise moment his lips were forever sealed and unable to respond to the thousand requests for clarification that are certainly not on the tip of my tongue alone" (Boltzmann 1974, 89–90).

On balance, Boltzmann's overall opinion of Hertz's mechanics was negative. He stated it most clearly and emphatically in his 1899 lecture in Munich:

> I therefore think that Hertz's mechanics is more a program for the distant future. Should people one day succeed in explaining without artificiality all natural phenomena by means of hidden motions, then the old mechanics would be superseded by that of Hertz. Until then the former [i.e., the old mechanics] can represent all phenomena in a really clear manner without introducing things that are not only hidden but of which we have not the slightest idea how we are to conceive of them. (Boltzmann 1974, 90)

IV. Lorentz

Our fourth and last opinion on Hertz's Mechanics comes from the eminent Dutch physicist, Hendrik Antoon Lorentz, who lived from 1853 to 1928 and was professor of theoretical physics in Leyden for 35 years. Lorentz's daughter Geertruida, tells us that her father had assembled a group of portraits of physicists above his desk in Leyden. As a little girl she used to ask him which one of these physicists was the most clever, to see if he would always give the same answer. Lorentz always did; he consistently pointed to Augustin Fresnel (1788–1827) "from olden times," as he put it, and to his contemporary, Heinrich Hertz.[8] Many years later Lorentz's daughter was surprised to learn that her father had never met Hertz. Lorentz did not begin to make scientific contacts outside the Netherlands until 1898 when for the first time he attended the *Naturforscherversammlung* in Düsseldorf. Hertz had died four years earlier, and Lorentz always regretted that he had too long postponed the pleasure of meeting the physicist he admired so much.[9]

In 1902 Lorentz presented a paper before the Royal Academy in Amsterdam with the title: "Some Considerations on the Principles of Dynamics, in Connexion with Hertz's *Prinzipien der Mechanik*" (Lorentz 1937b, 36–58).[10] Lorentz was greatly impressed by some aspects of Hertz's book. He writes "... it seems hardly possible to doubt the great advantage in conciseness and clearness of expression that is gained by the mathematical form Hertz has chosen for his statements" (Lorentz 1937b, 36).

Although Lorentz had serious doubts about Hertz's introduction of hidden masses and hidden motions to replace conventional "forces," he did not direct his energies to criticizing this or any other feature of Hertz's book. Rather he set out to test whether the advantages of Hertz's method could be preserved even if, "leaving aside the hypothesis of hidden motions, ... one considers the motion of a system governed by 'forces' in the usual sense of the word" (Lorentz 1937b, 36). Lorentz therefore attempted to develop a system of mechanics parallel to that of Hertz, but one that used "force," in the customary sense, rather than Hertz's mysterious hidden masses and hidden motions.

It is obviously impossible to enter into the details of Lorentz's 22-page article here. He follows Hertz's development rather closely, and reduces the motion of all *n* particles in a physical system to the motion of a single point in a $3n$-dimensional space. He considers the variations of the path of this motion and arrives at an equation that reduces, for systems acted on by no forces, to Hertz's fundamental law of the straightest path. He also shows that the same equation can be used to derive Hamilton's Principle and Jacobi's Principle of Least Action. This was consistent with the results of Hertz, who had shown in Book II of his *Mechanics* that all the usual formulations of mechanics – Newton's laws, Lagrange's equations, Hamilton's Principle, and the other minimal principles of mechanics – can be deduced as theorems from his *Grundgesetz*.

At the end of his paper Lorentz does not return to any further discussion of Hertz's *Mechanics*. But his conclusion seems clear: there is no need to eliminate the concept of force and to introduce hidden masses and hidden motions into mechanics. A perfectly satisfactory system of mechanics can be developed, as Lorentz thought he had demonstrated, by using the conventional concept of "force." Thus there is no need for the complexities and ambiguities that Hertz's approach introduces into the science of mechanics.

SUMMARY AND CONCLUSIONS

In summary, then, these four esteemed physicists all admired the many good qualities of Hertz's *Mechanics*. It was simple, clear, concise, beautiful – and was marked by a logical development and inevitability that they found attractive. They were also in agreement on the philosophical perceptiveness and physical insight displayed by Hertz in his Introduction. But they shared two important criticisms of Hertz's book.

First, his elimination of the concept of force and its replacement by a complicated system of hidden masses were not helpful in placing mechanics on a firm foundation. Rather, they constituted a step backward by endeavoring to replace a useful, if controverted, concept – force – by a very complicated system of imaginary entities. This criticism is quite valid and undoubtedly carried great weight with physicists in the decade after 1894.

Second, Hertz's *Mechanics* is totally impractical and does not lead to any important new physical results. This criticism is also valid, but it is *quite unfair*. Hertz's objective in his *Mechanics* was to put mechanics on an absolutely firm logical foundation; it was *not* to propose a new technique for solving problems in mechanics. He was interested, as he said, only "in the logical and philosophical aspects of mechanics," in explaining mechanics in so perfect a form "that there should no longer be any possibility of doubting it" (PM 9). But it turned out that Hertz's desire to establish mechanics on a firm logical foundation had little appeal for most physicists, who were quite content with the mechanics they had, since they knew how to apply it, and it led to results in agreement with experiment.

SOME ADDITIONAL CONSIDERATIONS

To round out our discussion, a few additional factors are worth mentioning, since they helped create an intellectual climate unhealthy for Hertz's *Mechanics*, a climate that would have remained inclement no matter how hard Hertz had worked to clarify and refine his book.

It is important to realize that Hertz had never discussed his book with any other physicist, not even Helmholtz, as he wrote in a letter to his parents on 19 November 1893: "It frightens me to come out with something that I have never talked over with any human being" (MLD 342). As a result, not only did the book lack the corrections and improvements that would have come from frank discussions with colleagues, but after Hertz's death no physicist was able to state definitively what Hertz really meant by certain passages in his *Mechanics*. Even Helmholtz, who had read the proofs, admitted that he had trouble understanding what Hertz was trying to do (Koenigsberger 1902–03, 3:104f.).

Also, Hertz's own verification of Maxwell's electromagnetic theory by his experiments in Karlsruhe had gradually led physicists to prefer an electromagnetic explanation of the phenomena of nature to a mechanical one.[11] For example, many German physicists at the turn of the century, including Emil Cohn (1854–1944), Emil Wiechert (1861–1928), and Wilhelm Wien (1864–1928) sought an explanation of both matter and energy, and hence of all physics (including mechanics) in terms of electromagnetism.[12] This attitude was slowly eroding physicists' certitude that mechanics was the most fundamental branch of physics, although Hertz himself never doubted its preeminence (PM xxxvii).

Another factor worth considering is the preference of physicists in Germany in the middle 1890s for experiment as opposed to theory. After Boltzmann left Munich to return to Vienna in 1894, his former chair of theoretical physics in Munich remained vacant for over a decade because no worthy replacement could be found.[13] A few years later Wilhelm Wien sent a letter dated of 11 June 1898 to Arnold Sommerfeld (1868–1951) containing the statements: "Theoretical physics in Germany lies almost completely fallow. ... Theoretical physics currently finds no takers" (Jungnickel and McCormmach 1986, 2:159).

This anti-theoretical feeling had been buttressed by the unexpected discovery of x-rays by Wilhelm Roentgen (1845–1923) in November 1895, just one year after the first edition of Hertz's *Mechanics* appeared. Accounts of Roentgen's discovery and its immediately-recognized practical importance soon filled newspapers and popular magazines throughout the world; the Golden Age of modern physics and modern medicine had begun. Just a few years later, in 1898, Marie and Pierre Curie discovered radioactivity. This was followed, in 1900, by Max Planck's announcement of the need to quantize energy to explain experimental observations on black-body radiation. Soon most physicists in Germany were busy doing experiments on x-rays, cathode rays, radioactivity, black-body radiation and spectroscopy, for it was in these newer, mostly *experimental* fields that the thrill of great discoveries like Roentgen's was to be looked for and hoped for. This emphasis on experiment in physics culminated in 1901 in the award of the first Nobel Prize in Physics to Roentgen for his discovery of x-rays.

All these factors worked to produce a physics community less interested in mechanics, and especially in the subtleties of a highly-abstract, severely-theoretical treatment of mechanics like that of Hertz. This, and the lack of enthusiasm for Hertz's approach to mechanics displayed by well-disposed theoretical physicists of the caliber of FitzGerald, Mach, Boltzmann and Lorentz, are sufficient to explain why Hertz's *Mechanics* was received with diffidence by the physics community in the decade immediately following its publication, and why it disappeared almost completely from physics after the development of quantum mechanics and relativity in the first decades of the new century.

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NOTES

1 To see the extent of this neglect of Hertz's *Principles of Mechanics*, consider just two examples: L.A. Pars in his *A Treatise on Analytical Mechanics* (New York: John Wiley and Sons, 1965) lists Hertz's book in his bibliography, but there is not a single reference to it in the text itself; Hamel 1967, p. 367 says of Hertz's efforts to eliminate force from mechanics: "This, of course, is an absurdity, since a mechanics without a concept of force is no mechanics." Hamel does, however, devote a few pages (366–367) to Hertz's fundamental law ["*Das Prinzip von Hertz*"].

2 On FitzGerald and the Maxwellians see Hunt 1991 and O'Hara and Pricha 1987.

3 In Jones and Walley's translation of Hertz's *Principles of Mechanics* for its first edition in 1899, they consistently translate Hertz's *Grundgesetz* as "fundamental law." This is the translation we adopt here, since Hertz frequently emphasizes the relationship between his fundamental "law" and Newton's first "law" of motion. In Germany at the turn of the century, however, Hertz's "law" was often referred to as *das Hertzsche Prinzip*. (Compare the discussions of Hertz's fundamental law in the papers of Jesper Lützen and Simon Saunders in the present volume).

4 These pages contain the best critical summary of Hertz's *Mechanics* to be found anywhere.

5 See also Klein 1974, p. 168; Emil Wiechert (1861–1928) once suggested the name *Kinetische Mechanik* for Hertz's new kind of mechanics.

6 The term "black year" was first applied to 1894 by Max Planck in 1935. See his *Physikalische Abhandlungen und Vorträge* (Braunschweig: Vieweg, 1958, 3: 358–363), p. 362; and Joseph F. Mulligan, "Max Planck and the 'Black Year' of German Physics," *American Journal of Physics* **62** (1994), pp. 1089–1097.

7 On this see Klein 1970, chapter 4. Vilhelm Bjerknes (1862–1951), who had been a research student in Bonn when Hertz was writing his Mechanics, also applied Hertz's mechanics to some hydrodynamical problems related to meteorology. Throughout his life Bjerknes considered Hertz's book to be the best starting point for all work in modern physics; on this see Friedman 1989, chapter 1.

8 G.L. de Haas-Lorentz (ed.), *H.A. Lorentz: Impressions of His Life and Work* (Amsterdam: North-Holland 1957), pp. 41–42.

9 Lorentz gave voice to his admiration for Hertz in his Nobel Prize acceptance speech in Stockholm in 1902. He refers to Maxwell and Hertz as the true founders of our present views on electromagnetism, and goes on: "... Hertz, that great German physicist, who, if he had not been snatched from us too soon, would certainly have been among the very first of those whom your Academy [the Swedish Royal Academy of Sciences, which decides on the recipients of Nobel Prizes] would have considered in fulfilling your annual task." On this see *Nobel Lectures: Physics* (New York: Elsevier, 1967), vol. 1, p. 16.

[10] This article of Lorentz is discussed in greater detail by Jesper Lützen in his paper in the present volume.
[11] On this see Rosenfeld 1957, esp. pp. 1660–1667, and Klein 1974, pp. 155–158 and 167–172. Albert Einstein once said that Maxwell and Hertz had demolished, without intending to, "the faith in mechanics as the final basis of all physical thinking"; in *Autobiographical Notes*, translated and edited by Paul A. Schilpp (La Salle: Open Court, 1979, p. 19).
[12] See, for example, the following papers: Emil Cohn, "Zur Systematik der Elektricitätslehre," *Annalen der Physik* **40** (1890), pp. 625–639; Emil Wiechert, "Über die Grundlagen der Elektrodynamik," *Annalen der Physik* **59** (1896), pp. 283–323; and Wien 1901.
[13] On this see Jungnickel and McCormmach 1986, pp. 157–159. It is worth noting that advanced students in Germany were almost forced to do research in experimental physics if they wanted eventually to become *ordinarii* [full professors], since there were only four chairs of theoretical physics in Germany at the end of the nineteenth century: Berlin (Max Planck), Göttingen (Voldemar Voigt), Königsberg (Paul Volkmann), and Munich (vacant from 1894 until 1905, when Arnold Sommerfeld was appointed).

KLAUS HENTSCHEL

HEINRICH HERTZ'S *MECHANICS*: A MODEL FOR WERNER HEISENBERG'S APRIL 1925 PAPER ON THE ANOMALOUS ZEEMAN EFFECT

1. INTRODUCTION

In this paper, I will show that Heisenberg's last paper before his invention of matrix mechanics in the summer of 1925[1] contains striking parallels to Hertz's *Mechanics*. Although other philosophical influences on the young Heisenberg, as well as his physics and mathematics background, have already been examined extensively,[2] this particular Hertzian strand in Heisenberg's writings at that specific time has not been pointed out in the pertinent secondary literature.[3]

I not only claim that Heisenberg was "influenced" by Hertz in this period, but that he in fact deliberately organized and formulated his paper with Hertz's mechanics in mind. There are not only terminological similarities (see section 2), but Hertz's influence is also visible in the very structure of the paper, in which three schemes[4] and their respective "quasi-mechanical models" (*quasimechanische Modelle*) are discussed one after another. His use of "schemes," at one point also called "symbolic model-like images" (*symbolische modellmäßige Bilder*), is analogous to the three *Bilder* of mechanics analyzed in the Introduction of Hertz's *Mechanics* (see section 3). Both authors introduce these alternative schemes as internally consistent and empirically adequate, therefore as "permissible" – that is, as acceptable in principle – and evaluate them further on the basis of supplementary criteria such as simplicity and appropriateness (*Einfachheit und Zweckmässigkeit*),[5] although both authors paradoxically end up developing extremely elaborate formalisms which only function as transitional stages in the development of their respective fields. Nevertheless, these close parallels between Hertz's and Heisenberg's texts might help to understand both of them better. Furthermore, this documentation of the hitherto underestimated importance of Hertz's philosophy of science to the young Heisenberg confirms the conjecture of Hertz's teacher Hermann von Helmholtz in his preface to Hertz's *Mechanics* that it would be of future heuristic importance.[6] The paper concludes by investigating why Heisenberg might have been attracted to Hertz's way of looking at the foundations of mechanics in early 1925, at a time of serious crisis for the Bohr-Sommerfeld quantum theory, shortly before the invention of the new quantum mechanics and shortly before the emergence of the concept of spin, which solved all the riddles Heisenberg was then facing. We thereby come to a deeper understanding of a constellation of theories in science that fosters this attitude towards models.

D. Baird et al. (eds.), Heinrich Hertz: Classical Physicist, Modern Philosopher, 183–223.

2. TERMINOLOGICAL SIMILARITIES

To begin with a comparative analysis of Hertz's and Heisenberg's texts, I must start with the most obvious feature: Heisenberg's recurrent and emphasized reference to "symbolic, model-like pictures" in statements like the following:

In order to utilize this hypothesis in the present state of quantum theory, one must rely on the use of symbolic, model-like *Bilder* which are formed more or less after the behavior of electrons in classical theory.[7]

This is clearly an allusion to Hertz's famous and by far most quoted statement in the Introduction to his *Mechanics*:

We form for ourselves images or symbols of external objects [*Wir machen uns innere Scheinbilder oder Symbole der äußeren Gegenstände*] (PM 1)

The term *Bilder* means "symbols" rather than "icons" (in Peirce's and Morris's classification of signs); thus the usual translation of the term as "mental representation" is not too misleading, although it does not capture the connotation of visualizability present in the German term. To illustrate further what he means by it, Hertz continues:

and the form we give them is such that the necessary consequents of the images in thought are always the images of the necessary consequents in nature of the things pictured. [...] The images which we here speak of are our conceptions of things. With the things [*Dinge*] they are in conformity in *one* important respect, namely in satisfying the above-mentioned requirement. For our purposes it is not necessary that they should be in conformity with the things in any other respect. (PM 1f.)[8]

As has been discussed extensively in the secondary literature on Hertz's conception of *Bild*, this passage in his mechanics marks an important point in discussions about the semantics of physics: All terms used by physicists in their statements about objects are understood here as *signs representing* one or more selected qualities of the object without necessarily bearing any further similarity or affinity to the represented object. This implies the abandonment of the traditional conception of physical theory as a unique, "true" account of reality. To simulate physical processes (such as, for instance, causal chains of momentum transfers), our mind operates with mental representations of them. The condition for their usefulness is that the outcome of this mental operation in turn represents the outcome of these physical causal chains of actions. Thus, mental representations have empirical correlates in the outside world without full equivalency between both.[9] The impact of Hertz's *Bild*-conception was to strengthen the conventionalist attitude towards theories in physics, at that time also advocated by Henri Le Roy and Henri Poincaré in France as well as by his Berlin colleague Gustav Robert Kirchhoff, who had demanded in his lectures on mechanics that, as the science of motion, mechanics should account for natural processes in the most complete and simplest description possible.[10]

After elaborating on his concept of *Bilder* in the Introduction to *The Principles of Mechanics*,[11] Hertz exemplified it with a lengthy discussion of three *Bilder* of mechanics which I will discuss briefly in the next section.

3. HERTZ'S *BILDER* OF MECHANICS

3. 1. The Newtonian and the energeticist Bilder *of mechanics*

The first *Bild* was the traditional (Newtonian) representation of mechanics based on the concepts of space, time, force and mass (cf., e.g., the introductory definitions and scholia of Newton's *Principia* 1687) together with later elaborations by Euler, d'Alembert, Lagrange and others. Force is introduced into this model as independent of motion and prior to movement, as the cause of acceleration. Without doubting the pragmatic virtues of the traditional *Bild* of mechanics, Hertz nevertheless expresses his feeling that it is not really logically satisfactory [*befriedigend*]. Aside from the problematic definition of "mass" in Newton's *Principia* which Hertz also traced in later textbooks such as, for instance, in William Thomson's and Peter Guthrie Tait's *Treatise on Natural Philosophy*,[12] Hertz's critique of this first *Bild* of mechanics focused on the definition of force. He singled out especially the equality of force and counter-force, since the validity of Newton's third law of motion is often only granted by postulating fictitious [*uneigentliche*] forces, such as centrifugal force which supposedly precisely compensates for the attracting gravitational or pulling force in rotating systems.

Now is this mode of expression permissible? Is what we call centrifugal force anything else than the inertia of the stone? Can we, without destroying the clearness of our conceptions, take the effect of inertia twice into account – firstly as mass, secondly as force? (PM 5f.)[13]

As will be illustrated further in section 7, Hertz's attempt to achieve conceptual parsimony led him to question whether the concept of "force" might not be dispensable in the foundations of mechanics, so that the remaining network of basic notions and their interconnections might be less burdened with superfluous auxiliary cogs [*leer mitlaufende Hilfsräder*].

But we have accumulated around the terms "force" and "electricity" more relations than can be completely reconciled amongst themselves. We have an obscure feeling of this and want to have things cleared up. Our confused wish finds expression in the confused question as to the nature of force and electricity. But the answer which we want is not really an answer to this question. It is not by finding out more and fresh relations and connections that it can be answered; but by removing the contradictions existing between those already known, and thus perhaps by reducing their number. (PM 7f.)

Thus it was not through any doubt in the logical permissibility of the Newtonian version of mechanics, but through the "tortured mind's" [*gequälte Geist*] desire to overcome the conceptually unsatisfactory multiplication of basic notions, that Hertz was led to consider seriously the following two schemes of mechanics.

Actually, Hertz himself temporarily pursued the second scheme of his *Mechanics*; it was in fact a fairly fashionable approach to mechanics in the 1880's, stirring up a lot of controversies which only served to increase the popularity of "energetics," as it was called.[14] According to Wilhelm Ostwald and Georg Helm, two of its main representatives, the concept of energy might function as the key notion of mechanics, replacing the concept of "force" in the description of mechanical processes. As the formalisms of Laplace and Hamilton already showed, a close analysis of the dependency of kinetic and potential energy of a mechanical system on the spatial and temporal coordinates already implied a great deal about the motions of the system. For instance, Laplace's equation, based solely on the fact that mechanical systems tend to minimize the difference between kinetic and potential energy, averaged over time, could already fully describe and predict the kinematics of systems such as a pendulum. Force is only introduced into this scheme as a useful auxiliary term, based on more or less arbitrary definitions, and not as a basic notion as in scheme I, and thus it cannot present any logical difficulties in this scheme.[15]

Compared with scheme I, according to Hertz the second scheme yields a clearer mental representation [*deutlicheres Bild*] by omitting statements about entities of which we know nothing, while accurately representing the relations between observable magnitudes.

> Herein lies the advantage of the conception of energy and of our second image of mechanics: that in the hypotheses of the problems there only enter characteristics which are directly accessible to experience, parameters, or arbitrary coordinates of the bodies under consideration; that the examination proceeds with the aid of these characteristics in a finite and complete form; and that the final result can again be directly translated into tangible experience. (PM 18)

Indeed, the thrust of the energetics movement had come from the focus on observables combined with a deep skepticism concerning all unobservable entities such as atoms and molecules; the latter were therefore rejected by representatives of energetics such as Ostwald and Duhem, who strove instead to describe physical processes in terms of continuum mechanics.[16] Though this description in terms of the least action principle and generalized thermodynamic potentials had some virtues (especially in the field of physical chemistry in which both Ostwald and Duhem worked), there were serious shortcomings, too. Among other things, Hertz specifically pointed to the problem of detecting how much energy is to be attributed to a space-time region without the prior introduction of notions such as force and potential. Although the conservation of energy and mass is presupposed by scheme II, in which all physical processes are effectively simply transformations of one type of energy into another, there seems to be no independent means of determining the absolute magnitude of say, the potential energy, since it can always be gauged differently (for instance, by adding a constant) without any change in the observable quantities of position, speed and acceleration, which only depend on its derivatives. Furthermore, the potential energy of gravitating systems cannot be defined strictly locally since it depends on the presence and magnitude of remote masses, even though they may not have had any physical impact whatsoever on the system under consideration. Therefore, Hertz did not believe that scheme II could ultimately avoid the obstacles and sticking points present in the first scheme of mechanics.[17]

3.2. Hertz's third Bild *of mechanics*

The third *Bild* was Hertz's own brainchild, and it was this *Bild*, solely grounded on the basic notions of time, space and mass, which he developed in much more detail in the main bulk of his *Mechanics*.[18] This scheme was analogous to Kirchhoff's scheme of mechanics in that it tried to introduce only three independent fundamental notions and not four as in schemes I and II; but it deviated from Kirchhoff's by dispensing with "force" while Kirchhoff's dispensed with "mass."

> The difficulties have hitherto been met with in connection with a fourth idea, such as the idea of force or of energy; this, as an independent fundamental conception, is avoided here. (PM 25)[19]

Hertz generalized the insight gained from impact collisions as the most paradigmatic physical process of the oldest forms of mechanics. Hertz tried to describe all mechanical processes without resorting to action-at-a-distance forces or elastic forces. Instead he imagined all motions of visible matter to be immersed in an incompressible fluid. Since not all visible bodies are directly adjacent to each other, this implied the existence of a great number of hidden masses [*verborgene Massen*] which would guarantee that mechanical action could be transmitted according to the laws of mechanics through quasi-rigid connections [*starre Verbindungen*] between the visible masses. To avoid violation of the conservation laws, the hidden masses in Hertz's scheme were cyclic, which means that each moving hidden mass point had to be replaced by an identical mass point. Potential energy of a mechanical system was thus nothing but kinetic energy of the hidden masses within Hertz's scheme.[20] Although it demanded the daring introduction of an indefinite number of hidden masses, it promised to dispense with the usual notion of force, since all apparent action-at-a-distance phenomena usually described with distance-dependent force laws could (in principle) be reduced to contiguous action between material bodies in rigid connections, as represented by (hydrodynamical) differential equations with constraints. I say "in principle" because Hertz unfortunately did not further exemplify this hypothetical interaction between visible and hidden masses for actual mechanical problems.[21] One simple example discussed by Mach consists in the rotation of one visible body with mass m around a central point with constant velocity v at distance r. While the conventional scheme describes this motion as due to a central force F compensating the centripetal force mv^2/r, Hertz's scheme accounts for this motion by postulating another hidden mass m at distance r of a rigidly linked central point opposite to the visible mass. It is obvious that for less elementary trajectories, many more hidden masses would be needed in Hertz's scheme.[22] Therefore, Mach commented:

> If we are not content to leave the assumption of occult masses and motions in its general form, but should endeavor to investigate them singly and in detail, we should be obliged, at least in the present state of our physical knowledge, to resort, even in the simplest cases, to fantastic and even questionable fictions, to which the *given* accelerations would be far preferable. [...] As an ideal program Hertz's mechanics is simpler and more beautiful, but for practical purposes our present system of mechanics is preferable, as Hertz himself, with his characteristic candor, admits. (Mach 1960, 323f.; cf. PM 39f.)

Nevertheless, Hertz preferred introducing these unobserved masses to introducing unobserved forces on ontological grounds, since the unobserved entities postulated by his scheme of mechanics were at least *not different in principle* from the other (observable) entities occuring in it otherwise.

If we wish to obtain an image of the universe which shall be well-rounded, complete, and comformable to law, we have to presuppose, behind the things which we see, other invisible things – to imagine confederates concealed beyond the limits of our senses. [...] We may admit that there is a hidden something [*unsichtbare Dinge*] at work, and yet deny that *this something* belongs to a special category. We are free to assume that *this* hidden *something* is nought else than motion and mass again, – motion and mass which differ from the visible ones not in themselves, but in relation to us and to our usual means of perception. Now this mode of conception is just our hypothesis. We assume that it is possible to conjoin with the visible masses of the universe other masses obeying the same laws, and of such a kind that the whole thereby becomes intelligible and conformable to law. (PM 25f.)[23]

Thus, although Hertz clearly departed from the recipes of phenomenalist physics by introducing these invented [*hinzugedichtete*] masses, he maintained another ingredient of positivist philosophy of science by following Occam's rule of not multiplying ontological types unnecessarily in his model [*essentia non sunt multiplicanda*]. For Hertz, only by truly basing mechanics on mechanically conceivable contiguous actions could prospects for a true unity of science based on a mechanical world view be opened up. Ultimately, Hertz hoped that his mechanics would also indicate a path to a deeper understanding of contiguous action in electrodynamics.[24]

Let us briefly look at the technical details of Hertz's third scheme of mechanics. Because he replaces forces with the action of hidden masses, he can simplify the usual expression for the constraint [*Zwang*] $Z = \sum_{i=1}^{3n} m_i \left(\frac{d^2 x_i}{dt^2} - \frac{X_i}{m_i} \right)^2$ by setting all forces X_k equal to zero. By further regarding all masses m_k as multiples of an atomic unit mass, the expression for Z reduces to $Z = \sum_{k=1}^{N} \frac{d^2 x_k}{dt^2}$, with N giving the (unknown) number of atomic masses involved. Because of the conservation of energy $\frac{1}{2} \sum \left(\frac{dx_k}{dt} \right)^2 = W \Rightarrow \left(\frac{ds}{dt} \right)^2$ = constant, which allows Hertz to divide Z by W^2 with $Z/W^2 = :K = \sum_{k=1}^{N} \left(\frac{d^2 x_k}{ds^2} \right)^2$, where ds is the line element, and $\sqrt{K}$ is related to the curvature of the system's path.[25]

So, the only fundamental law of Hertz's version of mechanics is the statement that all natural motions of an independent material system are described by the system pursuing one of its shortest possible paths with unaltered velocity.[26] Each deviation from a straight and uniform motion is ascribed to the presence of other (usually hidden) masses whose rigid connections to the observable mass enforce these changes of direction. So the real motion of the mass approaches its free motion as much as possible.[27] This statement fulfills the criterion of logical economy, since it combines Newton's first law of motion with Gauss's principle of smallest constraint [*kleinster Zwang*] within one basic law.[28] Hertz derives all other laws of mechanics from this one fundamental law (if necessary with the further postulation of hidden masses to account for force-like interactions). In particular, concepts like forces and energy are introduced into mechanics merely for convenience as auxiliary concepts.[29] In fact, it is precisely because Hertz sees an economical axiomatic structure of the theory which minimizes the number of independent assumptions needed as the ultimate aim of the theory, that he even accepts the high price of his scheme's loss of pragmatic usefulness. The strong similarities between this approach and Einstein's *Prinzipientheorie* of relativity and gravitation of 1915/16 have been pointed out in the literature several times, so I can perhaps afford to ignore this strand of Hertz's reception in my paper.

Another difference between Hertz's scheme III and the mechanics described in the other two schemes is that in Hertz's mechanics one does not start from the description of forces between two mass-points idealized as independent of the rest of the world; rather the whole system of observable masses under consideration is taken into account from the very beginning. Thus, the mechanics of realistic material systems is not an extension of the abstract mechanics of idealized mass-points, as is usually the case, but rather the latter is a limiting case of the former – according to Hertz, a gain of plausibility for his scheme:

> For, in reality, the material particle is simply an abstraction, whereas the material system is presented directly to us. All actual experience is obtained directly from systems; and it is only by processes of reasoning that we deduce conclusions as to possible experiences with single points. (PM 31)[30]

In spite of Hertz's fascination with this third variant of mechanics, which is discussed further and developed at length in the main bulk of his textbook, he did not forget the lessons drawn from his philosophical analysis of the consequences of the *Bild* conception analyzed above. While emphasizing the internal consistency of scheme III, he did not claim his scheme to be the *only* legitimate, nor the only empirically adequate one; what counted most was the consistency with which this scheme promised to account for *all* interactions in mechanical terms.

> This merit of the representation I consider to be of the greatest importance, indeed of unique importance. Whether the image is more appropriate than another; whether it only embraces all present experience, all this I regard almost as nothing compared with the question whether it is in itself conclusive, pure and free from contradiction. [...] What I have sought is not the only image of mechanics, nor yet the best image; I have only sought to find an intelligible image and to show by an example that this is possible and what it must look like. (PM 33, cf. also 40f.)

We will encounter the same modest praise of the merely formal virtues of one scheme, combined with a surprising regard for the empirical adequacy and pragmatic usefulness of alternative schemes, in Heisenberg's discussion of the three models for the description of the splitting patterns of spectral lines in magnetic fields. To this we now turn.

Table 1: Hertz's three *Bilder* of Mechanics, summarized from PM 4ff., 14ff., 24ff.

Bild	I	II	III
Basic notions apart from space, time, and mass	Force (*Kraft*)	Energy	None
Basic principle	Newton's laws of motion or Lagrange's generalization of d'Alembert's principle	Hamilton's integral principle of least action	Minimal curvature of the system's path in a high-dimensional representation space
Virtues	Empirical adequacy, practicality	phenomenological correctness	logical economy
Problems	Pseudoforces, mechanism of force transmission	Insufficient number of parameters for $3n$ variables	Hidden masses needed
Important representatives	Newton, Euler, Lagrange etc.	Ostwald, Helm, Duhem (briefly Hertz himself)	Hertz

4. HEISENBERG'S THREE SCHEMES TO DESCRIBE THE ANOMALOUS ZEEMAN EFFECT

I will give a very brief summary of the Zeeman effect's main features, as well as of those of its contemporary interpretations relevant for understanding Heisenberg's paper. Then I will discuss the three schemes given by Heisenberg in his April 1925 paper, and conclude this section by briefly discussing the modern interpretation of the Zeeman effect. This should help clarify the ambiguities Heisenberg had struggled with shortly before the discovery of spin and quantum mechanics.

4.1. The Zeeman effect and its early interpretation

The Zeeman effect concerns a splitting of spectral lines into several components when the light-emitting region is subjected to strong magnetic fields. The so-called normal Zeeman effect which describes the splitting of the lines into several equidistant components was discovered by Pieter Zeeman in late 1896 and explained only a few months later by Hendrik Antoon Lorentz on the basis of the Larmor precession of negatively charged electrons in magnetic fields.[31] Two years later, in late 1897 and 1898, more complicated splitting patterns were discovered by Alfred Cornu and Thomas Preston, soon to be called the "anomalous Zeeman effect," since they could not be accounted for by classical electron theory; and in 1921 Friedrich Paschen and Ernst Back in Tübingen showed that in very powerful magnetic fields the splitting patterns of the anomalous Zeeman effect were reconfigured into those of the normal Zeeman effect (Paschen-Back effect).[32] After the development of the Bohr-Sommerfeld quantum theory in 1913–15, which introduced several quantum numbers for describing the states of atomic systems, Pieter Debye and Arnold Sommerfeld could easily retrace the explanation of the normal Zeeman effect within the new framework. More specifically, the negatively charged electrons were assigned orbits around the positively charged nucleus which were characterized by the following set of quantum numbers: the main quantum number n specifying the total energy according to E_n = constant $/n^2$, the azimuth quantum number k originally conceived as specifying the angular momentum of the electron and the eccentricity of the orbit, which increases with decreasing k,[33] the inner quantum number j specifying the total angular momentum, which according to this semiclassical theory, in turn determines the multiplet structure of the terms,[34] and finally, the magnetic quantum number m (with ($|m| < j$), specifying the angle between the total angular momentum and the direction of the magnetic field around which the whole system performed a Larmor precession. The splitting of spectral lines in the magnetic field H was then accounted for by a change in the total energy of the orbit according to ΔE = constant $\cdot\ m \cdot H$, with m varying between $+j$ and $-j$. Thus, this "space quantization" required the angular momentum to orient itself within a restricted number of positions relative to the magnetic field.

While the framework of the Bohr-Sommerfeld quantum theory worked perfectly well in explaining the normal Zeeman splitting, it was not so easy to adapt this framework to the much more complicated anomalous Zeeman effect. Sommerfeld and his pupils in Munich, Bohr and his collaborators in Copenhagen, and Alfred Landé in Frankfurt (later in Tübingen), all worked hard to come to an understanding of the anomalous Zeeman effect.[35] Already in 1920, Heisenberg's teacher at the seminar for theoretical physics at the University of Munich, Arnold Sommerfeld, characterized the early attempts at describing the anomalous Zeeman effect within the framework of the Bohr-Sommerfeld quantum theory as a "mystery of numbers" [*Zahlenmysterium*],[36] asserting to his readers that "the electrodynamic mechanism of the anomalous Zeeman effects is still hidden from us."[37] The splitting of spectral lines $\Delta\nu$ in the magnetic field H was described by a phenomenological law first introduced by Carl Runge in 1907:

$$\Delta\nu = \frac{q}{r} aH, \tag{1}$$

with $a = e/4\pi m_e c$ and q and r integral numbers (the so-called Runge nominator and denominator, respectively) depending on the respective magnetic splitting. Although Sommerfeld had succeeded in expressing q/r in terms of differences of two such expressions, q_1/r_1 and q_2/r_2 associated with the initial and final states between which the transition took place in his magneto-optical decomposition rule, he could not explain satisfactorily *why* he had to associate specific values to the Runge denominators differing for singlet and triplet states and differing again for the various angular quantum number assignments. In short, in contrast to the *normal* Zeeman effect for which one had a satisfactory physical explanation, the *anomalous* Zeeman effect could somehow be described by phenomenological rules, but the physics behind these rules of this "spectral zoology" [*Termzoologie*] and "Zeeman botany" [*Zeemanbotanik*] was not understood at all.

The situation worsened when in late 1920 Heisenberg, who was assigned the task of checking if and how Ritz's combination law and Runge's law could be retraced in the experimental values for the anomalous Zeeman effect, pointed out to

Sommerfeld that half-integral quantum numbers were needed to account for the magnetic splitting of doublet spectra within the scheme developed by Sommerfeld and Runge. This proposal by the complete novice Heisenberg severely shocked Sommerfeld and his other pupils such as Wolfgang Pauli:

> So after a very short time, I would say perhaps one or two weeks, I came back to Sommerfeld, and I had a complete level scheme. Then I came up with a statement which I almost didn't dare to say, and he was, of course, completely shocked. I said: "Well, the whole thing works only if one uses half quantum numbers." Because at that time nobody ever spoke about half quantum numbers; the quantum number was an entire number, you know, an integral. "Well," he said, "that must be wrong. That is absolutely impossible; the only thing we know about the quantum theory is that we have integral numbers, and not half numbers; that's impossible." [...] For quite a long time nobody knew whether that was decent physics or not.[38]

Nevertheless, Sommerfeld could not come up with any plausible proposal to circumvent half-integral quantum numbers,[39] which furthermore also soon proved useful for the description of molecular band spectra. When Alfred Landé, Adolf Kratzer, and others in 1921 also introduced these non-integral quantum numbers, the resistance to this ad hoc move to "save appearances" within the approach of the Bohr-Sommerfeld quantum theory crumbled.[40] In 1921, Sommerfeld and Heisenberg then tried to derive Landé's phenomenological formulas from a quantum theoretical reformulation of Voigt's classical theory of the anomalous Zeeman effect of 1913. While Sommerfeld simply tried to find adequate translations into quantum theory of Voigt's classical differential equations for coupled quasi-elastically bound electrons, Heisenberg went much further with his adaptation of the so-called atomic "trunk" or "core" model in November 1921.[41]

4.2. Heisenberg's atomic core (or Rumpf) model

According to this model, a doublet atom (and in particular an alkali atom) consisted in a strongly coupled group of tightly bound electrons close to the nucleus, together called "atomic core" or "trunk" [*Atomrumpf*], and one outer electron, the so-called "series electron" or "valence electron" [*Serienelektron*] or [*Valenzelektron*], sometimes also called *Leuchtelektron*, both of which have an average angular momentum of 1/2 in the ground state. In excited states, the valence electron was supposed to carry an angular momentum of $n - 1/2$.[42] Even without an external magnetic field, there existed a magnetic field H_i produced by the valence electron rotating around the core, which could be calculated classically using the Biot-Savart formula for magnetic fields caused by Ampèrian circular currents. Therefore, depending on the angle θ between the axis of rotation of the valence electron and the magnetic moment of the core, the magnetic interaction energy became:

$$\Delta E = \frac{h}{4\pi}\frac{e}{2mc}\cdot H_i \cdot \cos\theta\,. \qquad (2)$$

To reach agreement with observations, *cos* θ could only have two values for doublet atoms, namely ± 1, indicating a parallel or antiparallel position of the two angular momentum vectors. Once an outer magnetic field was present, the directions of the angular momenta of both atomic trunk and valence electron changed in order to minimize the total energy of the total atomic system. The resulting formula for the splitting patterns in external magnetic fields H could successfully account for the anomalous Zeeman patterns; and formula 2 reproduced equally well the doublet width of the lithium 2p state and the doublet width's dependency on the main quantum number n.[43]

Finally, Heisenberg could even extend his model to account for triplet atoms by postulating two valence electrons aside from the atomic trunk. For the minimum energy ground states, the atomic trunk was now assigned a vanishing average angular momentum, and the two valence electrons were assigned antiparallel angular momenta so that the total angular momentum of the atomic system also vanished. This possibility explained why all known triplet systems always also included singlet lines. The second possibility was that the atomic trunk had an average angular momentum of +1 while the two valence electrons each had +1/2 thus leading to a combined total angular momentum of +2, again consistent with the observed splitting patterns. But the agreement between theory and experiment reached in Heisenberg's paper which he submitted to the *Zeitschrift für Physik* on December 17th, 1921, did not last long.

In 1923, when, due to Miguel Catalan's experimental researches in the early 1920's, more complex "multiplets" such as quartet, quintet and octet spectra were proven to exist in the spectra of manganese and chromium, Alfred Landé introduced his so-called g-formula. This could account for the complex splitting patterns in terms of three "apparent" angular momenta r, k, and j, r referring to the atomic core, k referring to the electron shell, and j referring to the combined angular momentum. Landé dubbed them "apparent" because their numerical values were related to the vectors via the strange rule $j = \sqrt{(J + 1/2)(J - 1/2)}$ (and similarly for k and r, with J between $L + R - 1/2$ and $|L - R| + 1/2$, thus giving a multiplicity of $2R$, or $2L$ if $L < R$).

$$\Delta E = \text{constant} \cdot \mathrm{g} \cdot H, \text{with}\, g = 1 + \frac{j^2 + r^2 - k^2}{2j^2} = 3/2 + \frac{r^2 - k^2}{2j^2}. \tag{3}$$

Although Landé had implemented Heisenberg's idea of distinguishing between an atomic trunk and an outer shell of valence electrons, the specifics of Landé's formula nevertheless lacked an adequate physical interpretation.[44] Carl Runge once called the odd rules of combination of all these quantum numbers "witches' times tables" [*Hexeneinmaleins*], to others it appeared to be just "atomystical" [*Atomystik*]. Wolfgang Pauli then developed a physical interpretation in terms of a model in which the magnetic quantum number m of the atom was set equal to $m_r + m_k$, but the magnetic energy was proportional to $m_k + 2m_r$, with m_r and m_k as components of the angular momenta r and k of the atomic core and shell respectively.[45]

At the beginning of 1925, the joint efforts of quantum theorists in interpreting multiplet spectra and the anomalous Zeeman splitting then led to the insight that the

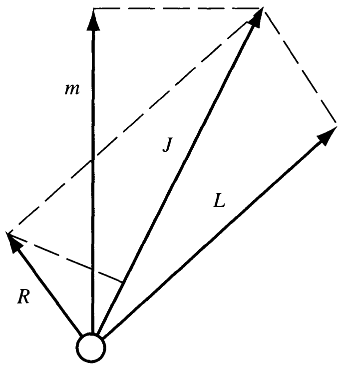

Figure 1. Landé's vector model: The combination of the orbital angular momentum vector L and the atomic core momentum vector $\vec{R}$ to the total momentum vector J and the latter's orientation around the external magnetic field characterized by m. $\vec{R}$, $\vec{K}$ and J have to be imagined precessing around the axis m. Redrawn from Hund, *op.cit.* (note 3), p. 118.

coupling of electrons in atomic shells involved a "mechanically unaccountable constraint" [*mechanisch unbeschreibbarer Zwang*],[46] which prevented a unique description of atomic states with the aid of any set of three quantum numbers such as j, r, k or n, k, j by imposing a characteristic ambiguity in the composition of their angular momenta. Also, new trouble arose for Heisenberg's atomic core model, because Pauli showed that the atomic core model had internal contradictions having to do with the relativistic mass variancy.[47] This made it probable that all closed shells would not contribute to the magnetic momentum of the atom and that the magnetic splitting should be understood purely in terms of the outer *Leuchtelektron*. Therefore, in January 1925 Wolfgang Pauli introduced a set of *four* quantum numbers n, k, m_k, m_r per electron to account for this ambiguity in the description of spectra, and discovered the exclusion principle according to which no two electrons in one atom are allowed to have identical values for all four quantum numbers.[48] It is this mechanically unaccountable "two-valuedness" [*Zweideutigkeit*] of the electron which Heisenberg set out to come to grips with in his April 1925 paper "Zur Quantentheorie der Multiplettstruktur und der anomalen Zeemaneffekte":

> The goal of the present work is a closer analysis of that characteristic two-valuedness. On the basis of this analysis we will try to arrive at a union [*Vereinigung*] of the different and seemingly contradictory formalisms, which have recently been proposed and which to some extent achieved great success in the description of the empirical facts [*bei der Beschreibung der empirischen Tatsachen teilweise große Erfolge erzielt haben*].

4.3. Heisenberg's paper of April 1925

Heisenberg made the basic assumption that the strange two-valuedness of the atomic state arises from the interaction of the atomic core [*Atomrest*] and the outer electron in such a manner that for fully specified states of core and electron there exist two different stationary states of the combined system and thus two resulting energy levels. In order to make sense of this initial hypothesis, Heisenberg had to assume one of the following two "symbolic model-like representations," associated with the two most pertinent models of the Zeeman effect discussed so far, namely with Landé's 1921 "incline theory" [*Neigungstheorie*], also elaborated further by Heisenberg himself (see p. 192 above), and with Pauli's "relativistic" theory involving a fourth quantum number.[49]

I. The electrons act on the atomic core through a non-mechanical constraint, such that the stationary state of the atomic core seems to be duplicated. II. The atomic core acts on the electron through a non-mechanical constraint, such that the stationary state of the electron seems to be duplicated.[50]

Not only Heisenberg's use of the term *symbolische modellmäßige Bilder*, but also the whole structure of his paper, where these two *Bilder* and later also a third are discussed one after another in a manner very similar to Hertz's Introduction, made it very clear that he in fact was alluding to Hertz's *Mechanics*, and not just to someone like Boltzmann who had also used the term after Hertz, the more so since the term *Bild* (in contrast to the term "model') was *not* in common use otherwise.[51]

What about Heisenberg himself: Did he know about Hertz's idiosyncratic account of mechanics? Although I did not find any direct quote from Hertz's *Mechanics* in Heisenberg's writings or letters, and although Hertz's textbook was in all probability not present in the Copenhagen institute of theoretical physics in 1925,[52] it is nevertheless safe to assume that he knew it from earlier on, simply because he had attended Sommerfeld's lectures on mechanics as part I of his course on theoretical physics at the University of Munich in his first term in the winter of 1920/21,[53] and it is in these lectures on mechanics that Sommerfeld (who rarely refers to any other textbook otherwise) discusses Hertz's mechanics and explicitly refers to it several times, discussing in detail Hertz's principle of the straightest path.[54] From all we know about Sommerfeld's lecturing style and the intensive training his students acquired in accompanying seminars and exercises, these lectures on mechanics by Sommerfeld made an especially deep impression on the young Heisenberg.[55] Unfortunately, upon being asked which textbooks on mechanics he had read during this period in the Kuhn interview on February 7, 1963, Heisenberg mentioned specifically only the mechanics textbook by Conrad Heinrich Müller and Georg Prange. This does contain explicit references to Hertz's mechanics,[56] but only appeared in 1923 in its first edition, so that in the winter of 1920/21, Heisenberg must have used others. Since Heisenberg also heard lectures on mathematics and rational mechanics by Aurel Voss at the University of Munich,[57] I think that it is plausible to assume that he also read Voss's contribution to Felix Klein's *Encyklopädie der mathematischen Wissenschaften mit Einschluß ihrer Anwendungen*, which appeared between 1901 and 1908. This is all the more likely since his main teacher Sommerfeld was himself involved with the editing of the physics part of this encyclopedia. Part I of the mechanics section includes Voss's survey of "rational mechanics," within which Hertz's treatment of mechanics and its precursors in the purely kinetic theories of William Thomson (Lord Kelvin) and J.J. Thomson are dealt with extensively.[58] Regardless of the route by which Heisenberg learned about Hertz's textbook, it is with respect to the two schemes in the last quote that Heisenberg then discussed the empirical compatibility and the criteria for a comparative evaluation of the schemes (discussed in sections 5 and 6), again with a strong Hertzian flavor, as is illustrated by the following quote in which we find several Hertzian motifs such as the emphasis on a significant freedom in introducing "model-like *Bilder*," necessary for an "appropriate formal description" of "empirical facts.'

The supposed hypothesis of a reciprocal two-valuedness affords for the use of model-like *Bilder* a degree of freedom which is higher than in any of the extant quantum-theoretic conceptions. This appears to enable us to achieve under the present state of theory a sensible formal description [*sinngemäße formale Beschreibung*] of the empirical phenomena in the complex structure of complicated spectra.[59]

How was this obscure "reciprocal two-valuedness" achieved in practice within Heisenberg's schemes I and II? Let us first look at scheme II, in which the duplicity is put into the description of the valence electron. The total energy $E(n,k)$ of the electron is divided into two parts, namely the internal energy [*Eigenenergie*] $E_E(n,k)$ and the interaction energy $E_W(n,k)$. Non-relativistically, the internal energy E_E is only dependent on the main quantum number n according to the usual Rydberg-Ritz formula $(E \sim 1/n^2)$, while the slight dependency on the azimuth quantum number k was a relativistic correction due to the fact that (semiclassically!) orbits with smaller orbit quantum numbers k are more eccentric and thus the electron comes closer to the atomic core, where it is accelerated towards higher velocities. Although the interaction energy E_W was uniquely specified by n and (half-integer) k, for every given E_E (with integer k) two possible E_W were available, depending on whether one chooses $k + 1/2$ or $k - 1/2$ (see Figure 2).

The reciprocity of the two-valuedness is clear from the fact that for every chosen $E_W\ (n,k)$ with semi-integer k two internal energies $E_E\ (n,k)$ were available which were slightly different due to the relativistic splitting.[60] Heisenberg also gave a similar diagram of reciprocal two-valuedness for the magnetic internal energy of the valence electron and the magnetic interaction energy between trunk and valence

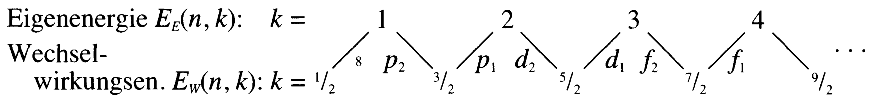

Figure 2. The reciprocal two-valuedness of internal energy E_E and interaction energy E_W according to Heisenberg, *op.cit.* (note 1), p. 844; a similar diagram already appears in Heisenberg's letter to Landé, February 2nd, 1925, AHQP 6,2 = DMM, microfilm no. 123, p. 2.

electron, where the former varied between positive and negative (integer) values m_k, while the latter varied between semi-integer values of m_k (both in steps of h). This meant that the total magnetic momentum of an atomic system was never completely parallel to the outer magnetic field, another expression of the magnetomechanical anomaly that excludes parallelism of angular momentum and magnetic momentum.[61]

Though scheme II successfully described the different energy levels of the terms labelled by the quantum numbers n and k, it could *not* account for the selection rules needed to predict which transitions between the many different energy levels of an atomic system the valence electron in fact made and which ones were "forbidden." These selection rules could only be derived from scheme I, since only in this scheme did the series electron have uniquely fixed quantum numbers, while in scheme II the angular momentum quantum number could have any of the two values of $k \pm 1$. The differences in the energy levels were reinterpreted in scheme I as caused by different angles θ of the angular momentum vector of the nuclear core relative to the series electron.[62]

Heisenberg's comparison of schemes I and II also yielded further virtues and disadvantages of the two alternatives: While scheme I could explain the transition from strong to weak magnetic fields and the associated Paschen-Back effect, scheme II allowed a simple interpretation of the g-values of Landé's formula.[63] Again and again, the curious result emerged that neither of these two schemes could account for all the observed peculiarities, while both schemes together covered the phenomenological regularities observed in one-electron systems quite well, in fact filling out each other's gaps.[64]

In order to describe atoms with two or more electrons, such as alkaline-earth atoms, Heisenberg developed a third scheme in which the duplicity of the two (or more) outer series electrons was shifted to the description of the atomic core:

> However, we can utilize further the freedom granted by the distribution, according to our basic hypothesis of the two-valuedness on the partial systems of the atom: We can cast the duplicity [*Duplizität*] of both electrons onto the atomic core resembling an inert gas [*Edelgas*]. [...] The third scheme which is thus attained by a naturally extended application [*naturgemäß verallgemeinerte Anwendung*] of our basic hypothesis can thus be placed as an equal next to the other two, and it leads to the following quasi-mechanical model.[65]

For alkaline-earth atoms, the atomic trunk thus did not have integral but semi-integral values of $r = 1/2$ and $3/2$, while the two outer electrons combine their angular momentum into one l which combines with r into the total angular momentum j of the atom. This step-by-step combination of the different angular momenta allowed a description of singlet and triplet lines in the spectra of these elements, and allowed the simple description of the transition from strong to weak magnetic fields, which was not possible within scheme I and II for systems with more than one electron.[66] The curious give and take between all three models of atomic systems is further illustrated in table 2.

Table 2: Heisenberg's three *symbolische modellmässige Bilder* to describe the anomalous Zeeman effect, condensed from Heisenberg's 1925 article.

Scheme	I	II	III
Source of nonmechanical duplication of stationary states	Atom trunk	Electron	RS-coupling of outer electrons
Empirical virtues	Accounts for selection rules	Accounts for energy levels	Accounts for occurrence of triplets and singlets and for transition from strong to weak magnetic fields
Problems	Does not account for energy levels	Does not account for selection rules and no application of correspondence principle	Does not account for absolute values of splitting widths
Representatives	Landé 1921, Heisenberg 1922	Pauli 1925	Heisenberg 1925, Russell-Saunders 1925

4.4. The later understanding of the anomalous Zeeman effect

Such periods of crisis (cf. Sec. 9.2), when several mutually incompatible *Bilder* are discussed and weighed without any really strong commitment on the part of the scientists to one or the other, generally do not last long. This is also true with respect to the crisis of the late quantum theory which led to the *symbolische modellmäßige Bilder* discussed in the foregoing. After the discovery of spin and the development of quantum mechanics in 1925, the unmechanical constraint was suddenly understood in a much simpler way. The concept of "spin" attributed to particles like the electron a new internal degree of freedom that could best be compared to an internal rotation of the electron around its own center of mass.[67] The duplicity of the angular momenta was reinterpreted as arising from the possible orientations of this internal axis of rotation parallel or antiparallel to external fields. So, in a sense, Heisenberg's scheme II, in which the stationary state of the electron was duplicated, was incorporated into the new quantum mechanics while Heisenberg's scheme I soon sank into oblivion. For multi-electron systems, scheme III (the so-called Russell-Saunders-coupling) remained in use as a description of all but the heaviest atoms, since the mutual coupling of spins and the mutual coupling of orbital angular momenta was most significant for all normal atoms. Only in atoms at the end of the periodic table do the orbital and spin angular momenta of individual electrons j_i first combine to total angular momenta of the individual electron j, which then combine to the total angular momentum J of the total atom in the so-called j-j-coupling. The new quantum mechanics could also account for the numerical totals of $|J|$ with $|J|$ integral for an even number of combined electrons, and $|J|$ semi-integer for an odd number of electrons in the atomic system under consideration, and with $J^2 \equiv J^2 = J(J + 1)$ and similar formulas for all other angular momentum vectors. This last result, especially which had been used as a mysterious empirical rule since Sommerfeld had redefined the quantum number $j := J-1/2$ in

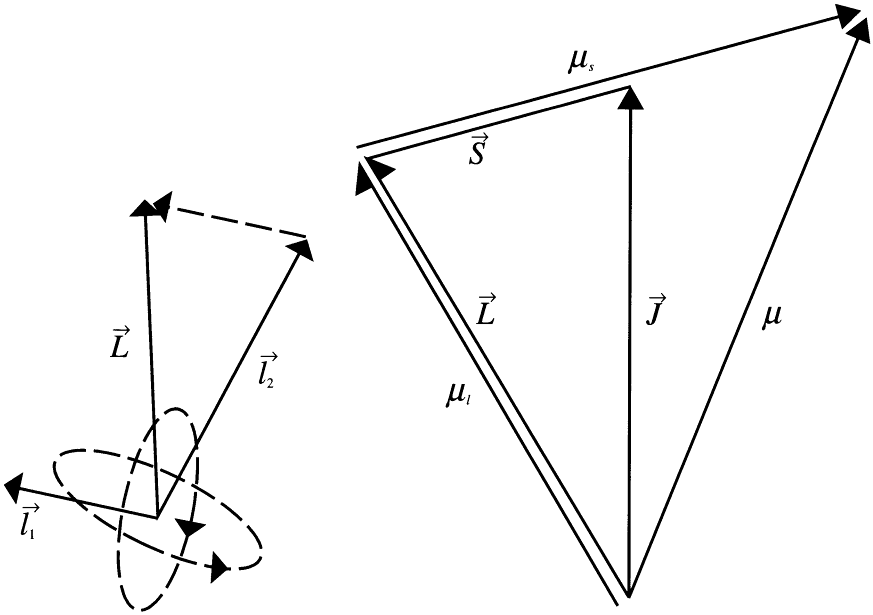

Figure 3. The combination of the orbital angular momentum vectors l_1 and l_2 to the total L-vector, the vectorial combination of $\vec{L}$ and $\vec{S}$ to the total $\vec{J}$ and the corresponding vectorial combination of the magnetic momenta to the total $\vec{\mu}$ which is not parallel to $\vec{J}$. Redrawn from *dtv-Atlas der Atomphysik* (Munich: dtv, 1976), pp. 40, 44.

1924, but could not be understood in the older frameworks of quantum theory, could now be elegantly derived from the commutation relations for angular momenta, that is from algebraic operations which in fact marked the core of the new quantum mechanics of Born, Heisenberg, Jordan and others.[68]

As became clear only later, the magneto-mechanical anomaly is connected to the fact that the magnetic moment $\vec{\mu}$ is usually connected to the angular momentum of a charged particle *via* $\vec{\mu} = e/2mc \cdot \vec{L}$, but for electrons which only carry spin 1/2, the magnetic moment μ is nevertheless a full Bohr magneton of *e/2mc*. This anomaly causes the total magnetic moment not to be parallel to the total angular momentum vector and therefore creates all the complications of the anomalous Zeeman effect.

5. THE EMPIRICAL ADEQUACY OF DIFFERENT "BILDER" AND THEIR NON-REPRESENTATIONAL CHARACTER

Hertz's definition of a *Bild* as only a partial mapping of a selected group of qualities into a sign representing this selected set (cf. section 3) already implies that this mapping is by no means unique, as Hertz was ready to point out in 1894:

> The images which we may form of things are not determined without ambiguity by the requirement that the consequents of the images must be the images of the consequents. Various images of the same objects are possible, and these images may differ in various respects. (PM 2)

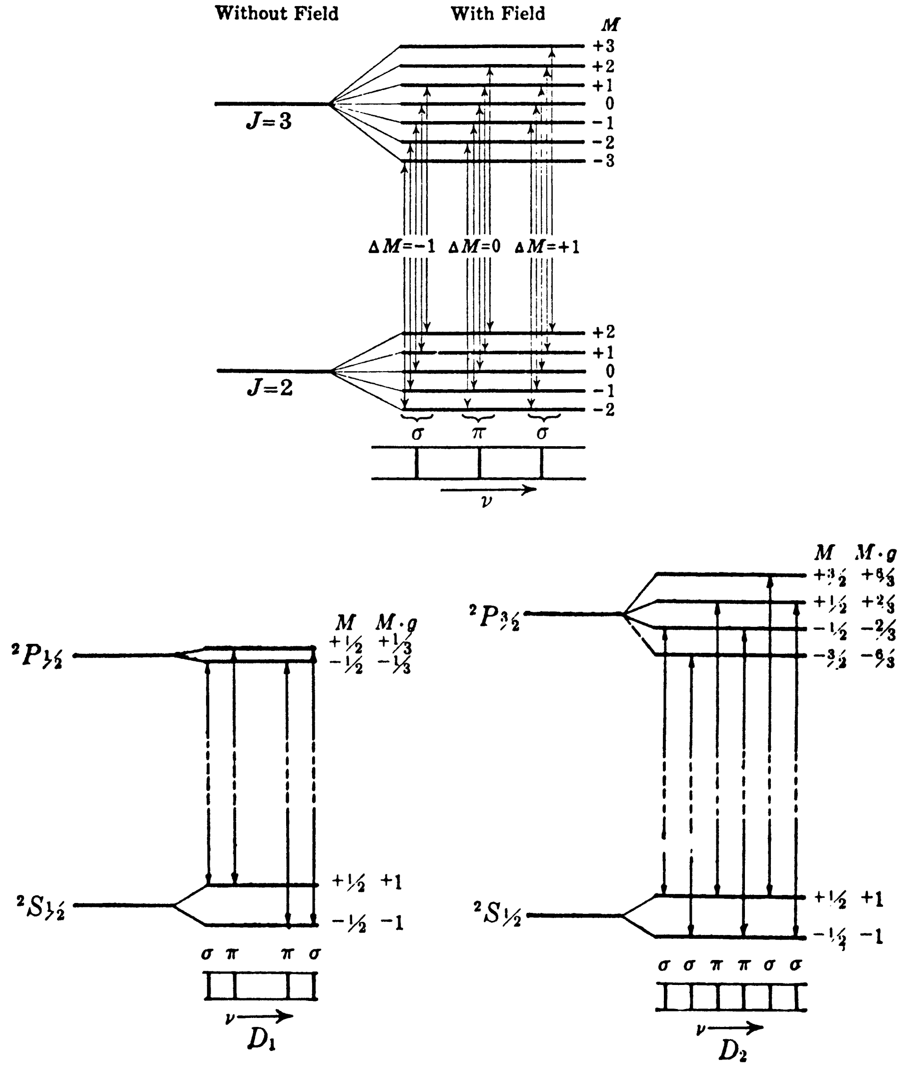

Figure 4. Term schemes and transitions for normal and anomalous Zeeman effect. Top: Normal Zeeman effect between $J = 3$ and $J = 2$; due to the equal spacings between the different energy levels labelled by different values of the magnetic quantum number M, the three groups of lines lead to only three distinguishable spectral lines for the three transitions with $\Delta M = 0, \pm 1$. Bottom: Due to different spacings of the levels $^2P_{1/2}$, $^2P_{3/2}$ and $^2S_{1/2}$, all 4 (respectively 6) transitions have different frequencies and thus lead to different components in the anomalous Zeeman effect. From: Gerhard Herzberg, *Atomic Spectra and Atomic Structure* (New York: Dover, 1945), pp. 104, 107 and 97 (top).

Thus, different *Bilder* (that is: mental images) of the same (unique) reality are possible: the mapping is a one-to-many mapping. By abandoning complete congruence with nature, the *uniqueness* of the mental representation often implied in former treatises of mechanics was thus given up.[69] After having introduced two of the three schemes sketched in section 4, Heisenberg stated:

These two *Bilder* [...] apparently stand *with equal legitimacy* side by side [*gleichberechtigt nebeneinander*], and the demand for the uniqueness [*Eindeutigkeit*] of the stationary states of the whole atom implies that in their consequences [*Ergebnissen*] they can *never contradict each other*. [...] If one considers the interaction of several electrons with one atomic core, then there may even be an entire sequence of equivalent pictures, which, if taken together, enable one to describe the stationary state of the atom.[70]

Thus, the different *Bilder* each cover all relevant empirical phenomena, and the extension of the *Bilder* to more complicated empirical phenomena will cause a further multiplication of empirically adequate symbolic representations [*gleichwertiger Bilder*]. Aside from this parallel between both authors, the last sentence of this quote also contains a new idea which, I think, is not yet present in Hertz's text, and which Heisenberg got from a different source (see section 8).

In part II of Hertz's *Mechanics*, in which he discusses dynamical models for the mechanics of material systems, Hertz becomes more specific on his use of the term "model" and on the criteria for the empirical compatibility of different models. First, he defines a material system as a model of another system, if and only if (i) the number of coordinates of both systems is the same, and if (ii) after a suitable correlation of the coordinates of both systems the same constraints prevail,[71] and if (iii) the expressions for virtual displacements are the same, given the correspondence between the coordinates according to (ii). In our context it is essential that once such a model for a material system is found, predictions of all relevant physical events in this material system can be based on the knowledge of the model alone:

In order to determine beforehand the course of the natural motion of a material system, it is sufficient to have a model of that system. The model may be much simpler than the system whose motions it represents. (PM §425)

Similarly, in Heisenberg's paper we can distinguish the level of "symbolic model-like *Bilder*," that is, the three schemes as outlined in 4.3, in their basic idea on the one hand, and the level of concrete model-building within them on the other hand. The latter level, concrete model-building, employs specific visual aids such as the vector-addition of $\boldsymbol{L}$ and $\boldsymbol{S}$ to form $\boldsymbol{J}$ (such as are drawn even now in introductory textbooks on quantum mechanics) and it could easily be made even more concrete in the form of physical models of arrows glued on top of each other with well-defined angles between them, rotating about the axis though the start- and endpoints of the open triangle. Heisenberg understood his three schemes for coupling nuclear and electron angular momentum as capturing the three simplest options open toward understanding certain features of the anomalous Zeeman effect, yet still on the level of abstract logical permissibility. The concrete models which resulted when these schemes were developed were certainly a very simplified mapping of the actual state of an atomic system, focusing exclusively on the magnitude of the resulting angular momentum observable in magnetic fields due to the anomalous Zeeman effect. Though a lot more information about electrons, atoms and fields was excluded from Heisenberg's models, the information about these selected quantities (essentially the four quantum numbers n, l, k, m) sufficed for a more or less successful prediction of the splitting patterns of spectral lines in the magnetic field as the then observable consequence of all this otherwise unobservable atomic tohuwabohu.[72]

Concerning Heisenberg's pragmatic comparison of his three schemes, he specified (in true Hertzian spirit) the *conditio sine qua non* for all these different representations to correspond to the same physical system:

The condition for treating schemes I, II, and III as different aspects [*verschiedene Seiten*] of the same physical problem [...] is once again that whenever certain results can be deduced from various of these schemes, the schemes yield identical results.[73]

Though Heisenberg later attributed the idea of focusing on observables very much to Einstein's influence,[74] it is probable that he was at least as much influenced by his colleague Wolfgang Pauli who had already realized in 1924 that quantum theory should concentrate more than before on the prediction of observable quantities:

I have also consistently avoided using the notion of an "orbit" in my paper. [...] I believe that the energy and momentum values of stationary states are much more real than "orbits." [...] We must not bind the atoms in the chains of our prejudices – to which, in my opinion, also belongs the assumption that electron orbits exist in the sense of ordinary mechanics – but we must, on the contrary, adapt our concepts to experience.[75]

The realization of the legitimacy of having different, mutually incompatible, but pragmatically equally valuable *Bilder* of the same system, also had important consequences for the understanding of the role of models in general: It implied their non-representational character. Wolfgang Pauli, the godchild of the phenomenalist Ernst Mach, was already well ahead in realizing the full implications of this in the early twenties. In a letter to Sommerfeld in December 1924, he commented upon the earlier efforts to find representational models with the following prophecy (which later proved to be correct):

There is now a deep and principled crisis for model conceptions, and I believe that it will result in a further radical accentuation of the division [*Verschärfung des Gegensatzes*] between classical and quantum-theory. [...] it will hardly be possible to maintain the notion of definite unique orbits for electrons in the atom. With all models one now gets the impression that with them we are speaking a language that is not sufficiently adequate for the simplicity and beauty of the quantum world.[76]

It is not clear to me precisely when Heisenberg adopted the view that all the quasi-classical models of atomic systems and processes such as transitions from one quasi-classical orbit to the other, were doomed to failure. His skepticism towards the traditional aim of physicists finding a unique representation of reality grew slowly during the early 20's, when he actively searched for such a selfconsistent and empirically adequate model, not only for the anomalous Zeeman effect, but also for resonant fluorescent radiation and dispersion phenomena.[77] Heisenberg apparently first used the term "model" with the full Hertzian connotation in a letter to Pauli in October 1923, in which he sketched a new approach to the problem of the anomalous Zeeman effect he was pursuing in Göttingen together with Max Born:

The model representations have, in principle, only a symbolic sense; they denote the classical analogue of the "discrete" quantum theory.[78]

Although Heisenberg did not stop improving his atomic core model, it is clear that by December 1924, he was well aware of the shortcomings of any model-dependent description of atomic processes. By calling all models proposed so far "swindles" [*Schwindel*], he alluded to their one-sided incompleteness, and to the partiality of all possible mappings of atomic systems with many parameters in schemes confined to a combination of a few independent quantum numbers.

[...] you have pushed the swindle to as yet unknown treacherous heights and have thus easily broken all previous records with which you castigated me (in that you recommended single electrons with 4 degrees of freedom) [...]. And if you think you have written something against the previous sorts of swindle, then this is naturally a misunderstanding, since a swindle times a swindle cannot yield anything correct [...].[79]

While his sharp-tongued colleague Wolfgang Pauli was already about to abolish model-dependent descriptions of atomic phenomena altogether and to look instead for a purely symbolic and formal way of describing atomic systems, Heisenberg played with the idea of "combining the different formalisms of the Zeeman effect together into a unified description," with the hope of arriving in due course at a new and unified theory.[80] Similarly, Hertz never reached the point where he was willing to dispense with mechanical models of the aether altogether, as most of the continental theoreticians working in electrodynamics after him did, although he came close to it.

6. THREE CRITERIA TO EVALUATE MODELS

According to Hertz, all *Bilder* and their respective models have to be evaluated according to the three criteria of permissibility [*Zulässigkeit*], empirical adequacy [*Richtigkeit*] and appropriateness [*Zweckmässigkeit*]; but this implies that all models that have passed the first two tests by being neither internally contradictory nor in any essential sense contradictory to what they signify,[81] are empirically on a par [*gleichberechtigt*]. Since most models that the scientific community takes seriously will pass the first two (syntactic and semantic) tests, a third criterion gains additional importance, since the acceptance or rejection of one among a whole set of competing models hinges mostly on the evaluation of its approriateness or suitability [*Zweckmässigkeit*]:

Of two images of the same object, the more appropriate is the one which pictures more of the essential relations of the object – the one which we may call the more distinct. Of two images of equal distinctness the more appropriate is the one which contains in addition to the essential characteristics, the smaller number of superfluous or empty relations – the simpler of the two. (PM 2)[82]

Thus, Hertz associated the "appropriateness" of a scheme with its "simplicity" or economy of independent theoretical assumptions. Mach's reception of Hertz further supports this reading since, in the third edition of his *Mechanik in ihrer Entwicklung historisch-kritisch dargestellt*, published in 1897, he stated that "Hertz's criterion of appropriateness coincides with our criterion of economy."[83] This coincidence is by no means accidental, since Hertz had read Mach's and Dühring's books on *Mechanik* soon after their appearance (MLD 189, 191). While Hertz's recourse to the criterion of "simplicity" closely parallels Poincaré's insistence on the importance of this selection principle, Hertz seemed to be less confident than Poincaré that the selection of the "simplest" model should always find a unique answer:

All physicists agree that the task of physics consists in tracing the phenomena of nature back to the simple laws of mechanics. But there is not the same agreement as to what these simple laws are. (PM xxxvii)
[...] we cannot decide without ambiguity whether an image is appropriate or not; as to this differences of opinion may arise. One image may be more suitable for one purpose, another for another; only by gradually testing many images can we finally succeed in obtaining the most appropriate. (PM 3)

Simplicity also figures prominently in Heisenberg's discussion of his respective models in atomic physics, although in quotes like the following, he is less clear about simplicity being a *desideratum* of the model and not of the described material system than Hertz is, who was crystal-clear about this.

Although the quantum-theoretical laws of the interaction of electrons within the atom are doubtlessly characterized by great simplicity, it seems that at the moment there exists no other way to interpret these laws than to employ model-dependent pictures of a symbolic nature in which this simplicity is hardly reflected in a satisfactory manner.[84]

7. APPROPRIATENESS AND THE FOCUS ON OBSERVABLES

A recurrent motif in Hertz's mechanics is a critique of concepts that have no semantic significance but have only been dragged along by the formalism. Paradigmatic for this strand is Hertz's critique of the concept of force as the key part of scheme I for mechanics (cf. section 3):

It cannot be denied that in very many cases the forces which are used in mechanics for treating physical problems are simply sleeping partners [*leergehende Nebenräder*] which keep out of business altogether when actual facts have to be represented. (PM 11f.)

According to Hertz, forces are all too often introduced only to account for Newton's action-reaction principle; in gravitation, forces are introduced only as auxiliary concepts [*Hilfsgrößen*] necessary to calculate accelerations and positions as directly observable. Hertz thus pleads for a principle of conceptual parsimony according to which physics should not introduce more theoretical elements than necessary to describe what can be observed.

But has physics always been sparing in the use of such relations? Has it not rather been compelled to fill the world to overflowing with forces of the most various kinds – with forces which never appeared in the phenomena, even with forces which only came into action in exceptional cases? (PM 12)

With the example of a piece of iron resting on a table, Hertz then illustrates the enormous multiplication of forces needed to account for this simple sense datum in terms of the concepts of the traditional mechanical scheme I:

We see a piece of iron resting upon a table, and we accordingly imagine that no causes of motion – no forces – are there present. Physics, which is based upon the mechanics considered here and necessarily determined by this basis, teaches us otherwise. Through the force of gravitation every atom of the iron is attracted by every other atom in the universe. But every atom of the iron is magnetic, and is thus connected by fresh forces with every other magnetic atom in the universe. [...] But, in fact, all the forces are so adjusted amongst each other that the effect of the whole lot is zero; that in spite of a thousand existing causes of motion, no motion takes place; that the iron remains at rest. Now if we place these conceptions before unprejudiced persons, who will believe us? Whom shall we convince that we are speaking of actual things, not images of a riotous imagination? And it is for us to reflect whether we have really depicted the state of rest of the iron and its particles in a simple manner. (PM 13)

Of course it must be mentioned that although Hertz's reason for developing an alternative scheme to account for the laws of mechanics might have been this principle of conceptual parsimony, he himself ended up postulating indefinitely many unobservable masses to close up the chain of action from one observable mass to another mass; so it is by no means clear how Hertz did in fact improve the foundations of mechanics just by eliminating forces at the cost of introducing auxiliary unobserved masses.[85]

Aside from Hertz's mistrust of the appropriateness of the concept of force for the foundations of mechanics, he was also driven to an emphasis on observable quantities by his conception of models:

If the same quantities are corresponding coordinates of a number of material systems which are models of one another, and if these corresponding coordinates alone are accessible to observation, then, so far as this limited observation is concerned, all these systems are not different from one another; they appear as like systems, however different in reality they may be in the number and connection of their material points. (PM §426)

Space does not permit a closer analysis of Heisenberg's later focus on observables and its consequences within the framework of the Copenhagen interpretation of quantum mechanics.[86]

8. THE MULTIPLICITY OF MODELS, THE CORRESPONDENCE PRINCIPLE AND COMPLEMENTARITY

While writing his April 1925 paper, Heisenberg was staying at the *Institut for teoretisk Fysik* in Copenhagen as a fellow of the Rockefeller foundation's *International Education Board*, and was working with Niels Bohr, whom he had known since Bohr's lectures in Göttingen in 1922.[87] Although Bohr only introduced his concept of complementarity formally in 1927 in his famous Como lecture, he certainly pondered about it a lot before exposing his idea in public, and certainly spoke about it with his collaborators, in particular with Pauli and Heisenberg during their common stay in Copenhagen during March/April 1925.[88] Anyone familiar with Bohr's concept, must hear the ring of complementarity also in these words from Heisenberg's 1925 paper:

> [...] Rather the two *Bilder* will have to complement each other, just because of that uniqueness of their consequences, in such a way that the quantities which remain undetermined in one scheme will be determined in the other, and vice versa. Both schemes together will provide, so to say, a convergent method of calculation for determining the properties of the stationary states of the atom.[89]

The first part of this quote already contains the germ of Bohr's concept of complementarity: namely the exclusion of sharply defining the one when fixing the other of two "complementary" physical qualities. The latter part, on the other hand, envisions a kind of convergence of the two schemes which is different from Bohr's later interpretation of complementarity. When Heisenberg again used the term *Bild* in his overview article on the history of quantum theory in 1929, he had already come to accept Bohr's reading of complementarity, although the Hertzian connotation of *Bild* implying the non-uniqueness of this mental representation, remained.[90]

As I see it, the main source for Heisenberg's well-balanced evaluation of two conflicting schemes for the description of physical phenomena is his understanding of Bohr's and Ehrenfest's correspondence principle.[91] In the last preserved letter to Sommerfeld prior to his April 1925 paper on the anomalous Zeeman effect, Heisenberg wrote:

> I am more and more inclined to think that the question "light quanta or correspondence principle" is merely a matter of definition. All quantum theoretical effects must have their counterpart in classical theory, since the classical theory is almost right. Therefore, all effects always have two names: a classical and a quantum theoretical name, and which one is regarded as preferable is merely a matter of taste.[92]

I might add that I found similar statements about the translatability of the terms of an old theory into those of a new theory in the context of Hertz's researches in electrodynamics.[93]

The foregoing quotes concerning the principles of correspondence and complementarity should remind us that Hertz's influence is certainly only one among several influences – at least equally important for the understanding of Heisenberg's paper of 1925 are, of course, Niels Bohr's and his collaborators' virtuosic use of the correspondence principle in Copenhagen, Sommerfeld's and Landé's model-building tradition enriched with *Zahlenmystik*, which dominated the history of early quantum theory up to 1925,[94] and in addition the competing approaches of the schools of Ehrenfest in Leyden and Born in Göttingen. All of these were based on the reliable and extensive experimental study of Zeeman patterns in magnetic fields carried out by Paschen and Back in Tübingen.[95] In conformity with the style of papers published in the modern-minded *Zeitschrift für Physik*, these internal traditions of prior work on the anomalous Zeeman effect are the only ones Heisenberg explicitly referred to in his footnotes to the paper. It is all the more exciting that we can trace a clearly identifiable strand from quite a different sector of physics.[96] But what is the ultimate purpose of this exercise, apart from contributing to the "Heisenberg philology" which has focussed so far on influences by Plato, Kant, Mach and Einstein?[97]

Although the main bulk of this paper was an argument for fairly strong terminological and structural similarities between Hertz's and Heisenberg's texts, I do not want to neglect reflecting a bit on the possible use of such an intertextual comparison.

9. ON THE BROADER SIGNIFICANCE OF HEISENBERG'S RECEPTION OF HERTZ'S PHILOSOPHY OF SCIENCE DURING A PERIOD OF CRISIS

Given the strong influence of Hertz's textbook on mechanics and its *Bild* conception of scientific concepts on the young Heisenberg, I would like to end this paper by addressing the question of *why* Hertz's book might have been so interesting to Heisenberg precisely in early 1925, at a time of serious crisis of the old system of quantum theory. It is tempting to draw a parallel to the situation in which Hertz made his famous contributions to electrodynamics, as described by Hermann von Helmholtz in his introduction to Hertz's posthumous textbook. A proliferation of competing models for electric and magnetic forces had been offered, such as W. Weber's instantaneous actions at a distance, and the alternative accounts of F.E. Neumann, Riemann, Grossmann and others, all differing in the precise form of the interaction potential which was held to be dependent on the relative distance, velocity and acceleration between two electric charge quanta.[98]

> This plentiful crop of hypotheses had become very unmanageable, and in dealing with them it was necessary to go through complicated calculations, resolutions of forces into their components in various directions, and so on. So at that time the domain of electromagnetics had become a pathless wilderness. Observed facts and deductions from exceedingly doubtful theories were inextricably mixed up together. (PM xxvi)

If I had not mentioned that this quote was by Hermann von Helmholtz, and if the word "electromagnetics" had not appeared in it, you might have thought this a characterization of the state of the art in quantum theory after 1922.

9.1. *The crisis of quantum theory: Mysteries and anomalies*

The successes of the early quantum theory in roughly explaining specific heats and hydrogen-like spectra repeated themselves neither when it came to explaining the more complicated patterns in spectra of multiple electron systems even with systems as simple as helium,[99] nor, even worse, in calculating line intensities.[100] Physical effects such as the anomalous Zeeman effect, though studied by some of the brightest theoreticians of the age, seemed not to be describable in purely mechanical terms; quantum number assignments needed to explain one feature flatly contradicted those needed for other features, and the limits of the conventional wisdom had been reached, or so went the gossip, spreading throughout the centers of theoretical research of the time.

Since Landé had systematically ordered and formalized [*geordnet und in Formel gebracht*] according to the extant quantum-theoretical principles the empirical material associated with the anomalous Zeeman effects, it became increasingly clear that an explanation of the appearances of the anomalous Zeeman effect would require far reaching changes in our quantum-theoretic conceptions [*tiefgreifende Änderungen in unseren quantentheoretischen Vorstellungen*].[101]

Though the former quantum theory had become a "thing of rags and patches" as Edward MacKinnon puts it, quoting W.S. Gilbert, all these difficulties were not taken as sufficient reason to abandon the proposed models – the fiddling and grafting went on, with Heisenberg as one of the most energetic and eager tinkerers.[102] Referring to Heisenberg's letter to Pauli of November 19, 1921, in which he expressed his guiding principle in developing his 1921 atomic core model, "Success sanctifies the means," Mehra and Rechenberg characterized Heisenberg's overall approach to the theory of the anomalous Zeeman effect and contrasted it to Sommerfeld's as follows:

He [Heisenberg] was willing to use every theoretical idea, even if it was not generally accepted by others, in order to arrive at formulae fitting the data. [...]. Heisenberg went deeply into the mechanical details of atomic models, whereas Sommerfeld confined himself to applying only general principles. In applying the laws of dynamics, Heisenberg allowed himself certain liberties, which Sommerfeld would have avoided. He employed selected parts of mechanics, electrodynamics, quantum theory and whatever else he needed in order to achieve his goal – in agreement with his motto, "success sanctifies the means" – and he was less concerned about the internal consistency of these theoretical parts and the basic assumptions of his model.[103]

Besides the ominous semi-integral quantum numbers, these theoretical ideas introduced by Heisenberg also included speculations about a possible violation of Rubinowicz's selection rules which expressed the conservation of total angular momentum of atom and radiation during emission and absorption and which Heisenberg believed to be fulfilled only in the average for ensembles of many atoms.[104] Most importantly, Heisenberg was even prepared to "graft" unmechanical characteristics to his otherwise mechanical atomic core model, just to account for certain experimental results.[105] Indeed, Wolfgang Pauli wittily remarked that Heisenberg tried to add two "nonsenses" in his discussion of the two schemes which he knew to be mutually incompatible with the hope of achieving a higher truth:

I think that what I am doing here is no greater nonsense than the previous conception of complex structure. My nonsense is conjugated with the nonsense that has been customary. Just for this reason I believe that this nonsense is necessary in the present state of the problem. The physicist who succeeds in adding the two nonsenses will obtain the truth.[106]

9.2. Characteristics of the crisis situation

This general characterization of the style in which atomic model-building was practiced prior to mid-1925 shows us, more than any further discussion of the details of Heisenberg's model for the anomalous Zeeman effect, that quantum physics was indeed at a serious impasse. This is the more so, since the exponents of this tactic of educated guesswork were well aware of the "dark sides" [*Schattenseiten*] of tinkering with model assumptions that lacked strict internal consistency. The frustrated participants themselves ironically dubbed it "number mysticism" [*Zahlenmystik*], "nonsense" [*Unsinn*] and even "fraud" or "swindle" [*Schwindel*][107] and referred to themselves as "Philistines of formalism" [*Formalismusphilister*],[108] who "always had to work in a kind of fog of uncertain knowledge."[109]

The specifics of this crisis situation thus included:

* The profileration of many competing models without any clear-cut criterion for choosing between them (see here sections 5–6), which was
* encouraged by the empirical compatibility of these models from the pragmatic point of view,
* and by unresolvable intertheoretic contradictions.[110]
* A quite formalistic mentality, which even allowed strange theoretical assumptions to be introduced in specific contexts as long as they helped adequately to describe observed but not yet understood phenomena,[111]
* even if they contradicted fundamental laws of physics such as conservation principles,[112]
* or well-established theories such as classical mechanics and electrodynamics.[113]
* A far-reaching ignorance about the microprocesses leading to the observed regularities in macrophysics,[114]
* in turn enforcing a focus on pragmatic virtues of models such as breadth of application, and precise match of theoretical predictions with experimental measurements,[115]
* combined with a deep skepticism of the representational character of the diverse models under discussion.[116]
* A widespread feeling of desperation[117]
* or of disgust when confronted with the piecemeal fashion in which the existing models were adapted to the complex empirical data through increasingly *ad-hoc* maneuvering.[118]
* A widespread sense of an imminent breakthrough lurking behind the next corner,[119]
* and (at least among the more radical physicists) a search for completely different approaches which ultimately led to the development of quantum mechanics from mid-1925 on.[120]

Both Hertz in the late 19th century, and Heisenberg in 1924/25 were facing situations of immediate and serious crisis. Both were well-acquainted with the several proposed "schemes" which claimed to be ways out of the predicament, and became committed to one or the other of them at various stages of their work.[121] But both understood that in a sense all of these schemes were empirically compatible, that each of them had some virtue *and* some fault, and they were careful in placing their bets, as is demonstrated, e.g., by Heisenberg's "closing remark":

> It should hardly be necessary to emphasize the preliminary and in many respects unsatisfactory character of the formulation of the problem of multiplet structure which is attempted in this paper.[122]

Needless to say, most of the characteristics of the crisis situation exemplified by quotes from the correspondence and papers of physicists around 1925 can also be retraced in the crisis situation of mechanics in the late 19th century. While Hertz could feel free to postulate his unobserved masses (cf. section 3), Heisenberg felt free to postulate certain coupling aspects of the angular momentum vectors: Both served to fill in the gaps between certain deep-seated theoretical features (such as the existence of quantum numbers) on the one hand, and the prediction of the magnitude of certain observables (such as the splitting patterns of spectral lines in the magnetic field) on the other hand. For both, the relevant criteria for comparing their own favorite models with other contemporary models were the ones discussed by Hertz (who, in turn, had faced the contemporary rival account by Kirchhoff – see section 6): logical permissibility, empirical adequacy, and appropriateness. Hertz as well as Heisenberg did *not* stay within the confines of a phenomenalist science which supposedly only discusses observable magnitudes,[123] but both carefully tried to restrict their burden of nonobservable, purely theoretical entities within self-imposed rules on where to stop this theoretical gap-filling. While Hertz justified his introduction of hidden masses by its prospect of making us understand the mechanism of transmission of forces, Heisenberg was equally willing to introduce "intermediary kinds of reality" such as, for instance, the waves which according to the Bohr-Kramers-Slater-paper of 1924 produced probabilities for decay or emission without being as real as, say, electromagnetic waves:

I found it extremely attractive, because I found that this idea of having such intermediate kinds of reality was just the price one had to pay for understanding quantum theory. By that time one knew that one had to pay a high price and you could not get any cheap solution. [...]. So I was a bit sceptical with the conservation of energy.[124] There, everybody felt, "Well, that's very dangerous. You can get on the wrong track." But still, this idea of having the waves which were just not quite real, but almost real, that was the thing of the right kind.[125]

In particular, both Hertz's and Heisenberg's models worked with concepts already prevalent in models of the time without introducing anything drastically new; that is, both models involved slight improvements over existing models, and aimed at a unification by using a different combination of already existing pieces.[126] Despite the fact that both models were explicitly compared with other alternative models proposed at the time and praised by their protagonists for their comparative logical economy and appropriateness, neither of them was long-lived – in fact neither of them appears to have been considered seriously and elaborated further by the scientific community, sometimes not even by the protagonists themselves:

It is a rather unusual state in physics that people would dare write papers of that kind. It was again this strange situation that everybody agreed by that time [1924] that physics did contain contradictions and it couldn't be helped. In such a situation, one is willing to take papers which only go somewhere in the right direction, even if they don't reach the goal.[127]

While the reception of Hertz's *Mechanik* might be illustrated by FitzGerald's polite but unenthusiastic comments on it in *Nature*, Pauli's reception of Heisenberg's paper and Kronig's contemporary paper was as follows:

I am not interested anymore in multiplets and anomalous Zeeman effects. Unless someone has a really new physical idea, we won't have any progress. The mere extension of the formal term-zoology to more and more complicated cases is to me basically a fruitless affair.[128]

Seen in the light of Heisenberg's recourse to the Hertzian "scheme comparison," Hertz's mechanics is mechanical zoology in the same sense in which Heisenberg's schemes were considered "Zeeman botany."[129] Both Hertz's and Heisenberg's contributions compared in this paper mark the close of a "classical" period of theoretical physics and the advent of the "modern."[130] Indeed, Hertz's hidden masses were henceforth often enough only reported as prime exemplars of the strange creatures (or should I say "monsters") envisioned by late 19th century mechanics. It was a new physical idea by Heisenberg in early summer 1925, to describe the transitions between different quantum states with what turned out to be matrices, that soon caused a much more radical and lasting revision.[131] Hertz's philosophy of models in science would thus appear to be a philosophy characteristic of transition periods shortly before conceptual breakthroughs. This philosophy would be proposed by researchers still involved in "grafting" *ad hoc* hypotheses into highly involved, but intuitively less and less compelling card-houses. It is for this reason that both Hertz and Heisenberg seem to have had second thoughts about their respective texts, that both of them intuitively felt that they had not yet fully achieved their ultimate aim. While Hertz's entry in his diary of November 19th, 1893 is fairly well-known,[132] similar doubts by Heisenberg about the ultimate adequacy of his models may be less familiar:

> I did realize that the explanation by means of a model really didn't work in some way. It worked only if one did things which were really not justified from classical mechanics, or at least I felt that the whole thing was a bit weak. On the other hand, I also felt that this was interesting and it looked as if it contained a number of things which looked like real physics. You know it was this standard situation in an undeveloped field of physics where you felt that you have gotten hold of some parts of reality, only you cannot rationalize it to the end. You cannot really get a perfectly clear picture. And, on the other hand, at the same time I felt, "Well, the picture is so interesting, why not give it to others, even if it's not clear."[133]

Precisely because the decision between competing models could not be based on the standard criteria of empirical adequacy and freedom of internal contradictions alone, both Hertz and Heisenberg had to turn to criteria of comparative theory evaluation along the lines of Hertz's introduction to his mechanics.

If this link between a certain (Hertzian) conventionalistic attitude towards models and specific problem situations in science suggested by my analysis does in fact exist, a last set of questions poses itself:

Did other quantum theorists who faced the same situation of crisis of the old quantum theory also refer to Hertz's mechanics at that time? Furthermore, did other scientists who were confronted with similar situations of crisis of model-building traditions in other fields at other times also revert to Hertzian *Bild* motifs? If so, this would considerably strengthen my claim.

Does the revival of interest in Hertz's *Bild* conception which we are witnessing today (and actually furthering by contributing our papers to this volume) indicate the occurrence of another scientific crisis situation similar to the ones which Hertz and Heisenberg faced? Both were confronted with a state of physics in which lots of *ad-hoc* maneuvers were made within a sophisticated framework to save the phenomena. There was a good prospect of a new and unified theory that would cope with the perplexing anomalies in a radically different way, and many participants felt it to be imminent although still "around the corner," that is, out of the direct line of sight. The breakthrough then came from very different theoretical considerations, not directly related to the model-grafting business going on before. Are there not indeed parallels to the present situation of the standard model of strong, electromagnetic and weak interactions, with all its baroque fine-tuning of coupling strengths and other free parameters, but with persistent problems in incorporating gravity into the scheme?

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NOTES

[1] See Werner Heisenberg, "Zur Quantentheorie der Multiplettstruktur und der anomalen Zeemaneffekte," *Zeitschrift für Physik* **32** (1925), pp. 841–860 [reprinted in Werner Heisenberg, *Gesammelte Werke: Collected Works*, edited by W. Blum, H.P. Dürr, and H. Rechenberg, vol. A I, pp. 306–328]. The paper was received on April 10, 1925 and published in issue no. 10/11 of June 30, 1925. Cf., e.g., Friedrich Hund's introduction to Heisenberg's papers on atomic and molecular structure (1922–1925) in Heisenberg, *Collected Works*, vol. A I, pp. 127–133, and Jagdish Mehra and Helmut Rechenberg's *The Discovery of Quantum Mechanics 1925*, volume 2 of their *Historical Development of Quantum Theory* (Berlin: Springer, 1982), section II.5; cf. also chapter V, pp. 261ff., for an account of Heisenberg's discovery of quantum mechanics in the second half of June and early July, 1925.

[2] See, e.g., Armin Hermann, *Werner Heisenberg in Selbstzeugnissen und Bilddokumenten* (Reinbek: Rowohlt, 1976); David Cassidy, *Uncertainty: The Science and Life of Werner Heisenberg* (New York: Freeman, 1992), pp. 46–48, chapters 6 to 8; M. Stöckler, C. Liesenfeld, and H. Seidel, in Bodo Geyer *et al.* (eds.), *Werner Heisenberg: Physiker und Philosoph*, pp. 335–355; for the physics context, see in particular Paul Forman, "Alfred Landé and the Anomalous Zeeman Effect, 1919–1921," *Historical Studies in the Physical Sciences* **2** (1970), pp. 153–261; Edward MacKinnon, "Heisenberg, Models, and the Rise of Matrix Mechanics," *Historical Studies in the Physical Sciences* **8** (1977), pp. 137–188; Daniel Serwer, "Unmechanischer Zwang: Pauli, Heisenberg, and the Rejection of the Mechanical Atom, 1923–1925," *Historical Studies in the Physical Sciences* **8** (1977), pp. 189–256; and the secondary texts mentioned in notes 1 and 3. See also T.S. Kuhn's interview with Heisenberg, November 30, 1962 in *Sources for History of Quantum Physics*.

[3] Apart from the more specific studies mentioned in the previous note, I have also checked broader overviews on the development of quantum theory and quantum mechanics, such as Max Jammer, *The Conceptual Development of Quantum Mechanics* (New York: McGraw Hill, 1966), chapter 3; Friedrich Hund, *Geschichte der Quantentheorie*, third edition (Mannheim: Bibliographisches Institut, 1984), chapters 9 to 12; Mehra and Rechenberg, *op.cit.* (note 1); Olivier Darrigol, *From c-numbers to q-numbers: The Classical*

Analogy in the History of Quantum Mechanics (Berkeley: University of California Press, 1992), chapters 7 to 10.

4 The German terms used by Heisenberg are "*modellmäßige Bilder*" and "*Schema*," later he also introduces "quasimechanical models of schemes" with "*Bild*" and "*Schema*" used interchangeably, while Hertz only uses the word "*Bild*": I will consistently leave the term "*Bild*" untranslated, and translate "*Schema*" as "scheme," and "*Modell*" as "model."

5 Cf. PM 2 for a discussion of the criteria for permissibility, correctness and appropriateness [*Zulässigkeit, Richtigkeit und Zweckmässigkeit*] of *Bilder*; see also Alfred Nordmann's contribution to this volume.

6 "In the future the book may prove to be of great heuristic value as a guide to the discovery of new and general characteristics of natural forces" (Helmholtz in PM xxxvi). The usual evaluation of the impact of Hertz's textbook by scientists as well as by historians sounds different, such as Boltzmann's "I have often heard Hertz's mechanics praised yet never seen anybody pursue the path he indicated" (Boltzmann 1974, 88), or McCormmach 1972, p. 348: "It seems that the heuristic value was not realized." Cf. also Reden 1988, pp. 69f.

7 Heisenberg, *op.cit.* (note 1), p. 842; in his earlier papers on the Zeeman effect and related spectroscopic matters, Heisenberg did *not* use this vocabulary – nor did other quantum theorists such as Kramers, Landé, or Pauli. The latter, however, was very important for Heisenberg due to his "insistence that atomic models could not be considered realistic representations of atoms"; the earliest use that I have found of the word "symbolic" in Heisenberg's correspondence is in his letter to Landé, Jan. 4, 1925: (Archive for History of Quantum Physics: AHQP, 6,2 = Deutsches Museum Munich: DMM, microfilm no. 123). See also the Pauli quote on p. 202 below and McKinnon *op.cit.* (note 2), p. 177.

8 I follow Nordmann's critique of the English translation of the third sentence in just using "things" instead of "things themselves'; the emphasis is original in both versions of the text. Mach also interpreted Hertz's *Bild* as synonymous with *Begriff* (concept): "The constructive images (or better, perhaps, the concepts), which we consciously and purposely form of objects [...]" (1960, p. 318). According to G. Frey's classification, Hertz's *Bilder* are thus "non-iconic symbolic models," cf. Frey's "Symbolische und ikonische Modelle," *Synthese* **12** (1960), pp. 213–221. See also Gregor Schiemann's contribution on the origin and development of the concept of image in this volume.

9 See, e.g., Boltzmann 1974, pp. 89ff.; d'Agostino 1990, pp. 382; on the many meanings and functions of "models" cf. also the contributions of Leo Apostel, G. Frey, H.J. Groenewold, and Patrick Suppes to Hans Freudenthal's Colloquium on "The Concept and the Role of the Model in Mathematics and Natural and Social Sciences," *Synthese* **12** (1960), pp. 125–161, 213–227, 287–301; Mary Hesse, *Models and Analogies in Science* (Notre Dame: University of Notre Dame Press, 1966), pp. 7ff.; and the contribution by Simon Saunders to this volume.

10 Gustav Robert Kirchhoff, *Vorlesungen über Mathematische Physik: Mechanik* (Leipzig: Teubner, 1876), §1: "*als ihre Aufgabe bezeichnen wir: die in der Natur vor sich gehenden Bewegungen vollständig und auf die einfachste Weise zu beschreiben.*" See also Mach 1960, p. 325; Voss 1901-08, §3, pp. 13f.; Boltzmann 1974, pp. 88f.; Conrad Heinrich Müller and Georg Prange, *Allgemeine Mechanik* (Hannover: Helwigsche Verlagsbuchhandlung, 1923), p. 1. For Kirchhoff's influence on Hertz see Reden 1988, p. 77, and Hermann 1988, pp. 8–9. For a discussion of conventionalism in physics see Werner Diederich, *Konventionalität in der Physik* (Berlin: Duncker und Humblot, 1974); and for the history and implications of the concept "model" see Frey, *op.cit.* (note 8); Mary Hesse's "Models and Analogy in Science," in Paul Edwards (ed.), *The Encyclopedia of Philosophy* (New York: Macmillan, 1967), vol. 5, pp. 354–359; Boris Spassky "Le développement du concept de modèle en physique," *XII. Int. Congrès d'Histoire des Sciences* (Paris, 1971), **5**, pp. 91–94; Gereon Wolters, "Modell," in J. Mittelstraß *et al.* (eds.), *Enzyklopädie Philosophie und Wissenschaftstheorie* (Mannheim: Bibliographisches Institut, 1984), vol. 2, pp. 911–913; and Margaret Morrison: "A Pragmatic View of Modeling in Physics," forthcoming in *Philosophia Naturalis* (1997).

11 Hertz's motive for writing this book is perhaps most succinctly put in his letter to Emil Cohn, Nov. 27, 1891: "This past summer I reflected a lot about ordinary mechanics [...] Here I would like to put some things in order and to determine the order of concepts in such a manner that one can see more clearly what is definition and what empirical fact, e.g., in the concept of force, of inertia, etc. I am already convinced that great simplifications are possible here. For example, as to what a mechanical force is, I have only now clarified this in a manner that satisfies me. But I have neither written these things down, nor do I know

whether they would later satisfy others, too" (# 3206 in the collection of the Deutsches Museum in Munich, cf. also pp. 160 and 169 in this volume).

[12] (Cambridge: Cambridge University Press, 1879), vol. 1, §§207f.: "[W]e cannot, of course, give a definition of matter which will satisfy the metaphysician."

[13] See also Mach 1960, p. 319; Sommerfeld 1952a, p. 60.

[14] For Hertz's own interest in scheme II, see PM 24; for the energetics movement of the 1880's and 1890's, see, e.g., Wilhelm Ostwald, "Studien zur Energetik," *Sitzungsberichte der sächsischen Akademie der Wissenschaften* (Leipzig), **43** (1891), pp. 271–288, and **44** (1892), pp. 211–237; Georg Helm, *Die Lehre von der Energetik, historisch-kritisch dargestellt* (Leipzig: Felix, 1887); Erwin Hiebert, "The Energetics Controversy and the New Thermodynamics," in Duane Roller (ed.), *Perspectives in the History of Science and Technology* (Norman: University of Oklahoma Press, 1971), pp. 67–86.

[15] "Hence, the idea of force as such cannot in this system involve any logical difficulties: nor can it come in question in estimating the correctness of the system; it can only increase or diminish its appropriateness " (PM 16).

[16] See, e.g., PM 17f.; for the debates pitting Ostwald and Helm versus Boltzmann and Planck see Ludwig Boltzmann, "Ein Wort der Mathematik an die Energetik," *Annalen der Physik* **(3. Folge) 57** (1895), pp. 39–71, and "Zur Energetik," *Annalen der Physik* **(3. Folge) 58** (1896), pp. 595–598; George Francis FitzGerald, "Ostwald's Energetics," in his *Scientific Writings*, (London: Longmans Green, 1902), pp. 387–391; Gustav Helm, "Über den gegenwärtigen Zustand der Energetik," *Annalen der Physik* **(3. Folge) 55** (1895), pp. iii–xviii, and "Zur Energetik," *Annalen der Physik und Chemie* **(3. Folge) 7** (1896), pp. 646–659; Hiebert *op.cit.* (note 14); Jungnickel & McCormmach 1986, pp. 217ff.

[17] PM 24: "avoid the harshness and ruggedness [*die Härten und Rauhigkeiten*] which were so disagreeable in the first image." See, however, Mach 1960, p. 319: "Neither is the case with energetic mechanics so bad as Hertz would have it."

[18] See again the quote in note 11 above.

[19] See Mach 1960, pp. 320f. and Kirchhoff *op.cit.* (note 10) who kept "force" but eliminated "mass" as a fundamental notion.

[20] See, e.g., Paulus 1916, p. 849 for a specific example of this reinterpretation, and *ibid.*, pp. 859ff. for its limitation to the first order approximation; cf. also Voss 1901-08, for an idea similar to one already discussed in Joseph John Thomson's *Applications of Dynamics to Physics and Chemistry* (London: Macmillan, 1888).

[21] Cf., e.g., Helmholtz in PM xxxvi, also PM 26, Brill 1900b, pp. 202f. about Kelvin's vortex atoms and Maxwell's rotatory aether models as possible illustrations of these cyclic systems whose interposition might help in accounting for the usual force laws. See also Voss 1901-08, §26 for William Thomson's and J.J. Thomson's kinetic theories as predecessors of Hertz's mechanics.

[22] For some further examples, see, for instance, Brill 1900b, Boltzmann 1900b, Paulus 1916; cf. also Boltzmann 1974, p. 111, Reden 1988, pp. 88ff.

[23] Compare the quote from Heisenberg on p. 210 below.

[24] Cf., e.g., McCormmach 1972, p. 348, Reden 1988, p. 81.

[25] This brief account of the main idea of Hertz's third scheme of mechanics is adapted from Sommerfeld 1952a, pp. 212ff., cf. p. 213 for the interpretation of K as curvature of a N-dimensional Euclidean representation space of the mechanical system, and Unsöld 1970 for the further geometrization of mechanics, electrodynamics, and gravitation in Einstein's general theory of relativity, which also dispensed with forces. See also the contribution by Jesper Lützen to this volume.

[26] PM 27 and §309: "Every natural motion of an independent material system consists herein, that the system follows with uniform velocity one of its straightest paths." Cf. Hertz's entry in his diary, May 8, 1892 (MLD 323), Mach 1960, pp. 320f., Voss 1901–08, pp. 159–163.

[27] Actually, for holonomous systems, this "straightest path" is identical with a geodetic path, i.e., a path for which $\delta \int ds = 0$ compared with all infinitesimally adjacent paths. Cf. Unsöld 1970, pp. 340ff. for the reappearance of geodetic paths in Einstein's general theory of relativity.

[28] This reference to Gauss's *Prinzip vom kleinstem Zwang* actually highlights the parallels between Hertz's version of mechanics of n masses with $3n$ coordinates and higher-dimensional Euclidean geometry. Hertz clearly perceived them but did not emphasize them further; rather he wanted to stress the immediate connection of his mechanics with experi-

ence, see PM 30f. Cf. also Mach 1960, p. 323, Sommerfeld 1952a, pp. 212–214, and further references given by Jesper Lützen in this volume on the geometrization of mechanics.

29 PM 28: "a mathematical aid whose properties are entirely in our power. It cannot, therefore, in itself have anything mysterious to us." Cf. d'Agostino 1990, pp. 385f. and Darrigol 1993a, pp. 243ff. as well as the contribution by d'Agostino in this volume, for Hertz's theory ideal and its connection to his interpretation of Maxwell's electrodynamics.

30 For the holistic aspect of Hertz's view of theories, see, e.g., d'Agostino 1990, p. 391.

31 For references and a detailed account of the interaction of theory and experiment in this episode, which includes a successful prediction of the polarizations of the splitting components in the normal Zeeman effect, see Pieter Zeeman, *Magnetooptische Untersuchungen mit besonderer Berücksichtigung der magnetischen Zerlegung der Spektrallinien* (Leipzig: Barth, 1914); Theodore Arabatzis, "The Discovery of the Zeeman Effect: A Case Study of the Interaction of Theory and Experiment," *Studies in History and Philosophy of Science* **23** (1992), pp. 365–388.

32 Friedrich Paschen and Ernst Back, "Liniengruppen magnetisch vervollständigt," *Physica* **1** (1921), pp. 261–273; for a fuller account of the history of experiments and theories of the Zeeman effect and associated phenomena, see Ernst Back and Alfred Landé, *Zeemaneffekte und Multiplettstruktur der Spektrallinien* (Berlin: Springer, 1925); Arnold Sommerfeld, *Atombau und Spektrallinien* (Braunschweig: Vieweg, 1919; fourth edition 1924); or Zeeman, *op.cit.* (note 31).

33 This quantum number was renamed orbital quantum number $l = k - 1$ in Heisenberg *op.cit.* (note 1) who removed any connection with a mechanical interpretation; cf. also MacKinnon *op.cit.* (note 2), p. 160.

34 Cf., however, Heisenberg in his interview with T.S. Kuhn, February 11th, 1963, AHQP, roll 1419/2 (=DMM, no. 269), transcript p. 20 on Sommerfeld's reluctance to interpret the inner quantum number as the total angular momentum, and *ibid.*, July 2, 1963, transcript pp. 14f. on his general reluctance to interpret formulas in terms of a model: "So he would just say, 'Well, never mind; we will understand it later on, but isn't it nice that we have these simple laws?' This was an attitude that he had quite frequently."

35 Space does not permit a more detailed description of these attempts predating Heisenberg's 1925 paper; see, however, Back and Landé, *op.cit.* (note 32); Paul Forman, *op.cit.* (note 2) and "The Doublet Riddle and Atomic Physics circa 1924," *Isis* **59** (1968), pp. 156–174; MacKinnon, *op.cit.* (note 2); Serwer, *op.cit.* (note 2); Jammer, *op.cit.* (note 3), chapter 3; Mehra and Rechenberg, *op.cit.* (note 1); Darrigol, *op.cit.* (note 3), chapters 7 and 8.

36 Arnold Sommerfeld, "Ein Zahlenmysterium in der Theorie des Zeemaneffektes," *Naturwissenschaften* **8** (1920), pp. 61–64; cf. also Mehra and Rechenberg, *op.cit.* (note 1), section I.6.

37 Arnold Sommerfeld and Ernst Back, "25 Jahre Zeemaneffekt," *Zeitschrift für Physik* **9** (1921), pp. 911–916, cf. p. 913.

38 Werner Heisenberg in an interview with T.S. Kuhn, AHQP, roll 1419/2 (= DMM, no. 269), transcript of session November 30, 1962, p. 6; see also Sommerfeld's pencilled remarks to Heisenberg's letter to him, dated December 4, 1922, DMM, call number: 1977–28, A–136, p.7: "imprecise half numbers [*ungenaue halbe Zahlen*]" and "cheating [*Mogelei*]"; cf. furthermore Sommerfeld's letter to Toshio Takamine, September 30, 1923 (AHQP: 34,6) about his hope to be able to discriminate between half-integral and integral quantum numbers in measurements of the Stark effect: "I am afraid that the problem I suggested to you is not very fruitful, after all. Precision of measurement would have to be extraordinary in order to permit, aside from the Stark effect, a distinction between whole and half quanta."

39 In 1922, in the *Nachtrag* of the third edition of his *op.cit.* (note 32), Sommerfeld tried to circumvent half-integral quantum numbers by letting m change by ±2 or 0 instead of ±1 or 0 as prescribed in Rubinowicz's selection rule, that is by an *ad hoc* modification for the selection rule of the magnetic quantum number m: cf., the interview of Kuhn with Heisenberg, February 11, 1963, AHQP, roll 1419/2, transcript pp. 18f: "Sommerfeld liked integral numbers, as I told you. He liked precise mathematical laws and integral numbers. So the half quantum was for him just something awful. He disliked that intensely, but, of course, he saw on the other hand that by means of this half number, one could get order into the anomalous Zeeman effect. [...] But I remember that for a long time, even in the Sommerfeld institute, the attitude of the others was, Well, this half quantum number of

Heisenberg is quite an interesting, but certainly wrong idea. For such a young man it's very nice that he thought about it. There may be something in it. But certainly that can't be true because if you start with half integral numbers where do you end?'"

40 See, e.g., the interview of T.S. Kuhn with Heisenberg, February 7, 1963, transcript in AHQP, roll 1419/2, p. 13 and February 11, 1963, p. 19 for further support from zero point energy; cf., e.g., Forman *op.cit.* (note 2), p. 186 for the contrast between the earlier "*a priori*" principle approach and the later "*a posteriori*" and somewhat *ad hoc* model-building. See also Mehra and Rechenberg *op.cit.* (note 1), pp. 30f.

41 See Werner Heisenberg, "Zur Quantentheorie der Linienstruktur und der anomalen Zeemaneffekte," *Zeitschrift für Physik* **8** (1922), pp. 273–297, pp. 276ff. [reprinted in *Gesammelte Werke, op.cit.* (note 1), pp. 134–158]; Heisenberg's letters to Sommerfeld in 1922, DMM, Sommerfeld papers, call number: 1977–28, A–136; Heisenberg's letter to Pauli, Dec. 17, 1921, published in Karl von Meyenn (ed.), *Wolfgang Pauli: Das Gewissen der Physik* (Braunschweig: Vieweg, 1988), pp. 48f.; Heisenberg's interview with T.S. Kuhn, AHQP, roll 1419/2, DMM 269, February 11, 1963, transcript pp. 9ff., and Kuhn's questionnaire, *ibid.*, p. 4. See also Jammer, *op.cit.* (note 3), pp. 128f.; MacKinnon, *op.cit.* (note 2), pp. 141f.; Serwer, *op.cit.* (note 2), pp. 202f., 208ff.; Mehra and Rechenberg, *op.cit.* (note 1), pp. 37–39; Cassidy, *op.cit.* (note 2), pp. 121ff.; Darrigol, *op.cit.* (note 3), pp. 184ff., and p. 208 below for a more general description of Heisenberg's approach towards model building.

42 From now on, all angular momentum values are given in units of $h/2$.

43 See Heisenberg, *op.cit.* (note 41), pp. 286ff.: Prediction: $\Delta\nu = 0.32\ \text{cm}^{-1}$, Kent's measurement: $\Delta\nu = 0.34\ \text{cm}^{-1}$.

44 Cf., for example, Werner Heisenberg, "Die Entwicklung der Quantentheorie 1918–1928," *Naturwissenschaften* **17** (1929), pp. 490–496, p. 491: "formal synopsis of the experimental findings, [...] the model-like interpretation of which was still unknown." Cf. also Back and Landé, *op.cit.* (note 32), p. v; also Werner Heisenberg's 1968 talk "Erinnerungen an die Entwicklung der Atomphysik in den letzten 50 Jahren," in his *Gesammelte Werke, op.cit.* (note 1), vol. C IV, pp. 22–36, especially p. 23.

45 See Alfred Landé, "Termstruktur und Zeemaneffekt des Multipletts," *Zeitschrift für Physik* **15** (1923), pp. 189–205; and Wolfgang Pauli, "Über die Gesetzmäßigkeit des anomalen Zeemaneffektes," *Zeitschrift für Physik* **16** (1923), pp. 155–164 (Pauli's terminology is given in the following formula). For more on Landé's and Pauli's work on the anomalous Zeeman effect (which is better known today by the term "vector model') see, e.g., Back and Landé, *op.cit.* (note 32), §5–9; Jammer, *op.cit.* (note 3), pp. 128ff.; Forman, *op.cit.* (note 2); MacKinnon, *op.cit.* (note 2), pp. 141f., 157f.; Serwer, *op.cit.* (note 2), pp. 206f., 214f.; Hund in Heisenberg's *Gesammelte Werke, op.cit.* (note 1), pp. 131f.; Mehra and Rechenberg, *op.cit.* (note 1), pp. 31ff.; Hund, *op.cit.* (note 3), pp. 116ff. Cf. also the right half of figure 3 on page 199 below.

46 See Serwer, *op.cit.* (note 2), p. 190 for translation difficulties presented by this term "*unmechanischer Zwang*," also pp. 232ff., 248ff. for the history of its introduction.

47 Wolfgang Pauli, "Über den Einfluß der Geschwindigkeitsabhängigkeit der Elektronenmasse auf den Zeemaneffekt," *Zeitschrift für Physik* **31** (1925), pp. 371–385.

48 Wolfgang Pauli, "Über den Zusammenhang des Abschlusses der Elektronengruppen im Atom mit der Komplexstruktur der Spektren," *Zeitschrift für Physik* **31** (1925), pp. 765–783, p. 765: "a peculiar, classically indescribable kind of two-valuedness [*Zweideutigkeit*] of the quantum-theoretical properties of the valence electron"; although this amounted to another degree of freedom for the electron, the implications of this postulate were not realized until several months later. For the history and impact of the exclusion principle see, e.g., Jammer, *op.cit.* (note 3), pp. 133ff.; MacKinnon, *op.cit.* (note 2), p. 158; Serwer, *op.cit.* (note 2), pp. 233ff.; Hund, *op.cit.* (note 3), pp. 120ff.; Pauli in Meyenn (ed.), *op.cit.* (note 41), pp. 201ff.; Darrigol, *op.cit.* (note 3), pp. 201ff. The exclusion principle remained valid in the new quantum mechanics which reinterpreted the fourth quantum number as spin, a kind of internal angular momentum of the electron.

49 Heisenberg, *op.cit.* (note 41), and Pauli, *op.cit.* (note 48). See Heisenberg, *op.cit.* (note 1), p. 842, especially notes 1 and 2; also Serwer, *op.cit.* (note 2), p. 242, footnote 164.

50 Heisenberg *op.cit.* (note 1), p. 842: "I. Das Elektron wirkt auf den Atomrest durch einen unmechanischen Zwang derart, daß sich der stationäre Zustand des Elektrons scheinbar verdoppelt. II. Der Atomrest wirkt auf das Elektron durch einen unmechanischen Zwang derart, daß sich der stationäre Zustand des Elektrons scheinbar verdoppelt." Note that Heisenberg had already discussed alternative assumptions in earlier papers, see, e.g., Heisenberg *op. cit.* (note 41), p. 288 or in his letter to Landé, February 2, 1925, AHQP 6,2 =

DMM, microfilm no. 123, pp. 2–3 here quoted in note 89 below. However, Heisenberg did *not* use the Hertzian terminology in these two earlier quotes, which suggests that he became aware of the structural similarity with Hertz's mechanics between February and April 1925.

51 Cf. here note 7 above for Heisenberg's rare usage of the term in other papers; cf., e.g., Voss, 1901-08, p. 14f. for occurrences of the term "*Bild*" immediately coupled to Hertz's mechanics as its source. Incidentally, the same personification can be found with Kirchhoff's dictum of the "simplest description possible," and with Mach's terminus "economy of thinking" [*Denkökonomie*].

52 Finn Aaserud, director of the Niels Bohr Archive at Copenhagen was so kind as to check this for me; Hertz's mechanics is listed neither in the catalogues of the institute library nor in the collection of Bohr's personal books.

53 See, e.g., Heisenberg's interview with T.S. Kuhn, AHQP, roll 1419/2 (= DMM, no. 269), February 7, 1963, transcript p. 8. According to the *Vorlesungsverzeichnisse* of the *Ludwig-Maximilians-Universität-München*, Sommerfeld lectured on mechanics in the winter terms of 1920/21, lasting from October 21 to March 15, with lectures between 9 and 10 a.m. each Monday, Tuesday, Thursday and Friday, and then again in the winter term of 1923/24; the course also included major lectures on hydrodynamics, Maxwell's theory and electron theory, optics, thermodynamics, and partial differential equations in the subsequent terms. See also Hermann 1988, pp. 3f. for Sommerfeld's account of how deeply he in turn was impressed with Hertz's electrodynamics.

54 See Sommerfeld 1952a, pp. 212–214. Since Sommerfeld's lectures were only published in 1943, it is likely that the Hertz strand in Sommerfeld's lectures at that time was even stronger, since Sommerfeld had already studied Hertz's textbook soon after its appearance. Unfortunately, Heisenberg either did not make his own lecture notes or they were not preserved in the Heisenberg papers (personal communication from Dr. H. Rechenberg, Munich). See, however, Heisenberg's review of the Sommerfeld lectures on mechanics in *Naturwissenschaften* **31** (1943), pp. 350f. [reprinted in his *Gesammelte Werke*, *op.cit.* (note 1), vol. C IV, pp. 254f.], p. 350: "The lectures about theoretical physics which over the course of the years Sommerfeld repeatedly held in Munich, and which served as the foundation of the schooling Sommerfeld provided his numerous students, including this reviewer ... [*Grundlage ... für die Schule, in der Sommerfeld seine zahlreichen Schüler ... erzogen hat*]."

55 See, e.g., the transcripts of the Heisenberg interviews by T.S. Kuhn, AHQP, roll 1419/2 (=DMM, no. 269), November 30, 1962, pp. 6f.: "I had first to learn classical mechanics"; February 7, 1963, pp. 1f. on Sommerfeld's "very rigorous way of educating the students," pp. 4f. on the textbooks read by Heisenberg, and February 11, 1963, p. 9.

56 See Müller and Prange, *op.cit.* (note 10), pp. 140 on holonomous and non-holonomous constraints, and pp. 159ff. on Hertz's principle of straightest path.

57 See Kuhn's interview with Heisenberg, AHQP, roll 1419/2, (DMM, no. 269), November 30, 1962, transcript p. 7.

58 See Voss 1901–08, §§1, 3, 12, 23, 28, 37, 39 and 42, here especially §§26–28.

59 Heisenberg, *op.cit.* (note 1), p. 843. It might be helpful in this regard to quote from Heisenberg's interview with T.S. Kuhn, February 11, 1963 (AHQP, 1419/2, DMM microfilm no. 269), transcript pp. 16f., illustrating the kind of connotations the term "picture" [*Bild*] had for the late Heisenberg: "What quite frequently happens in physics is that, from seeing some part of the experimental situation, you get a feeling of how the general experimental situation is. That is, you get some kind of picture. Well, there should be quotation marks around the word 'picture.' This 'picture' allows you to guess how other experiments might come out, and, of course, then you try to give to this picture some definite form in words or in mathematical formula. Then what frequently happens later on is that the mathematical formulation of the 'picture' or the formulation of the 'picture' in words, turns out to be rather wrong. Still, the experimental guesses are rather right. That is, the actual 'picture' that you had in mind was much better than the rationalization that you tried to put down in the publication. [...]. The picture changes over and over again and it's so nice to see how such pictures change."

60 The existence of this fine-structure of the spectral lines predicted by Sommerfeld's and Planck's relativistic version of the Bohr-Sommerfeld atomic theory was confirmed by Paschen and others. However, for the intricacies of this experimental "confirmation," see Helge Kragh, "The Fine Structure of Hydrogen and the Gross Structure of the Physics Community," *Historical Studies in the Physical Sciences* **15** (1984/85), pp. 67–125.

61 See Heisenberg, *op.cit.* (note 1), p. 846; cf. also figure 3.

62 *Ibid*, p. 845; cf. Werner Heisenberg, "Über eine Abänderung der formalen Regeln der Quantentheorie beim Problem der anomalen Zeemaneffekte," *Zeitschrift für Physik* **26**

(1924), pp. 841–860 [reprinted in his *Gesammelte Werke*, *op.cit.* (note 1), pp. 289–305, pp. 292, 299ff].

63 Heisenberg, *op.cit.* (note 1), pp. 847f.; cf. Alfred Landé and Werner Heisenberg, "Termstruktur des Multipletts höherer Stufe," *Zeitschrift für Physik* **25** (1924), pp. 279–286.

64 This remarkable duality of the two schemes foreshadowed Bohr's later principle of complementarity, as will be discussed further in section 8; cf. also Niels Bohr, "The Quantum Postulate and the Recent Development of Atomic Theory," *Nature*, April 25th, 1928, Supplement pp. 580–590; Mehra and Rechenberg, *op.cit.* (note 1), p. 206; Hund, *op.cit.* (note 3), p. 123; and Hund in Heisenberg's *Gesammelte Werke*, *op.cit.* (note 1), p. 132.

65 Heisenberg *op.cit.* (note 1), p. 849f. Alkaline-earth atoms had just been discussed by Henri Norris Russell and F.A. Saunders, "New Regularities in the Spectra of Alkaline Earths," *Astrophysical Journal* **61** (1925), pp. 38–61. This article also independently introduced a model description equivalent to Heisenberg's scheme III.

66 See Heisenberg, *op.cit.* (note 1), pp. 850ff.; cf. also Ralph de Laer Kronig, "Über die Intensität der Mehrfachlinien und ihrer Zeemankomponenten," *Zeitschrift für Physik* **31** (1925), pp. 885–897.

67 E.g., George Uhlenbeck and Samuel Goudsmit, "Ersetzung der Hypothese vom unmechanischen Zwang durch eine Forderung bezüglich des inneren Verhaltens jedes einzelnen Elektrons," *Naturwissenschaften* **13** (1925), pp. 953f. For the history of spin see also Samuel Goudsmit, "Die Entdeckung des Elektronenspins," *Physikalische Blätter* **21**, pp. 445–453, and "It might as well be spin," *Physics Today*, June 1976, pp. 40–43; George Uhlenbeck, "Personal Reminiscences," *Physics Today*, June 1976, pp. 43–48; MacKinnon, *op.cit.* (note 2), p. 158. However, see also Heisenberg's interview with T.S.Kuhn, February 15, 1963, AHQP, roll 1419/2 (= DMM no. 269), transcript p. 30 for the reasons for the considerable resistance to any classical pictures for electron spin: "As you say, the fact that one was already so far away from classical mechanics made it much easier to throw away, to brush away, a classical picture because, after all, classical physics is not correct anyway, so therefore why bother about such a rather queer and odd picture as a spinning electron."

68 Max Born and Pascual Jordan, "Zur Quantenmechanik," *Zeitschrift für Physik* **34** (1925), pp. 858–888; Max Born, Werner Heisenberg, and Pascual Jordan, "Zur Quantenmechanik II," *Zeitschrift für Physik* **35** (1925), pp. 557–615.; see also the previous note. Cf. also, e.g., Jammer, *op.cit.* (note 3), chapters 3.4 and 4; Hund, *op.cit.* (note 3), chapters 10ff.; Mehra and Rechenberg, *op.cit.* (note 1), chapter 5, also volumes 3 and 4; Darrigol, *op.cit.* (note 3), pp. 201ff. and chapter 12.

69 According to Helmut Pulte's "C.G.J. Jacobis Vermächtnis einer 'konventionalen' analytischen Mechanik," *Annals of Science* **51** (1994), pp. 497–516, a similar conventionalistic move can already be found in Jacobi's lectures on mechanics; cf. also Gregor Schiemann's contribution to this volume.

70 Heisenberg, *op.cit.* (note 1), p. 842 [the English translation of the second sentence was provided by Mehra and Rechenberg, *op.cit.* (note 1), p. 202]; for the part omitted from this quote, see section 8 below.

71 "[...] with a suitable arrangement of the coordinates for both systems the equations of condition exist" (PM §418).

72 Cf., e.g., Pauli's letter to Kronig, Oct. 9, 1925: "beim heutigen allgemeinen Wirrwarr in der Atomphysik ..." In the same letter, Pauli describes his search for one single "substitute model" [*Ersatzmodell*], that could replace all three schemes presented in Heisenberg's 1925 paper, *op.cit.* (note 1).

73 Heisenberg *op.cit.* (note 1), p. 851; cf. also p. 845.

74 See, e.g., Heisenberg's interview with T.S. Kuhn, AHQP, roll no. 1419/2 (= DMM, no. 269), February 15, 1963, transcript pp. 18f., or Werner Heisenberg, *Physics and Beyond* (New York: Harper, 1971) [translation of *Der Teil und das Ganze*], chapter 5.

75 Pauli letter to Bohr, Dec. 12, 1924, in Meyenn (ed.), *op.cit.* (note 41), p. 189: "The (as of yet unattained) goal must be to deduce these and all other physically real, observable properties of stationary states from (whole) quantum numbers and quantum-theoretical laws." Cf. also Serwer, *op.cit.* (note 2), pp. 243ff.; Mehra and Rechenberg, *op.cit.* (note 1), p. 196.

76 Pauli letter to Sommerfeld, Dec 6, 1924, in Meyenn (ed.), *op.cit.* (note 41), p. 182; cf., e.g., Serwer, *op.cit.* (note 2), p. 223.

77 See especially MacKinnon, *op.cit.* (note 2) for Heisenberg's changing attitude towards models and its interdependency upon his theoretical studies at that time.

[78] Heisenberg letter to Pauli, Oct. 9, 1923, in Meyenn (ed.) *op.cit.* (note 4), p. 125: "Die Modellvorstellungen haben prinzipiell nur einen symbolischen Sinn, sie sind das klassische Analogon zur 'diskreten' Quantentheorie." [English translation in Mehra and Rechenberg, *op.cit.* (note 1), p. 113.] Heisenberg does not yet use the term *Bild* here. For Bohr's related emphasis on the "renunciation as to visualisation in the ordinary sense" and the view of the new quantum mechanics as a "symbolic transcription" of classical mechanics, see Bohr *op.cit.* (note 64), pp. 580, 586, 590. For the changing emphasis on visualizability [*Anschaulichkeit*] which was lost and regained in the history of quantum theory, see esp. Arthur Ian Miller, *Imagery in Scientific Thought* (Cambridge: MIT Press, 1986), chapter 4, esp. p. 128.

[79] Heisenberg letter to Pauli, Dec. 15, 1924, in Meyenn (ed.) *op.cit.* (note 4), p. 192f.: "Sie [haben] den Schwindel auf eine bisher ungeahnte, schwindelhafte Höhe [getrieben] und damit alle bisherigen Rekorde deren Sie mich beschimpft, spielend geschlagen [...] (indem Sie einzelne Elektronen mit 4 Freiheitsgraden einführen) [...]. Und wenn Sie selbst meinen, etwas gegen die bisherigen Arten von Schwindel geschrieben zu haben, so ist das natürlich Mißverständnis, denn Schwindel × Schwindel gibt nichts richtiges und daher können sich zwei Schwindel nie widersprechen" (original emphasis omitted). Cf. also Heisenberg's interview with T.S. Kuhn, February 11, 1963, AHQP, roll no. 1419/2 (DMM, no. 269), transcript p. 21, and Mehra and Rechenberg, *op.cit.* (note 1), p. 198.

[80] See Heisenberg to Landé, January 4, 1925, quoted in Mehra and Rechenberg, *op.cit.* (note 1), p. 199. For a general comparison of Pauli's and Heisenberg's approaches see MacKinnon, *op.cit.* (note 2), p. 159; and Serwer, *op.cit.* (note 2), pp. 195f.

[81] "We shall denote as incorrect any permissible pictures if their essential relations contradict the relations of external things" (PM 2). For these three criteria see Mach 1960, p. 318; Schaffner 1970, pp. 318ff.; d'Agostino 1990, pp. 384f. and Alfred Nordmann's contribution in this volume; contrary to him, I do not read Hertz as saying that his first scheme actually contradicts the first two criteria.

[82] Cf. PM 40f. and Nordmann in this volume for Hertz's comparative evaluation of the three *Bilder* of mechanics.

[83] Mach 1960, p. 318. Cf. Schaffner 1970, p. 326. Conspicuously absent from Hertz's evaluation scheme are pragmatic criteria.

[84] Heisenberg, *op.cit.* (note 1), p. 860: "Obwohl die quantentheoretischen Gesetze der Wechselwirkung der Elektronen im Atom sich zweifellos durch große Einfachheit auszeichnen, scheint es zurzeit jedoch keinen anderen Weg zur Deutung dieser Gesetze zu geben, als den über modellmäßige Bilder symbolischer Bedeutung, bei denen diese Einfachheit kaum genügend zum Ausdruck kommt." [English translation from Mehra and Rechenberg, *op.cit.* (note 1), p. 205.] Cf., e.g., PM 24.

[85] Cf., e.g., Sommerfeld 1952a, pp. 5, 60, 212–214.

[86] See, however, Heisenberg's account from 1929, *op.cit.* (note 44), section IV; *op.cit.* (note 74); and his *Gesammelte Werke*, *op.cit.* (note 1), pp. 478ff.; cf., e.g., Jammer, *op.cit.* (note 3), chapter 7; Catherine Chevalley, "Physical Reality and Closed Theories in Werner Heisenberg's Early Papers," in D. Batens and J.P. van Bendegem (eds.), *Theory and Experiment* (Dordrecht: Reidel, 1988), pp. 159–167; and Darrigol, *op.cit.* (note 3), chapter 13.

[87] For Heisenberg's relation to Bohr, see, e.g., Kuhn's interview with Heisenberg, AHQP, roll 1419/2 (DMM, no. 269), November 30, 1962, transcript pp. 12–14; cf. Mehra and Rechenberg, *op.cit.* (note 1), sections III.1–3; Cassidy, *op.cit.* (note 2), pp. 83, 171ff, 244ff.

[88] For the Como lecture see Bohr, *op.cit.* (note 64); also see, e.g., Bohr to Pauli, Dec. 22, 1924, for an early indication of the concept that was slowly being formed: "Altogether, both kinds of insanity [the Bohr-Sommerfeld theory and Pauli's new theory of 1924] may be connected too closely to the truth so that one cannot criticise them as isolated aspects" [in Meyenn (ed.), *op.cit.* (note 41), pp. 103ff.]. See also Heisenberg's 1968-talk, *op.cit.* (note 44), p. 27: "At the time Bohr was already thinking of his complementarity."

[89] Heisenberg, *op.cit.* (note 1), p. 842: "Vielmehr werden sich die beiden Bilder eben wegen jener Eindeutigkeit in ihren Aussagen so ergänzen müssen, daß die Größen, die in dem einen Schema unbestimmt bleiben, im anderen bestimmt werden und umgekehrt; die beiden Schemata zusammen werden sozusagen ein konvergentes Rechenverfahren zur Bestimmung der Eigenschaft der stationären Zustände des Atoms bilden." [English translation in Mehra and Rechenberg, *op.cit.* (note 1), p. 202.] Cf. also Heisenberg to Landé, February 2, 1925, AHQP 6,2 = DMM, microfilm no. 123, pp. 2–3: "What's essential, it

seems to me, is that such apparently so fundamentally different schemes as Pauli's and the J-scheme, are only *two* sides of the same thing, where we will probably be forced to always maintain this duality [*Zweiheit*] of the *visualizable* conception [*anschaulichen Vorstellung*]" (emphasis in the original).

90 See Heisenberg's account from 1929, *op.cit.* (note 44), p. 494: "The quantum-theory of the wave-*Bild* thus stands after these investigations *as totally equally justified* [*völlig gleichberechtigt*] next to the quantum theory of the particle-*Bild*" (emphasis in the original). According to Hund, *op.cit.* (note 3), p. 123, the indications of the later principle of complementarity before 1927 were more confusing than enlightening.

91 See, e.g, Heisenberg's 1929-account, *op.cit.* (note 44), section 1; Jammer, *op.cit.* (note 3), sections 3.2; Hund, *op.cit.* (note 3), chapters 6f.; Serwer, *op.cit.* (note 2), p. 192.

92 Heisenberg to Sommerfeld, November 18, 1924, DMM, call number 1977–28, A–136: "Im übrigen glaube ich aber immer mehr, daß die Frage 'Lichtquanten oder Korrespondenzprinzip' eine Wortfrage ist. Alle Effekte in der Quantentheorie müssen ja ihr Analogon in der klassischen Theorie haben, denn die klassische Theorie ist doch fast richtig; also haben alle Effekte immer zwei Namen: einen klassischen und einen quantentheoretischen u. welchen man vorzieht, ist eine Art Geschmackssache."

93 See Hertz to Emil Cohn, February 25, 1891, from DMM, manuscript number 3198–3207: "You write that certain concepts are now less clear to you than initially. I can't say that for myself. While I cannot translate into the new language every question that is posed to me by someone or by myself in the language of the old theory, and while I thereby find myself for some time at a loss [*in Verlegenheit gerathe*], I have not found contradictions within the theory or with the facts."

94 See, e.g., Sommerfeld, *op.cit.* (note 36); Sommerfeld and Back, *op.cit.* (note 37); Alfred Landé, "Über den anomalen Zeemaneffekt," *Zeitschrift für Physik* **5** (1921), pp. 231–241, **7** (1921), pp. 398–405; Landé, *op.cit.* (note 45); also, e.g., Forman, *op.cit.* (note 2); Serwer, *op.cit.* (note 2); Darrigol, *op.cit.* (note 3), chapters 7 and 8.

95 See, e.g., Back's contribution to Back and Landé, *op.cit.* (note 32) for an overview of the experimental techniques used in the measurement of the Zeeman effects.

96 Apparently, Heisenberg also did not mention Hertz in later texts, either in his lectures on magneto- and electrooptics in the summer term of 1925 ('Sommersemester 1925,' kindly checked for me by Dr. H. Rechenberg in the Heisenberg Archive, Munich), or in any of his later reflections on broader philosophical issues.

97 See note 2 above and the references given there, in particular pp. 2–4 of the transcript of T.S. Kuhn's interview with Heisenberg, November 30, 1962 in AHQP, roll 1419/2 (= DMM, no. 269), on the importance of Plato, Kant and (to a much lesser extent) Mach for the young Heisenberg. Incidentally: Heisenberg's early reading of Plato might have supported his acceptance of Hertz's *Bild*-conception.

98 See, e.g., Kaiser 1981 and Buchwald 1985 for excellent overviews of the plethora of theories in electrodynamics in the late 19th century. See also Darrigol 1993a and d'Agostino 1990, p. 381, as well as Dugas 1988, chapter X for the plurality of theories in mechanics.

99 See, e.g., Heisenberg's letter to Sommerfeld, January 4, 1923 "On the He-Question": "[...] that I can't comprehend how Bohr and P.[auli] can insist so vehemently on such an uncontrollable and unfruitful literal application of general quantum-principles. And as thoroughly as I am convinced that P.'s [Pauli's] views are incorrect, I find equally inappropriate Bohr's self-assured consistency [*so unschicklich ist mir die sichere Konsequenz*] by which he finds correct everything that comes out wrong and incorrect everything that comes out right. This state of physics is really uncongenial to me. I would therefore like to ask you to find an American or Japanese (Takamine?) who will as quickly and reliably as possible measure the Starkeffekt in the I. order of He and Li."

100 See, e.g,. Heisenberg , *op.cit.* (note 44), section II; Forman, *op.cit.* (note 35) for the "doublet riddle" arising early in 1924; Serwer, *op.cit.* (note 2), section 1 for the "riddle of statistical weights" which was identified already in 1923.

101 Heisenberg, *op.cit.* (note 62), p. 291; see, e.g., the two introductory sentences of Heisenberg's *op.cit.* (note 1); cf. also Mehra and Rechenberg, *op.cit.* (note 1), chapter II: "Towards the Recognition of the Crisis."

102 See MacKinnon, *op.cit.* (note 2), p. 144; similarly Unsöld 1970, p. 342 interprets sections 4 and 5 of book 2 of Hertz's mechanics as a late expression of his ideal of a mechanical model of the aether which was soon given up completely.

[103] Mehra and Rechenberg, *op.cit.* (note 1), pp. 37, 39. See also Heisenberg's interview with T.S. Kuhn in AHQP, roll 1419/2 (= DMM, no. 269), February 11, 1963, transcript p. 12 for Heisenberg's own account of the difference between him and Sommerfeld: "I always liked Bohr's correspondence principle just because it gave that kind of lack of rigidity, that flexibility in the picture, which could lead to real mathematical schemes. Well, Sommerfeld disliked any non-rigidity." Cf. Cassidy, *op.cit.* (note 2), p. 125 for the motto and Heisenberg's attitude towards models at that time. For a later reappearance of this attitude see Gerald Holton, "'Success sanctifies the means': Heisenberg, Oppenheimer, and the Transition to Modern Physics," in Everett Mendelsohn (ed.), *Transformation and Transition in the Sciences* (Cambridge: Cambridge University Press, 1970), pp. 155–173.

[104] See Heisenberg, *op.cit.* (note 41), p. 281; also, e.g., Mehra and Rechenberg, *op.cit.* (note 1), p. 38 and p. 43 for Bohr's reaction in a letter to Sommerfeld, April 30, 1922: "I must confess that several of the assumptions employed by you and your collaborators in the very promising theory of the anomalous Zeeman effect hardly appears to me to be consistent with a unified picture of quantum theory." See also Heisenberg's interview with T.S. Kuhn (AHQP as quoted above), February 11, 1963, transcript pp. 13–15 for Heisenberg's account of the different attitudes.

[105] See section 4.2 above. It was this theoretical opportunism of the model-builder Heisenberg which Pauli found so "unphilosophical," see his letter to Bohr, February 11, 1924, in Meyenn (ed.), *op.cit.* (note 41), p. 143; cf. Serwer, *op.cit.* (note 2), p. 238.

[106] Pauli to Bohr, Dec. 12, 1924, in Meyenn (ed.), *op.cit.* (note 41), p. 188: "ich glaube, daß das, was ich hier mache, kein größerer Unsinn ist als die bisherige Auffassung der Komplexstruktur. Mein Unsinn ist zu dem bisherigen Unsinn konjugiert. Eben deshalb glaube ich, daß dieser Unsinn beim jetztigen Stand des Problems notwendigerweise gemacht werden muß. Der Physiker, dem es einmal gelingen wird, diese beiden Unsinne zu addieren, der wird die Wahrheit erhalten." Cf. Serwer *op.cit.* (note 2), p. 241.

[107] See, e.g., the letters by Heisenberg to Pauli, November 19, 1921, Bohr to Pauli, December 11, and December 22, 1924, Pauli to Bohr, December 12, 1924, all in Meyenn (ed.), *op.cit.* (note 41). See also Kuhn's interview with Heisenberg, AHQP, roll 1419/2 (= DMM, No. 269), November 30, 1962, transcript p. 11 on Sommerfeld's "mystic" enthusiasm for integral numbers.

[108] See Heisenberg to Pauli, December 15, 1924, in Meyenn (ed.) *op.cit.* (note 41), p. 192.

[109] Heisenberg in his interview with T.S. Kuhn, November 30, 1962, AHQP, transcript p. 11.

[110] For instance, Bohr's assumption of closed orbits of electrons around positively charged atomic cores contradicted Larmor's theorem concerning the radiation of all accelerated charges; cf. also incompatible quantum number assignments (see note 100 above).

[111] Cf., e.g., Pauli to Bohr, December 12, 1924 in Meyenn (ed.) *op.cit.* (note 41), p. 188, Heisenberg's letter to Pauli, December 16, quoted here on p.36, or the quote on Heisenberg's introduction of half quantum numbers on p. 192 above.

[112] Not only Heisenberg speculated about possible violations of angular momentum conservation – in 1924, Bohr, Kramers and Slater also considered whether energy conservation might be violated in statistical processes and just conserved in the statistical average.

[113] See, e.g., Niels Bohr, "Seven Lectures on the Theory of Atomic Structure" (Göttingen, 1992) vol. 4 of Niels Bohr's *Collected Works* (Amsterdam: Elsevier, 1977), p. 391: "We must conclude from the occurrence of the anomalous Zeeman effect that the classical theory is inadequate"; or Heisenberg to Landé, undated, around the beginning of October 1922: "Gradually it is becoming the general conviction that one must really give up much of the present mechanics and physics if one wants to arrive in a different manner at the anomalous Zeeman effect," quoted in Mehra and Rechenberg, *op.cit.* (note 1), p. 99.

[114] See, e.g., Sommerfeld and Back, *op.cit.* (note 37), p. 913: "the electrodynamic mechanism of the anomalous Zeeman effects is still hidden from us." Cf. also Heisenberg's interview with T.S. Kuhn, AHQP, roll 1419/2, transcript p. 15: "that looks as if it is correct physics. We don't understand it anyway, but these formulas which we must stick to – they are good."

[115] See, e.g., Pauli to Bohr, December 12, 1924: "The goal (not yet achieved) must be to deduce these [energy and momentum] and all other physically real, observable quantities of the stationary states from the (integral) quantum numbers and quantum-theoretical laws" (cf. section 7 above).

116 See, e.g., Bohr to Pauli, December 22, 1924: "I have a feeling that we stand at a turning point, since now the extent of the entire swindle has been characterized so exhaustively." Cf. Pauli to Sommerfeld, December 6, 1924, and MacKinnon, *op.cit.* (note 2) p. 140.
117 See, e.g., Pauli's letter to Sommerfeld, July 19, 1923 in Meyenn (ed.), *op.cit.* (note 41), p. 106) about the "greatly lamentable state" [*großer Jammer*] of the theory of the anomalous Zeeman effect and of many-electron systems.
118 See, e.g., Heisenberg to Sommerfeld, January 4, 1923: "This state of physics is really uncongenial to me," and Pauli to Kronig, May 21, 1925, in Meyenn (ed.), *op.cit.* (note 41), p. 216: "At the moment physics is once again quite convoluted [*sehr verfahren*], much too difficult for me at any rate, and I wish I were a film-comedian or something like that and had never even heard of physics." Cf. also the quote from this letter on p. 210 below, and T.S. Kuhn's questionnaire for his interview with Heisenberg, AHQP, roll 1419/2 (= DMM, no. 269), transcript p. 11: "How did people react to the gigantic equivocalness of the scientific situation?"
119 Cf. *ibid.*, p. 10: "In 1924 Born felt that the breakthrough in quantum theory was near enough to justify calling his *Atommechanik* Vol. I."
120 See, e.g., Pauli to Bohr, December 12, 1924.
121 Hertz reports having once seriously considered scheme II (energetics) before favoring scheme III (PM 24) and Heisenberg had published papers fitting under his scheme I, namely *op.cit.* (note 4) and *op.cit.* (note 62).
122 Heisenberg, *op.cit.* (note 1), p. 860 in the concluding section "Schlußbemerkungen." A similar tone was adopted by Back and Landé, *op.cit.* (note 32), p. v, noting the "provisional character of the current quantum-theory of the atom."
123 Especially for Heisenberg, this entails the correction of a deeply-rooted myth spread by no one else but Heisenberg himself, who later reconstructed his path to quantum mechanics via an alleged self-imposed restriction of theory to the sole prediction of observables. See, e.g., Heisenberg, *op.cit.* (note 74), chapter 5.
124 According to Bohr, Kramers, and Slater, energy is conserved only in the statistical average, not in individual processes, cf. Niels Bohr, H.A. Kramer, and J.G. Slater, "The Quantum Theory of Radiation," *Philosophical Magazine* **47** (1924), pp. 785–802. This was soon refuted by Walther Bothe and Hans Geiger with the help of coincidence measurements: see, e.g., the commentary in vol. 5 of Bohr's *Collected Works* (Amsterdam: Elsevier, 1984), pp. 75ff.
125 Heisenberg in his interview with T.S. Kuhn, February 19, 1963, AHQP, roll 1419/2 (DMM, no. 269), transcript p. 22. See also Kuhn's reply, *ibid.*: "It's interesting to me that in '24, in both Göttingen and Copenhagen, it is clear that people know the thing's all screwed up. [...]. The physicists are willing to take more drastic steps than they have ever been willing to take before."
126 See, e.g., Heisenberg to Landé, AHQP 6,2 = DMM, microfilm no. 123: "if only one were to succeed to piece together the various formalisms about the Zeemaneffect into a unified description, that would probably be a theory." With reference to Poincaré's similar approach towards problems in the electrodynamics of moving bodies, Arthur Miller, *op.cit.* (note 78) has called this attitude "modificationism" and contrasted it against Einstein's readiness toward a more radical break. Cf. *ibid.*, p. 27 for Hertz's influence on Poincaré.
127 Heisenberg in the interview by T.S. Kuhn, February 19, 1963, AHQP, roll 1419/2 (= DMM, no. 269), transcript p. 20.
128 Pauli to Kronig, May 21, 1925, in Meyenn (ed.) 1979, p. 215: "Mich selbst interessieren die Multipletts und der anomale Zeemaneffekt momentan gar nicht mehr. Wenn nicht jemand eine wirklich physikalische Idee hat, wird man da kaum weiter kommen. Das bloße Ausdehnen der formalen Zoologie auf immer kompliziertere Fälle halte ich im Grund' doch für eine unfruchtbare Sache."
129 In this context, see the evaluation of Hertz's mechanics by Sommerfeld 1952a, pp. 5 and 60: "interesting, but not very fruitful."
130 This is Jungnickel and McCormmach's 1986, p. 212 description of the situation of Hertz's mechanics; cf. also Heidelberger's and Schiemann's contributions to this volume.
131 On the invention of matrix mechanics in June 1925, see, e.g., Heisenberg, *op.cit.* (note 44), section III; the third group of papers in his *Gesammelte Werke*, *op.cit.* (note 1), pp. 329–504; MacKinnon, *op.cit.* (note 2), pp. 163ff.; Mehra and Rechenberg, *op.cit.* (note 1), chapter IV.

[132] See MLD 343: "Now that it is being set, I have very much the feeling of 'God protect the house.' The book could easily reduce my by and large good repute to rack and ruin, 'if the casting fail.' Even a minor fault can flaw the whole. It frightens me to come out with something that I have never talked over with any human being. At least I find comfort in knowing myself to be a member of several academies; where else to obtain confidence in oneself?"

[133] Heisenberg in his interview with T.S. Kuhn, February 11, 1963, AHQP, roll 1419/2, DMM no. 269, transcript p. 16.

ULRICH MAJER

HEINRICH HERTZ'S PICTURE-CONCEPTION OF THEORIES: ITS ELABORATION BY HILBERT, WEYL, AND RAMSEY

INTRODUCTION

Heinrich Hertz became famous for two quite different things: the detection of radio-waves and the invention of the "picture" metaphor for theories. It would seem as if these two things have little or nothing to do with each other. Accordingly, the first topic mainly has been considered by physicists and historians of science; whereas, the second has been left to philosophers of science and, perhaps more appropriately, to philosophers of language. But this separation, this division of labor between history of science and philosophy, is in my eyes a fundamental mistake. Not only does it contradict Hertz's own intention to keep both things, physics and philosophy, closely together, but it gives the wrong impression as if the invention of the picture metaphor had nothing to do with the detection of the radio waves and vice versa. Exactly the contrary is true as Hertz himself points out in the introduction to his first book, *Untersuchungen über die Ausbreitung der elektrischen Kraft*:

> What we here indicate as having been accomplished by the experiments is accomplished independently of the *correctness* of particular theories. The *meaning* [*Bedeutung*] of the experiments is nevertheless to be sought in their *connection* between the experiments with the *theory* [*in ihrem Zusammenhang mit der Theorie*] in connection with which they were really undertaken. (EW 19, emphasis added)[1]

Of course, the theory which Hertz has in mind is the electromagnetic theory of Faraday and Maxwell. "And now," Hertz asks, "to be more precise, what is it that we call the Faraday-Maxwell theory?" (EW 20) In the second part, B, of the Introduction, Hertz makes clear three points beyond any doubt:

(i) The question, "What is Maxwell's theory?" has no *unique* answer unless we *supplement* the mathematical formulation of the theory with a *physical interpretation* or *model* for its descriptive terms. Only in this way by constructing a model, can we fix the *physical content* of Maxwell's theory uniquely.

This means we can split the question, "What is Maxwell's theory?" into two parts and answer each part separately. First we ask: What is the *mathematical* content of Maxwell's theory? Hertz's answer to this question is well known: "Maxwell's theory is the system of Maxwell's equations" (EW 21). But this answer does not determine the *physical* content of Maxwell's theory uniquely! Many *different* interpretations are possible. In order to fix these, we have to construct a physical model for the *propagation of electromagnetic forces* and to combine it with

D. Baird et al. (eds.), Heinrich Hertz: Classical Physicist, Modern Philosopher, 225–242.

Maxwell's equations. If this is done appropriately, we have an unambiguous physical interpretation of the mathematical theory and a unique answer to the question of the *physical* content of Maxwell's theory.

(ii) Several, quite *different* physical models can be constructed for the propagation of electromagnetic forces in space and, hence, for Maxwell's equations. This means several physical interpretations of Maxwell's theory are possible. The question: Which is the most adequate one? is answered by the next point.

(iii) Hertz himself constructed a *new* "physical" model for the propagation of radio waves, which denied the existence of *instant forces* completely. This is the so called "fourth standpoint [*vierte Standpunkt*]" (EW 25–27) in the Introduction. Only in this way was he able to explain the different experiments with the radio waves *consistently*.

What is the upshot of these three points? First, we must notice that the different *physical models* or *Standpunkte* in the first book *On the Propagation of Electromagnetic Forces* later became the *pictures* in the picture metaphor of *The Principles of Mechanics*. Expressed then in this new terminology, the essential point of the three remarks is that the construction of an *alternative* picture for Maxwell's theory was a *presupposition* for the "detection" of the radio waves because only in the "new" picture can electromagnetic waves move "freely in space" as we suppose radio-waves move. This shows that the picture metaphor is by no means a mere philosophical flourish but the backbone of a new, and for the time being, rather revolutionary conception of physical theories – indeed, so revolutionary that today, we still have to learn the lesson.

But, what is the point of the new conception? Why is it so *revolutionary* that we still have to come to grips with its characterization of theories as pictures or to use Weyl's more appropriate term, *symbolic constructions*? In order to answer these questions, I will contrast two different interpretations in which Hertz's new conception of theories as pictures was received by his contemporaries and successors in the twentieth century. What I have in mind is, on the one hand, Wittgenstein's *Tractatus Logico-Philosophicus*, which is the outstanding philosophical reaction to the picture-metaphor, and, on the other hand, the way Hilbert,Weyl, and finally Ramsey, "H-W-R," picked-up and elaborated Hertz's proposal for a new symbolic conception of theories.

The first interpretation is well-known and much debated among philosophers of all persuasions. Whereas, the second is almost completely neglected and not discussed at all – at least not in philosophical circles. Now, one is tempted to ask: What is the reason for this unequal fortune of the two interpretations? I will postpone this question and instead tackle another, more important question, namely: Why should this situation be changed? Why should the second, H-W-R interpretation be favored over the first? The answer, in my view, is very simple and obvious; it has two parts. (i) The interpretation offered by Hilbert, Weyl and Ramsey is closer to Hertz's *original intention* of "theory-formation" than Wittgenstein's *Tractatus* with its *linguistic* reconstruction of the picture-metaphor as a *theory of language*. (ii) The elaboration of Hertz's conception of theories as pictures or symbolic constructions by Hilbert, Weyl and Ramsey is much more sophisticated than any comparable proposal in the *Tractatus* – the cryptic remark in 6.341 about Newton's mechanics as a "form of description of the world" notwithstanding.

Now, this assertion seems not only to stand in *conflict* with my own interpretation of the *Tractatus* as a work which was strongly influenced by Hertz, but it seems also to be a wild and unjustified *exaggeration* in both its parts. On the contrary, I am still convinced that Wittgenstein when writing the *Tractatus* was deeply impressed and influenced by Hertz, and the manual of translation between Wittgenstein's *Tractatus* and Hertz's *Principles of Mechanics*, which I published some years ago (Majer 1983 and 1985), is still basically correct. However, I have changed my mind regarding Wittgenstein's *reading* of Hertz. Earlier I believed that Wittgenstein had understood Hertz's notion of picture very well, but now I am convinced that his understanding is rather superficial and misleading. Accordingly, I think that Wittgenstein's presentation of the picture theory in the *Tractatus* does not go to the heart of the matter – at least not as far as Hertz is concerned – but it fiddles around with logico-linguistic subtleties. Now this point is only a variant of my double assertion that the HWR-interpretation is closer to Hertz's original intentions than Wittgenstein's *Tractatus*, and furthermore, is much more sophisticated. Thus remains the objection that my assertion is a wild and unjustified exaggeration. This, I take very seriously. In order to dispel it and to justify my claim, I will proceed in the following somewhat dialectical way:

First, I will outline the different attitudes with which Wittgenstein, on the one side, and Hilbert, Weyl and Ramsey on the other, have read Hertz's work. This gives a certain hint to the adequate *context* in which Hertz's work should be seen, namely the context of *concept-* and *theory-formation* in the modern logical sense. Then, I will explain in greater detail what this means; that is, what the core of theory-formation is, seen from a modern mathematical perspective. After this, I will return to the *Tractatus* and show that Wittgenstein offered a completely different reading of Hertz's *Principles*, a reading which is rooted in the idea of logical analysis in general and of propositional functions in particular. In the final section, I will argue for three conclusions: first, that both readings are largely *incompatible*; second, that the HWR-interpretation is closer to the original *intentions* of Hertz, which can be judged as a middle course between Hilbert's *axiomatic* procedure and Weyl's *constructivistic* approach; and last, that, judged from the perspective of mathematics and natural science, the HWR-interpretation is superior to that of the *Tractatus.*

A HISTORICAL SKETCH OF THE ALTERNATIVE READINGS

First, let us consider the way in which Hertz's work was received, particularly his second book, *The Principles of Mechanics*. Here, two circumstances are remarkable: (i) philosophically-oriented scientists like Poincaré, Boltzmann and Hilbert reacted almost immediately; for example, Hilbert referred to the book already in his 1894 lecture "*Die Grundlagen der Geometrie* [The Foundations of Geometry]," the same year in which the book was published. He quoted from memory the famous sentence from the introduction of the book in which Hertz states his picture metaphor.[2]

We form for ourselves mental pictures [*innere Scheinbilder*] or symbols of external objects; and the form we give them is such that the necessary consequents of the pictures in thought are always the pictures of the necessary consequents in nature of the objects pictured. (PM 1)

Hilbert understood this sentence – as the context makes clear – as an excellent description of what he (and other scientists) were *actually* doing, when they *axiomatized* a theory, such as Euclidean geometry – or any other theory, whether mathematical or physical doesn't matter. I will come back to this point later; first, let me mention the second remarkable circumstance – how the philosophers reacted. (ii) What is remarkable is that they did *not* react at all, at least not the *professional* philosophers during the first thirty years after the publication of the *Principles*. Wittgenstein was an exception in every respect: first he was not a "trained" philosopher but an engineer; second, he had *studied*[3] primarily the *logical* writings of Frege and Russell and accordingly tried to *relate* the picture idea of Hertz to the logicism of Frege and Russell;[4] third, Wittgenstein's interest in Hertz's work must be at least as early as 1914 because he started his work on the *Tractatus* in this year. Professional philosophers – even those of the analytic tradition – first showed interest in Hertz's work, if at all, in discussing Wittgenstein's *Tractatus* in the 1920s and 1970s.[5]

Frege is a typical example for the first period. After he had discussed with Wittgenstein an early version of the *Tractatus*, he wrote a trilogy of essays called "Logical Investigations." In the first essay, "The Thought" (1919), Frege criticized the picture conception of thoughts as basically misconceived. Unfortunately, Frege did not mention Hertz explicitly in this context; hence, we don't know whether he studied his work or not. But, we know that Frege referred implicitly to Helmholtz as a "sense-physiologist," who in a chain of reasoning – starting from sense impressions as mere signs – comes to the conclusion that everything is an idea (in Hume's sense of representation). Now, Helmholtz was the principal teacher of Hertz, and this makes it probable that Frege not only sensed that Helmholtz's epistemology led to *idealism* and *solipsism*, but also that Hertz's conception of pictures *insufficiently* recognized truth.[6]

So, we have these two quite different receptions of Hertz's work by scientists like Hilbert on the one side and philosophers like Wittgenstein and possibly Frege on the other. The question arises: which reception is right? Of course, there is no right or wrong reception or interpretation of the picture metaphor, but only a more or less appropriate one. Before I deal with this question, I think it is useful to go a step further in the history and see what happened in the twentieth century after the *Tractatus* was published. Evidently not much. Philosophers paid little attention to Hertz, and Wittgenstein turned away from his own position in the *Tractatus*. Little wonder that there is not one reference to Hertz in Wittgenstein's discussions with the Vienna Circle, notably those with Schlick and Waismann.

And the scientists? Did they pay further attention to Hertz? It does not seem so because the first euphoria about the picture metaphor was long forgotten and the peculiar form in which Hertz had stated the principles of mechanics was highly problematic because of the "hidden masses" which Hertz had substituted in place of *forces*. So, it seems that only Hertz's discovery of the radio-waves has withstood the forgetfulness of scientific progress. Radio frequencies are still measured in "hertz." However, a closer inspection reveals that this impression is not quite correct. In 1928, Hermann Weyl published a long monograph essay, *Philosophie der Mathematik und Naturwissenschaft* [*Philosophy of Mathematics and Natural Science*], in which he referred to Hertz's picture conception of theories both implicitly and explicitly. Implicitly, he discussed the demand that a theory of the world should not contain superfluous terms, and explicitly, he referred to Hertz in connection with the question: What is the ultimate purpose of forming theories?[7] Hertz's inclusion here was by no means random, but it was part of Weyl's effort to find a new philosophy of science and mathematics, one that reconciled his own constructive position with the axiomatic approach of Hilbert, his former teacher and, subsequently, his prime opponent with respect to the foundations of mathematics and science. I will come back to this reconciliation which culminated in Weyl's notion of "science as symbolic construction."[8] For now, I will return to the discussion of the picture conception in the late 1920s.

About a year after Weyl published his essay a young English philosopher of mathematics, F.P. Ramsey, being dissatisfied with the *logicism* of Frege and Russell, turned to Weyl's book and became convinced that Weyl's characterization of theories as symbolic constructions along the lines of Hertz was basically right.[9] Of course, Weyl's pleading for Hertz had fallen on well-prepared ground because Ramsey had already studied the *Principles* one or two years before, as we know from his manuscript "On Truth."[10] In it, he considered the possibility that a theory might contain sentences which are neither true nor false because they functioned as general existence claims of theoretical quantities like "mass" or "force" in mechanics. Unfortunately, nobody really understood what Ramsey was after when he wrote his "Last Papers" on "Theories," "General Propositions and Causality" etc.,[11] not even his closest friends, Braithwaite and Wittgenstein, not to mention his teacher, Russell. Consequently, the second reception of Hertz's work ended as suddenly as the first, having no lasting effect. The situation first changed in the 1970s, when a new interest in the *Tractatus* also revived the interest in Hertz's writings.[12]

TWO INTERPRETATIONS OF THE PICTURE METAPHOR

Now, I return to the question: What is the right attitude with which Hertz's picture metaphor should be viewed and accordingly interpreted? Of course, as I have already stressed, there is no simple answer to this question – like the HWR interpretation is right and Wittgenstein's is wrong. Different interpretations are possible and the only *reasonable* question is: Which is the most appropriate one? To find a justified and reliable answer to this question, I investigate more closely and precisely the epistemological *context* in which the technical term "picture" was invented.

At first glance, the picture metaphor looks like a claim about *language* – how we form "mental pictures or symbols of the external objects" – and, in fact to a certain extent it is one! In his new theory of vision, Helmholtz had developed the idea that the diverse sense-impressions, like colors, pitches, and so on, form a kind of language in the following structural sense: a *single* sense impression is a mere *sign* for some external "object," which causes the sense impression. However, it is no *picture* of the external object (which causes the impression) because it has no *similarity* with it but only a causal connection to it! Single sense impressions are like the *letters* of an alphabet. However, the situation changes completely, if we go to *strings* of sense impressions. These can be *pictures* in the literal sense that they *represent*, in their space and time-like order, the external *order of things* which cause the impressions. Strings of sense-impressions are like *words* in a sentence; they represent in their internal relations some of the external relations among the things outside. This is only a cursory description of Hemholtz's idea, but it may suffice to indicate in which sense Wittgenstein is right when he reads the picture metaphor as a claim about language.

The picture metaphor, however, does not express a statement about language, at least not primarily as I will try to show, but it does express a statement about *theoretical thinking*. How we *transcend* the domain of immediately given phenomena in order to obtain a reasonable idea of phenomena we have not experienced. Of course, language is involved in this peculiar process of theorizing. This is one of the lessons we have learned from Hertz. But language is not a *rigid* system of rules to form complex statements out of simpler ones – as Wittgenstein suggests in his *Tractatus*. On the contrary, language is a *flexible* tool of our intellect to create *new* symbols in order to *expand* our sphere of cognition beyond the limits of the actual given phenomena. I explain below precisely what this means, but for the time being, it should be crystal clear that the picture metaphor viewed in this *creative* way, is not so much a statement about *language* as about a method of *cognition*, how we form new "theoretical ideas," with which we can predict the future from the past. Judged from this "creative perspective," the difference between Hertz and Helmholtz comes to this: while Helmholtz supposes the alphabet of our sense impressions to be *fixed* and only acknowledges a variation in the strings of impressions according to different sequences of causes, Hertz views the alphabet of theoretical thinking as open for new ideas which have no direct counterpart in the external world, but which we introduce for the purpose of prediction. This original difference becomes duplicated in the different readings of the picture metaphor. Wittgenstein's *Tractatus* is closer to Helmholtz's *static* view of language with a fixed alphabet; while the HWR-interpretation is more inclined to Hertz's *dynamic* conception of language as open for theoretical changes, in particular the incorporation of new ideas.

With this opposition of these two attitudes towards the picture metaphor in mind, I turn to the main question: What is the epistemological core of the HWR-interpretation? To find the right answer, one has to bear in mind that all three interpreters, Hilbert, Weyl, and Ramsey, were mathematicians as well as philosophically inclined natural scientists. This means, they could well judge whether a proposal in *philosophy of science* made sense from a *mathematical* point of view. Hertz's proposal, entailed in the picture metaphor, did make sense from the mathematical perspective; it reflected the mathematical method of *domain extension*, as it was practiced implicitly at least since the time of Leibniz, and more explicitly in the works of Kummer, Dirichlet, Dedekind, and many others. To make clear what I have in mind, let me explain briefly the method of *ideal elements*, as Hilbert called the process of domain extension[13] and its relation to the axiomatic point of view. Then, I return to the *difference* between Wittgenstein's logicistic reading of Hertz, as presented in the *Tractatus*, and the constructive interpretation of the picture metaphor by Hilbert, Weyl and Ramsey. This contrast sheds a new and interesting light on Hertz's epistemology, which lies hidden beneath the picture metaphor.

THE METHOD OF IDEAL ELEMENTS

What is the method of *ideal elements*? Why is it *essential* for mathematics? What is the *rôle* or function of this method in mathematics? Instead of giving a general and abstract answer, it is easier to present some concrete examples. Let me begin with geometry.

As you know, in Euclidean geometry we can prove the sentence that any two straight lines cut each other at most in one point. This sentence includes the special case that two straight lines do not cut each other at all. The reason is well known; to every point outside a given straight line and in the same plane, there exists exactly one straight line, called *parallel*, which does not cut the given straight line. So far so good. However, from a methodological point of view, distinguishing two different cases of cutting and non-cutting straight lines is rather disturbing. It would be much simpler and more harmonious if we had only one case to consider instead of two. Is it possible to have a version of (Euclidean) geometry such that all lines in the same plane cut each other in exactly one point? The answer, given by Klein, is yes; with the method of ideal elements, we can solve the task to simplify and harmonize geometry. All we have to do is to introduce new points at "infinite distance" such that every pair of parallels cut each other in one of these points at infinite distance.

Unfortunately however, the extension of the domain of geometrical objects by *ideal* points at infinite distance leads to a new inhomogeneity on a different level: The sentence of Euclidean geometry that two arbitrary points always determine a straight line loses its validity because two different points at infinite distance do not determine a single straight line but a set of parallels, i.e., two different *directions*. For this reason, we have to introduce a further ideal element into our domain of objects, namely a straight line, which connects all points at infinity. Every "common" straight line cuts this new ideal straight line in exactly one point determined by the "common" line's direction. Let's call the new element the "infinitely distant straight line." With this two-fold domain extension, by *infinitely distant points* and an *infinitely distant straight line,* we have obtained a version of geometry that can be stated in full generality, without any distinction of cases. We have the following two sentences:

(1) Two arbitrary points always determine uniquely one straight line.

(2) Two arbitrary straight lines always determine uniquely one point.

As you can see from the literal formulation of the two sentences, the result is a remarkable *symmetry* between points and straight lines; both correspond to each other in such a perfect manner that you can substitute, *salva veritate,* points for straight lines and vice versa. This "duality" between points and lines, their interchangeability *salva veritate*, is a result of the method of ideal elements. It could not be obtained in the old, more restricted domain of common points and lines. Hence, the method of ideal elements is essentially a method of *domain extension* by one or more sets of objects in order to obtain a simplification and unification of the relations between the objects of a domain.[14]

This example from geometry is by no means the only one. We encounter the method of ideal elements in every branch and at every level of mathematics. Probably better known is the case of arithmetic, where we rely on the method right from the beginning. (i) Having constructed the natural numbers by the process of induction we introduce *negative* numbers in order to simplify the operation of *subtraction*; (ii) from here we extend into the domain of *rationals* in order to universalize the operation of *division*; (iii) next, we introduce *Dedekind's cuts* in order to complete the discrete spectrum of rationals to the continuum of the real numbers; (iv) finally, we introduce the *imaginaries* in order to close and unify the domain of arithmetic operations. Hence, the method of ideal elements can be characterized, still rather roughly, in this way: the method aims at a *simplification* and *unification* of the propositions and operations in a certain domain of investigation. The method consists in extending a given domain of objects by new elements, such that within the extended domain the operations and propositions become simpler, more uniform and complete, or more self-contained.

But what has the method of ideal elements to do with Hertz's view of science that we make ourselves mental pictures or symbols in order to predict the future from the past? Apparently nothing at all, because the range of observable phenomena exists *independently* from our knowledge of it; it is simply *out there*, whether we know it or not. For this reason, we cannot *expand* it in any serious sense. Of course we can and do expand our *knowledge* of the phenomena. But, that is another matter; it has nothing to do with the *existence* of the phenomena, which are supposed, at least in science, to exist independently from us. This argument (correct as it is) overlooks an important anthropological fact which is epistemologically highly relevant; namely, human beings "organize" knowledge of phenomena deductively by introducing *new* elements, such as "mass" and "force" in mechanics, or "electric charge" and "polarization" in electrodynamics.

These new elements do not belong by *themselves* to the range of observable phenomena, but they are introduced exclusively for the purpose of explanation. Roughly speaking,[15] they serve the purpose of deduction and unification of the observable phenomena from few basic principles. That the *new* elements are *our inventions*, in an *axiomatic* presentation of the phenomena, is masked by the circumstance that the new elements appear "already at hand" or "given" together with the phenomena. That is a mistake, as Hilbert himself points out,[16] because their status of *existence* is a different one from that of the observable phenomena. They only exist *relative* to the *phenomena* and a particular *theory*, that is a certain set of *hypotheses* by which we explain the phenomena. The impression to the contrary only arises because in an *axiomatic* presentation of the theory, we do not distinguish between the *old* and *new*, the *given* and the *invented* elements; we deal with them as if they had the same status of existence. We become aware that this is not true when we see that we can, and in fact sometimes must, exchange them for different ones. Before I explain this more properly, let me turn to the *difference* between Wittgenstein's reading of Hertz, as presented in the *Tractatus*, and the HWR-interpretation. This comparison sheds an interesting light on Hertz's work.

A MINIMAL INTERPRETATION OF THE *TRACTATUS*

First, I have to say that there is no interpretation of the *Tractatus* to which everybody would agree. Nevertheless, there is a *minimal* interpretation to which everybody could agree. This interpretation is sufficient to contrast Wittgenstein's reading of Hertz with the HWR reading. First, one more preliminary: Most philosophers agree that Wittgenstein's two main sources of inspiration in writing the *Tractatus* were the works of Frege and of Hertz.[17] But there is no philosopher – at least none to my knowledge – who knows how to reconcile the works of Frege and Hertz without violating the intentions of one or the other or both. In this respect, Wittgenstein is no exception; his synthesis in the *Tractatus*, either is inconsistent or violates at least the intentions of one or both! The *Tractatus* is a mixture of the first and third ingredient: it is inconsistent *and* it violates both Frege's and Hertz's intentions. On the other hand, this does not matter because the minimal interpretation is so weak that such contradictions cannot be derived.

According to *Tractatus* propositions 1 to 1.13, the world is the totality of *all* facts. A *fact*, according to proposition 2, is the existence of a *state of affairs*, and a state of affairs, we are told by proposition 2.01, is a combination or – perhaps better – a *concatenation of objects* (2.03). The *relations* between the objects in a certain concatenation is called the *structure* of the respective state of affairs (2.032) and the structure of a certain fact is the *same* as that of the corresponding state of affairs. The *totality* of the *existing* states of affairs – that is the totality of facts – is the world.

Notice that this first part of the *Tractatus* gives only a *formal* description of the world. Formal in two senses: (i) It sidesteps the question, what objects, their concatenations and, hence, states of affairs really are, and (ii) it characterizes states of affairs as having only one of two "modi of existence." To be or not to be – *tertium non datur*. This point is confirmed by 2.05, where Wittgenstein asserts that the totality of the *existing* states of affairs also determines which states of affairs do *not* exist. This is not intended as a *logical conclusion* (but only as a partition of facts into *positive* and *negative*, in order to cope with the logical operations of *affirmation* and *negation*). Still, a *logical principle* is involved, that of the *excluded middle*. This is very important because this principle is highly problematic. Wittgenstein was not aware of this circumstance, as far as I know, because it is presupposed as valid throughout the *Tractatus*.

From the *principle of the excluded middle* for states of affairs, it follows at once – at least, if we suppose that the world is finite – that the totality of all facts also determines which states of affairs do not exist. This *determinism* can be constructed as a kind of possible worlds semantics; given a number of states of affairs, we can construct the set of all possible states of affairs by assigning every state of affairs one of two symbols, 0 and 1. The set of all sequences of the two symbols, 0 and 1, represents all possible worlds. The actual world, or *reality*, as Wittgenstein says, is represented by one of these sequences. This modal reading is supported by 2.06, where Wittgenstein explains *reality* as the existence and non-existence of states of affairs. One should be aware that not states of affairs *as such*, but the existence or non-existence of states of affairs, are part of reality. Accordingly, one *complete* chain of existing and non-existing states of affairs is identical with the actual world or reality: "The sum-total of reality is the world" (2.063).

What I have just outlined is frequently called the *ontology* of the *Tractatus*. I find this very misleading because we do not really become informed about which entities exist in the universe. We are only told that the world is the totality of existing and non-existing states of affairs, of positive and negative facts, without a third possibility.[18] Instead, I see it as a kind of *semantic* presupposition necessary for the *picture* theory of propositions developed in the central part of the *Tractatus*. Fortunately, I need only very few assumptions of the picture theory for my minimal interpretation, and I can ignore almost all philosophical subtleties and technical details. I need three aspects of the picture theory. The minimal interpretation includes:

First, the one-to-one *correspondence* between the *elementary* propositions and *states of affairs*. Once this is granted and the *logical operations* (affirmation, negation, conjunction and disjunction) are defined as a means with which *complex* propositions are built out of *simpler* ones,[19] the rest of the picture theory of propositions almost follows automatically. In particular, the general claim 4.01 follows that, "The [true] proposition is a picture of reality."[20]

Second, the minimal interpretation includes the principle of a *complete logical analysis* of every *meaningful* proposition. By this principle, I mean that it is possible to perform a logical analysis or decomposition of a grammatically simple sentence into a sequence of elementary sentences, such that the resulting sequence of elementary propositions is a *picture of reality*. This means that every elementary proposition occurring in the sequence is a picture of an existing or a non-existing state of affairs. The principle of *complete logical analysis* is more *implicitly* than explicitly stated in the *Tractatus*.[21] Its philosophical *significance* is this: whether or not a sentence really is simple, or only looks simple, it can be decomposed into its basic logical constituents, a conjunction of affirmations and negations of elementary propositions. Then it can be compared with reality.

Third, the minimal interpretation includes the assertion in 4.11 that, "The totality of true sentences is the whole of natural science." This assertion often is not taken seriously because today we tend to think it is false anyway. Without a doubt, it was intended as a *serious* assertion by Wittgenstein. It is important for my minimal interpretation for the following reason; even if we do not agree with this assertion, we can agree with a considerably *weaker* assertion, which lies at the bottom not only of Wittgenstein's convictions, but of the convictions of almost all philosophers of science at that time. The only *meaningful* propositions are the propositions of science. This together with the *principle of complete analysis* and the logical constitution of the world implies that the *principle of the excluded middle* is carried over from the *states of affairs* to the *sentences* of science; all sentences of science are either true or false depending on whether they do or do not *correspond* with reality.

This is the minimal interpretation of the *Tractatus* to which I think everybody can agree. Of this interpretation primarily I need the last result regarding the validity of the principle of the excluded middle for scientific theories.

THE HILBERT-WEYL-RAMSEY-INTERPRETATION

It is the supposed validity of the principle of excluded middle for all scientific sentences – including all theoretical sentences – which stands in sharp contrast with the HWR-interpretation. Although Hilbert,Weyl, and Ramsey have slightly different opinions, which I come to, regarding the principle of excluded middle, all three agree that the relation between science and the world, between a scientific theory and reality, is much more *indirect* and *hypothetical* than Wittgenstein supposed it to be in the *Tractatus*; they deny the one-to-one correspondence between facts and true elementary propositions. According to their view, Hertz was the first to notice that *not all* scientific sentences are either true or false, but some of them have a rather *different* and very *peculiar* relation to reality. According to their view, Hertz not only was the first to recognize this remarkable *meta-theoretical* fact, but also, he analyzed the *reasons* for the indirectness of the theory-world-relation correctly with his picture metaphor.

According to Hertz, usually we encounter the world *active*ly, creatively and constructively, in accordance with our manual and mental abilities; we are not simply *passive* receivers and describers, whatever the expressions may be. So far, all agree. Beyond this agreement, it is important to notice that our *mental* abilities generally exceed our *manual* abilities by far. This asymmetry encapsulates the core of the reason why the principle of excluded middle *cannot* be applied to *all* sentences of a theory; it has to be restricted to a certain subclass of statements. In their imaginations people could fly millennia before the first airplane was built. A similar asymmetry or *superiority* of our *theoretical abilities* over our *practical* abilities not only exists with respect to Greek myth but also with respect to *modern science*. This was the first message Hertz had to tell his positivisticly inclined contemporaries, Avenarius, Mach and others.

Hertz had a second message no less important than the first and closely related to the first; scientific *theories*, being the products of our mental activities, have an irreducible *hypothetical* character or *speculative* status. This cannot be removed from science completely without destroying science itself as a *theoretical* enterprise. It is this *aesthetic* component, as I will call it for reasons which will become clear in a moment, which provides the specific universal reason why the principle of excluded middle *cannot* be applied to a scientific theory as a closed *whole*. It can be applied only to a specific *fragment* of a theory, its so called *observational* consequences. This sounds rather mysterious or even highly dubious, but it is exactly the point which was taken up by Hilbert, Weyl, and Ramsey.

On this view, theoretical elements, like "mass" and "force," are introduced to *explain* the range of certain phenomena, to simplify and unify the predictions of the motions of material points or bodies; this is analogous to the introduction of negative and rational numbers to simplify and unify the arithmetical operations of subtraction and division. Now, *pure* theoretical sentences, like "Mass is a positive-valued function of material points," are neither true nor false *as such*, but only become so when they are *testable* in an empirical sense; to be empirically testable, such theoretical elements must be connected with the observable phenomena by some kind of "bridge laws" like the *proportionality* of force and acceleration. This seems trivial, but it is in fact the first and most simple reason why some scientific propositions are "either true or false" *per se*. To be so, these propositions must have at least some relations with the *observational* sentences, which in their turn are either true or false in the sense that they do or don't correspond with reality. Other reasons require other restrictions on the principle of the excluded middle, but before I investigate these, I first give some quotations from Hertz, which confirm the HWR-interpretation of his theory of science that the notion of picture or symbol entails an "irreducibly aesthetic component."

First, in the sentence quoted at the beginning, Hertz does not speak of "pictures" or "images" (as it has been mistranslated) but of symbols or inner or mental appearances of pictures ["*innere Scheinbilder oder Symbole*," PM 1, cf. §302] which we make ourselves in order to obtain a certain goal. This is very important because it saves us from the erroneous idea put forward in the *Tractatus* that every non-logical symbol must designate or represent some element of reality in order to make *sense*. Almost the contrary is true! Take for example the notions of *forces at a distance* and *absolute space*. Both not only are useful but *necessary* in Newton's theory of motion, inertia and gravitation. Nonetheless, we do *not* believe, and neither did Leibniz or Huygens, that they designate or represent some element of reality.

Second, as is well known, Hertz presents three "criteria" for the *evaluation* of the symbols we make ourselves in order to predict the future from the past: (1) the criterion of *logical consistency*; (2) the criterion of *empirical correctness*; (3) the criterion of *simplicity* and *distinctness*. The conjunction of the last two is, somewhat misleadingly labeled, "*die Zweckmäßigkeit der Bilder* [the appropriateness of pictures or images]" (PM 2).[22] But, this is not what I want to talk about; instead I want to point out the specific epistemological character of criteria (2) and (3). It is the relation between these two criteria that is the key to a correct understanding of Hertz's philosophy of science in general and its "aesthetic component" in particular. Of the second criterion, Hertz remarks:

The pictures we make of the things are *not uniquely* determined by the requirement that the consequences of the pictures must be the pictures of the consequences. *Different* pictures of the *same* objects are possible, and these pictures may differ in various respects. (PM 2, emphasis added)

I call this the *empirical underdetermination* of the choice between various possible pictures or theories. It is much debated today among different camps of philosophy of science. But I think, given Hertz's explanation for it, it *cannot* be denied for two reasons:

(i) The objection that scientists *de facto* agree in their choice among different theories is no objection at all because Hertz speaks of *possible* pictures. That no alternative pictures or theories are in use only shows a certain lack of scientific imagination or a mental laziness.

(ii) The objection usually put forward by *realists* that experience in the long run leaves us no choice among different pictures [theories] of nature because of the way things *really* are, is blocked by Hertz. Hertz stresses that he only speaks of *reality* with respect to *phenomena*, not in regard to *things in themselves*:

> The pictures which we speak of are our representations [*Vorstellungen*] of things; they are in conformity with the things in *one* important respect, namely in satisfying the above-mentioned requirement. For fulfilling their purpose it is not necessary that they should be in conformity with the things in any other respect whatsoever. Indeed, we do not know, nor have we any means of knowing, whether our representations [*Vorstellungen*] of things are in conformity with the things in any other than this *one* fundamental respect. (PM 1f.)

This remark shows that any objection to the empirical underdetermination of the choice among various possible pictures which relies on the *reality* of things as they exist *independently* from our "representations" is hopelessly muddled. It confounds the *meaning of symbols* with the things in reality. Of course, the meaning of such symbols is intended to be *objective*. That is, such meanings are constructed *as if* the entities represented by these symbols existed independently from us. But, this *objectivity* is no insurance that they are *real* in the specific sense needed by the objection. Namely, that the things supposed to *exist* in reality are the *same* as the things designated by us in concordance with all past and future phenomena. It is an essential aspect of Hertz's notion of symbol that it separates the sphere of strictly manmade *symbols* with their objective meanings from the real world, or "things in themselves [*Dinge an sich*]," as they exist independently from us. The latter idea is only a meta-theoretical construct, as Peirce noted correctly, for the explanation of scientific progress.[23]

Turning to the third criterion, it is natural to ask: is the underdetermination of theory choice unavoidable? Of course, we are convinced that the second criterion *alone* is insufficient to determine our choice among different possible theories uniquely. But with the addition of the third criterion, that of simplicity and distinctness, is the underdetermination left open by the second criterion, that of empirical correctness removed? The astonishing, somehow unexpected, answer is: No!

> But we cannot decide without ambiguity whether a picture is appropriate or not; as to this, differences of opinion may arise. One picture may be more suitable for one purpose, another for another. Only by gradually testing many pictures can we finally succeed in obtaining the most appropriate. (PM 3)

First, one has to notice the plural! That the plural is no slip becomes clear if one recalls that the existence of more than one picture for the phenomena of electrodynamics was the original motive for Hertz's introduction of his picture metaphor and for his theory of *symbols* outlined in the Introduction of the *Principles*. Here we encounter the core of the matter. But what do simplicity and distinctness mean? This is notoriously difficult to answer and I cannot answer in full detail. But so much I can, and have to, say:

Simplicity is the requirement that a picture (or theory) entails no *superfluous* elements where *superfluous* means roughly that the presence or non-presence of the elements in question have *no effect* whatsoever on the observable consequences of the theory.

Distinctness is the complementary requirement that a picture (or theory) entails *enough* elements to represent all the *objective* relations between the observed phenomena, which really *exist*; thus, a picture is distinct if no objective relation among the observable phenomena is *missing*.

Now, it is important to understand that these two requirements, although patently they are different, belong together – each compensates the other. Every effort to *raise* the distinctness of a theory raises the danger of introducing new superfluous elements; inversely, every attempt to eliminate an element assumed to be superfluous raises the danger of eliminating too much, such that some *objective* relations among the observed phenomena are lost. A good example of the delicate balance between simplicity and distinctness is the changing rôles of the mathematical concepts of potential and field, whose physical meanings were not always beyond doubt.

It is also very important to see why these two requirements (together with the first and second requirements of consistency and correctness) do *not uniquely* determine the choice of theory. The reason has to do with the fact that these two requirements do not constrain "nature" but our "reasoning about nature." This Hertz stresses because this remark is often misunderstood. Let me try to make it as clear as possible by approaching it differently.

Obviously, the requirements of simplicity and distinctness cannot apply to the set of *observational consequences* (of a correct theory) because this set is supposed already to be *uniquely* determined by the requirement of empirical correctness. In other words, it is supposed that all pictures which do not fulfill the requirement of empirical correctness are ruled out before the criteria of simplicity and distinctness are applied. Hence, the requirements of simplicity and distinctness must have a *different* target than the set of observational consequences of a theory. The "aesthetic" answer which Hertz gives is the only reasonable one, and it explains why the principle of excluded middle has no force with respect to theories as long as they are logically consistent and empirically correct.

The requirements of simplicity and distinctness address our "theoretical reasoning," the way in which we *theorize* about a certain domain of given phenomena in order to anticipate novel phenomena. What does it mean to *theorize* in this connection? Does it only mean to give a correct *description* of the phenomena? No, and once more no! Rather, it is a kind of "explanation" (if that expression is understood liberally enough) in the sense of offering *reasons* – not immediately sensible to us – but which allow us to deduce a maximum of future phenomena from past phenomena. This "explanation" or *Tieferlegung der Fundamente* [deeper grounding of the foundations], as Hilbert puts it, neither is *determined* by the nature of things outside as the realists suppose, nor is completely *independent* of them, as the idealists maintain. Instead, it must obey certain requirements to fulfill its purpose of facilitating our "understanding" of phenomena. These requirements are intimately connected with the nature of our thinking and reasoning. This brings me back to my starting point, the interpretation of Hertz's view by Hilbert, Weyl, and Ramsey. What is the main point of their interpretation and what has it to do with the rejection of the principle of excluded middle for theories?

The answer should be obvious by now: it is the method of *domain extension* by ideal elements, as Hilbert calls them, or by symbolic constructions, as Weyl says, or by the introduction of theoretical terms, as Ramsey labels them. Despite minor differences all three methods have this in common: we *transcend* the domain of immediately given phenomena, and step into the sphere of theoretical elements, elements which cannot be completely reduced to the phenomena. In this sphere, truth, although still important, cannot be our *sole* guide. Other aspects of theory-evaluation, such as simplicity and distinctness, play an equally important rôle. Further aspects like completeness and compactness have been discussed, but I will not go into these. All these criteria of theory-evaluation, by which we prefer one theory over another, have one remarkable characteristic in common: they are not, at least not initially, the expression of our logical faculty of thinking, like the requirements of logical consistency and empirical correctness are. Rather, they are the expressions of our faculty of *reasoning*, which aims at a certain perfection in the formation of theories. Hence, the proper question in regard to a theory is not "right or wrong" with respect to certain phenomena – that is supposed to have been settled in advance by empirical means – but, "better or worse" with respect to these "aesthetic" aspects of reasoning.[24] So far, I think, Hilbert, Weyl and Ramsey coincide in their interpretation of Hertz and differ from Wittgenstein. A certain divergence of interpretations arises over the question of the legitimacy of existence statements. I finish with this point, which is still rather controversial.[25]

Hilbert, the champion of the axiomatic method and domain extension by ideal elements, favors the most *liberal* unrestricted use of existence statements: for him an existence claim already is legitimate, if it does not lead to contradictions with the remaining axioms of a theory. Weyl stigmatizes Hilbert's position as "existence-absolutism." In contrast, Weyl imposes severe restrictions on any kind of existence assumptions. A simple existence assumption is legitimate, in fact it makes sense, only if it is abstracted from a concrete given object. A *general* existence assumption of the form "for all x, there is a y" is legitimate only if we have constructed a *law* f(x) = y, from which the general existence assumption can be deduced. This idea was picked up by Ramsey and slightly liberalized, insofar as he suspended Weyl's demand that the law has to be "constructed."[26] Hertz, it seems to me, steers a course between Hilbert's absolutism and Weyl's constructivism, which is rather close to Ramsey's view. According to Hertz, any creation of symbols is legitimate, which does not postulate empty or superfluous terms. That is, any existence assumption is legitimate, if and only if some observational consequences among the phenomena can be deduced from it, whether or not the object, as we have conceived it theoretically, really exists.

Now, this position leads to a serious question: if existence-claims cannot be verified by immediate inspection, because the postulated objects are not directly *sensible,* but only indirectly sensible through some consequences, what is the nature of truth? Here we run into one of the most serious problems of scientific epistemology; obviously, Wittgenstein's theory of truth does not work any more. The reason for its failure is not so much that the idea of correspondence is completely inappropriate, rather it is insufficient. It is insufficient in at least two regards; first, it does not say how we have to judge theoretical sentences which imply the existence of *theoretical* entities. A naive correspondence theory of truth cannot work, for a complete reduction of all theoretical elements to the observational phenomena would be necessary. This is blocked by the second respect in which theories are not mere *descriptions* of the world, but instead they have "merits of their own," like simplicity and distinctness. These cannot be evaluated by correspondence to the world, but only in relation to our mind. Therefore, Wittgenstein misrepresents Hertz radically when he says:

> That too says something about the world, that it can be described more simply with one system of mechanics than with another (*Tractatus* 6.342).

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NOTES

[1] The significance of the experiments lies in the fact that they show that the electromagnetic forces, which were believed to spread instantaneously in space, indeed need a certain amount of time: "This fact forms the *philosophic* result of the experiments; and, indeed, in a certain sense the most important result" (EW 19, emphasis added).

[2] This lecture, dated 1893, is included in Hilbert's *Nachlaß* at the *Niedersächsische Staats- und Universitätsbibliothek* in Göttingen. It was first delivered in 1894 with the reference to Hertz inserted on page 10 of the manuscript.

[3] It is difficult to say in which sense Wittgenstein had *studied.* Obviously, he had studied not in the usual sense of visiting the regular courses for undergraduates but to a large extent in an autodidactical style by working through the writings of Frege and Russell self-reliantly and then discussing their works with them.

[4] The question, whether and to what extent his effort was successful, I will first take up in the section on the minimal interpretation of the *Tractatus*.

[5] The second period of interest is the more important one because philosophers began to study Hertz's writings in their original form.This happened in the discussion of Wittgenstein's *Tractatus*. The only philosophers I know who dealt with the picture conception independently of the *Tractatus* are Cassirer's *Philosophie der symbolischen Formen* [Philosophy of Symbolic Forms] and Russell's *Principles of Mathematics*. An interest in Hertz's work *for its own sake* is still very rare among philosophers, but the situation has begun to change.

[6] Indeed from his perspective, Frege had good reasons for his suspicion that the picture notion would be as insufficient for the recognition of truth as Helmholtz's idea of sense impressions as mere signs. This is because Hertz's notion of "mental pictures" as "symbols of external objects" was built on Helmholtz's idea that *sequences* of signs are *pictures* of sequences of phenomena in the literal sense that both have the same order or structure. But "isomorphism of structures" is not enough for the recognition of truth in Frege's sense. For this we must recognize the things themselves, not only the structure of their appearances.

[7] The implicit reference is on p. 121 and the explicit one on p. 162 of Hermann Weyl's *Philosophy of Mathematics and Natural Science* (Princeton: Princeton University Press, 1949).

[8] This expression is taken from Weyl's conclusive essay "Wissenschaft als symbolische Konstruktion des Menschen [Science as a Symbolic Construction by Human Beings]," *Eranos Jahrbuch 1948* (Zürich: Rhein Verlag, 1948), pp. 375–473. This essay summarizes Weyl's reconciliation with Hilbert.

[9] This is shown by manuscript 005–17-01, written in August 1929 (Frank Ramsey Collection, Hillman Library at the University of Pittsburgh). Here Ramsey explicitly refers to Weyl's *Philosophie der Mathematik und Naturwissenschaft, op.cit.*, (note 7), pp. 162f., where Weyl explains Hertz's conception of theories, in particular his demand that a theory should contain no *superfluous* terms with respect to the explanation of phenomena, such as, for example, velocity in absolute space as Ramsey himself quite correctly notes with respect to Newton's mechanics.

[10] Compare the editorial introduction by Nicholas Rescher and Ulrich Majer to *Frank Plumpton Ramsey: "On Truth"* (Dordrecht: Kluwer, 1990).

[11] This situation remains unchanged, although the so-called Ramsey-view has won great popularity in the last two decades – thanks to the extensive work of Sneed and Stegmüller. But these authorities understand by the label "Ramsey-view" a totally different conception of theories than I do, and, I suppose, Ramsey did. Compare Ulrich Majer, "Ramsey's Conception of Theories: An Intuitionistic Approach," *History of Philosophy Quarterly* **6** (1989), pp. 233–258.

[12] The starting point was probably Hacker 1986 which was first published in 1972.

[13] For a more detailed exposition of this method, see Ulrich Majer, "Hilberts Method der idealen Elemente und Kants regulativer Gebrauch der Ideen," *Kant Studien* **84**:1 (1993), pp. 51–77.

[14] Cf. Dedekind's *Über die Einführung neuer Funktionen in der Mathematik*, in which he explains the method of domain extension. Cf. Dedekind's *Gesammelte Mathematische Werke*, edited by Robert Fricke, Emmy Noether, and Öystein Ore (Braunschweig: Vieweg, 1932), vol. 3, pp. 428–438.

[15] The concept of *explanation* deserves a more detailed exposition than I can give here. Here I focus only on its two main aspects: (i) its logically deductive structure and (ii) its unifying function. But I should mention a third point, which is often misunderstood: despite its deductive nature, the logical structure of an explanation is the opposite of that of a proof. In a "proof" we start from *true* premises and arrive at *conclusions*, which are *judged* to be true, because we have deduced them from true premises. In an "explanation" we already know the *explanandum* (the phenomena) to be true, and we seek one or several hypotheses, from which they *could* be deduced logically, if the hypotheses were true. But, of course, often we do not know, even after centuries, whether these hypotheses are true.

[16] See Hilbert, *Natur und mathematisches Erkennen*, lecture script for the 1919 summer term, prepared for publication by Paul Bernays (Göttingen: Mathematisches Institut der Universität Göttingen, 1919, reprinted 1990), pp. 104ff.

[17] See for example Gordon Baker, *Wittgenstein, Frege, and the Vienna Circle* (Oxford: Blackwell, 1988).

[18] Of course, Wittgenstein says a lot more about *objects* and *form*, *substance* and *relations*, etc. But all this is philosophical flourish, because it has no technical implications.

[19] What I have in mind is explained in *Tractatus* 5.2–5.4. Here, Wittgenstein first explains his notion of operation and then says in 5.234: "The truth-functions of the elementary sentences are the results of operations, which have the elementary sentences as their bases." Obviously he means complex instead of elementary sentences in the first instance, because otherwise the next proposition, 5.2341, would make no sense: "Negation, logical addition, logical multiplication, etc., are operations."

[20] "Proposition" has to be understood as "completely analyzed proposition" in order to assure that it is in its *distributive normal form*: a conjunction of affirmations and negations of elementary propositions.

[21] The two most explicit statements are 3.25 "A proposition has one and only one complete analysis," and 4.221 "It is obvious that the analysis of propositions must bring us to elementary propositions which consist of names in immediate combination."

[22] Of course, Hertz is aware that this notion is a bit ambiguous because he remarks: "The appropriateness of which we have spoken has no reference to practical applications or the needs of mankind" (PM 40).

[23] See Ulrich Majer, "Ein konstruktiver Begriff der Wahrheit," in *Analyomen*, edited by Georg Meggle and Ulla Wessels (Berlin: de Gruyter, 1994), pp. 225–240.

[24] It should be obvious that not only is this the case in natural science, but also in mathematics, because, as soon as we leave the domain of finite mathematics and enter the domain of infinite mathematics, we have no other guide than our own reason.

[25] For a more extensive discussion of this question see Ulrich Majer, "Zu einer bemerkenswerten Differenz zwischen Brouwer und Weyl," in *Exact Sciences and their Philosophical Foundations,* edited by Wolfgang Deppert, Kurt Hübner, *et al.* (Frankfurt: Peter Lang, 1988), pp. 543–551; and *op.cit.*, (note 11).

[26] What it means to "construct a law" is not easy to say. The best approximation may be the notion of a *mechanism* or operation, by which we move from one individual object to the next. Ramsey suspended the constructive demand to specify a mechanism or operation. For Ramsey, it is sufficient to *know* a law, which implies the general existence claim.

GERD GRASSHOFF

HERTZ'S PHILOSOPHY OF NATURE IN WITTGENSTEIN'S *TRACTATUS**

1. INTRODUCTION

By many remarks in his writings and to his friends Wittgenstein expressed his high esteem of Heinrich Hertz. During his second period in Cambridge in 1931 Wittgenstein jotted into his notebook thoughts resembling Fania Pascal's reports of "confessions" which Wittgenstein revealed to his embarrassed friends. These confessions were made with the strong inner urge to reveal all "sins" hoping for a relief from inner pains and self-accusations. Among these notes there is a passage where Wittgenstein reflects about his own original philosophical contributions.

> There is truth in my idea that really in my thinking I am only reproductive. I believe that I have never invented a new line of thought: that has always been given me by someone else. I have only seized on it immediately with a passionate urge for the work of clarification. That is how Boltzmann, Hertz, Schopenhauer, Frege, Russell, Kraus, Loos, Weininger, Spengler, Sraffa influenced me.[1]

This most carefully assembled passage signifies possible influences on Wittgenstein's intellectual development.[2] The list of persons who had an influence on Wittgenstein's thought is in chronological order. Only Boltzmann, Hertz, and Schopenhauer precede Frege and Russell. When we limit our search for possible intellectual stimuli to these sources, what conceptions of those authors might have influenced the young Wittgenstein? Wittgenstein's biographer, Brian McGuinness, has a clear-cut assignment:

> Hertz and Boltzmann gave him the idea of a mental picture or correlate of reality in which all that was essential was the logical structure of (in their case) the scientific theory involved. Russell, however, provided the tools for the extension of such analysis to the whole of our language. With the help of these tools Wittgenstein thought we could see what had to exist or be the case and what might (and might as well) be one way or the other. No matter that his conception of logic and language finally differed from Russell's: it arose from Russell's question what logic could show us about the nature of language and it was the Archimedean point in his book, the logical insight that solved the problems of philosophy.[3]

Like many others, McGuinness acknowledges that the very idea of empirical statements as pictures of reality is due to Boltzmann or Hertz. A number of commentators attribute the Hertzian heritage in Wittgenstein's early theory to the picture theory. Griffin was one of the first to phrase it:

D. Baird et al. (eds.), Heinrich Hertz: Classical Physicist, Modern Philosopher, 243–268.

The picture theory comes almost in its entirety from Hertz. Wittgenstein was the first, perhaps, to apply the picture theory of meaning to the whole of language, but not the first to apply it to a part.[4]

According to this interpretation Wittgenstein assumed the picture theory according to which language and thoughts mirror essential structures of the real world. Yet, while Hertz applied the picture theory to the field of physics, Wittgenstein extended the concept to the entire set of propositions without its restriction to the language of physics.

Parallels between Hertz's and Wittgenstein's thought can certainly be established in the following fields.

(a) Both authors follow a motif of conceptual criticism, which dissolves the "bewitching of the mind" and dissolves pseudo-problems caused by hidden conceptual confusion.
(b) Simplicity or other pragmatic reasons decide between alternative correct theories.
(c) All scientific theories are in principle fallible.
(d) Both share the notion of causality, according to which necessity of nature reflects only necessity of thought.
(e) Both develop and advocate a picture theory, according to which structures of the world are mapped to pictorial elements, which can take the form of linguistic expressions.

Except for the picture theory I shall comment on these parallels only marginally. Instead I shall concentrate on a key element of Wittgenstein's early philosophy, which has remained unclear and left interpreters of the *Tractatus* with the impression of its being an enigmatic book with aesthetic attraction, yet with hardly any useful application. The key for any interpretation is the interpretation of "simple objects," from which states of affairs are composed. Of states of affairs we make ourselves pictures to thereby obtain knowledge of the external world. Contrary to the common view, Wittgenstein's philosophical starting point is not the logic or the philosophy of language fostered by Russell and Frege. The *Prinzipien der Mechanik* of Heinrich Hertz was Wittgenstein's leitmotif and philosophical stimulus. With a full grasp of its metaphysical content, Wittgenstein used it as the foundation for the philosophical architecture which he built in close connection with the logical theory proposed by Russell and Frege.

2. BIOGRAPHICAL RELATIONS

Many biographical sketches of Wittgenstein's intellectual maturation suggest that the *Tractatus*, Wittgenstein's earliest book, is an extension of the logical considerations of Russell and Frege. Anscombe, along with many others, emphasizes the philosophical ancestry of the *Tractatus*:

If we look for Wittgenstein's philosophical ancestry, we should rather look to Schopenhauer; [...] for the rest, Wittgenstein's philosophical influences are pretty well confined to Frege and Russell.[5]

Wittgenstein's strong affection for contemporary physics cannot be overlooked.[6] Until the age of fourteen Wittgenstein was educated privately. He finished school with the Matura in the Realgymnasium of Linz and his musical brother Paul had no doubts that Ludwig's aspiration and talent were in physics. Ludwig's mediocre marks in these subjects do not contradict Paul's impression. A non-standard education and Wittgenstein's nonconformist character easily could have caused deficiencies in the canonical knowledge required for the Matura. After he had mastered the examinations, he intended to study under the most famous Viennese physicist of the time – Ludwig Boltzmann.

2.1. Boltzmann

Boltzmann had been asked by the Austrian ministry of education and cultural affairs to lecture on "The Philosophy of Nature and Scientific Methodology."[7] He filled the vacant lectureship in Philosophy, once held by Ernst Mach, by giving lectures without taking the philosophy chair. The *Neue Freie Presse* reported of his inaugural lecture on philosophy of nature: "When the doors to the lecture room were opened, there was a dangerous crush... The speaker, who was greeted with tumultuous applause, recalled Ernst Mach at the beginning of his lecture."[8] The lectures were held throughout the winter semester 1903/1904 and they were continued a year later. The last six lectures ended on January 22, 1906.[9] Boltzmann's philosophical ambitions, which Ernst Mach considered incredibly naive and casual, are characterized in a letter which he wrote to Brentano: "This winter semester I am lecturing again on the philosophy of nature two hours a week and I am making life easier for myself. I take any one of the well-known philosophers (at the moment Schopenhauer) and, using the philosopher's view, I show where I agree or disagree with him."[10] Yet Boltzmann had been the first intellectual influence mentioned by Wittgenstein. This influence came either through his lectures held on a popular level, or through the publication *Populäre Schriften*, a copy of which Wittgenstein is known to have possessed.

2.2. Engineering

The very year, 1906, Wittgenstein was preparing to enter the university, Boltzmann committed suicide. At the beginning of the same academic year, Wittgenstein enrolled in the Technische Hochschule in Maschinen-Ingenieurswesen in Berlin-Charlottenburg. McGuinness could not locate any enrollment lists of the time,[11] but Wittgenstein should have taken obligatory courses in physics and mathematics, which are required for engineering degrees. In 1908 Wittgenstein left Berlin for Manchester, where he again enrolled in engineering. According to the memories of his sister Hermine, it was at this time that Wittgenstein's interest shifted to the philosophical foundations of natural sciences. His new horizons apparently could not be sufficiently satisfied in the field of engineering and Wittgenstein visited Frege in Jena in Summer 1911 asking for advice. There are several accounts of Wittgenstein's report to his friends about this visit:

> I wrote to Frege putting forward some objections to his theories, and waited anxiously for a reply. To my great pleasure, Frege wrote and asked me to come and see him. [...] He absolutely wiped the floor

with me, and I felt very depressed; but at the end he said "You must come again," so I cheered up. I had several discussions with him after that. Frege would never talk about anything else but logic and mathematics; and if I started on some other subject, he would say something polite and then plunge back into logic and mathematics.[12]

Particularly interesting about Wittgenstein's account is his complaint about Frege's unwillingness to talk about anything but logic and mathematics. It is unlikely that Wittgenstein complained of a lack of small talk. According to two independent sources (von Wright and Wittgenstein's sister) (a) Wittgenstein went to Frege with a prepared list of philosophical questions and (b) Frege referred him to Russell for further studies. Wittgenstein had read Frege's work and might have considered him initially as the most suitable expert to answer his questions. Why did Wittgenstein not study with Frege and why did Frege send him to Russell as the most suitable teacher for his kind of interest? Clearly Wittgenstein could afford to choose the best universities with teachers of his liking. Hardly a personal motive attracted him to Cambridge; as far as we know he had no friend nor relative in Cambridge, and no friendship from his time in Manchester continued after he left. It must be the difference in expertise between Russell and Frege which made the former a better choice in the eyes both of Frege and Wittgenstein. Frege's recently found correspondence addressed to Wittgenstein provides no answer to the riddle and leaves it open to a wide range of speculation.[13] The very fact of the visit in Jena and its outcome manifests, however, that Wittgenstein had a philosophical problem which perturbed him more than a prospect of further work in engineering, the answer to which might have required knowledge of a wider range of fields than Frege was willing or able to talk about. In the first known philosophical notes from 1913, Wittgenstein defined philosophy as logic and metaphysics.[14] Philosophy is understood as a necessary prerequisite for any scientific investigation; its task is to clarify conceptual obscurities and provide a secure basis for the undertaking of science including metaphysics. Frege had nothing to offer in this respect, while Russell undoubtedly was the most prominent philosopher who worked both in logic and the foundations of modern science. The correspondence from Frege to Wittgenstein during the war shows that Wittgenstein never lost interest in Frege, but that it was a one-sided affection. The subject in the correspondence (the letters from Wittgenstein to Frege seem to be lost) is restricted to war experiences and mathematics. After Wittgenstein had sent Frege a copy of the *Tractatus*, Frege expressed his sheer non-comprehension of even the first sentences of *Tractatus* in a letter from June 28, 1919. The exchange of ideas between Frege and Wittgenstein certainly was not intense.[15] While Wittgenstein sought logical clarification from Frege and Russell, he brought with him a conception of philosophy of nature, which is largely due to Heinrich Hertz's *Prinzipien der Mechanik*.

3. PRINCIPLES OF MECHANICS

3.1. Hertz's methodology

In his often cited Introduction to the *Mechanics* Hertz says scientific theories represent the external world via pictures [*Scheinbilder*] or symbols [*Symbole*]. For a reader of our time, the title of the book – *Principles of Mechanics* – suggests that the book covers a disciplinary fraction of physics called "mechanics," and not the foundations of other physical disciplines and natural sciences. Yet, for Hertz and many of his contemporaries, physics was the only fundamental theory to which all statements of other sciences ultimately could be reduced. Furthermore, at the time of Hertz and Boltzmann the foundation of physics was laid out by mechanics exclusively. Outside of mechanics there was no other physical domain of (non-mechanical) objects about which one could theorize. All other seemingly non-mechanical concepts like those of heat and electricity ultimately would be reduced to mechanical notions. Hertz envisaged mechanics as a universal science which in principle describes all external states of the world and their changes. It was one of Hertz's major undertakings to reduce the electrodynamical laws, expressed in Maxwell's Laws, to mechanical equivalents. As a pupil of Helmholtz, it was clear to Hertz that all physiological facts, such as sense perception, should be expressed by a mechanical theory.

Hertz's intention in writing the *Mechanics* went beyond the wish to express a universal, true theory. Hertz also criticized the conceptual clarity of the systems of definitions in established forms of theorizing in physics. Newton, in his *Principia Mathematica*, formulated his laws in terms of four fundamental concepts: space, time, mass and force.[16] Newton's definition of force was widely criticized and frequently rejected. In his Introduction, Hertz discussed the main sequence of theories after Newton. He developed three main criteria for methodologically evaluating them. All acceptable theories must be (logically) permissible [*zulässig*] in the sense of being conceptually consistent and coherent with the *Denkgesetze*, i.e. logic. They must be correct [*richtig*] by being in accordance with experience [*Erfahrung*], i.e. compatible with all empirical information. If there are two correct theories – Hertz considered all physical theories, from Newton, Lagrange and up to Hamilton's theory, to be correct – then the most appropriate [*zweckmässig*] theory is preferable. A theory is more appropriate than another, if it is either more distinct [*deutlicher*], i.e. it represents more essential relations [*Beziehungen*] between objects, or in the case of two equally distinct theories if it is simpler than the other by having less *superfluous* [*überflüssige*] or empty relationships (PM 2).

The best theory, accordingly, would be (1) *permissible*, i.e. logically sound, (2) *correct*, i.e. compatible with all empirical information, and (3) most *appropriate* by representing all essential relationships of the world in the simplest fashion. Hertz's own theory satisfied these constraints: like the preceding mechanical theories, his *Mechanics* accords with all experiences; it is a universal theory about every fact of the external world, and it is maximally simple, since it is based on only one empirical Fundamental Law [*Grundgesetz*] (PM 33–40). No other theory before and after Hertz displayed such a structural elegance and logical clarity while being a universal theory of all phenomena and causal processes in the external world.[17]

A theory as a picture of the world is correct, if symbols of the picture denote objects in the world and if the relations expressed in the picture accord with experience. According to Hertz, we use symbols representing objects in the external world in such a fashion that necessary consequences of the picture [*denknotwendig*] in our thoughts are taken to be necessary consequences of nature [*naturnotwendig*] (PM 1). Any correct scientific theory faces a permanent threat of empirical falsification since predictions about the future constellations of objects in the world might turn out to be false. Being well aware of the history of physics with various different yet empirically equivalent mechanical theories, Hertz maintains that in principle many different theories could be compatible with the given experiences.

3.2. *Hertz's definition of mechanical concepts*

3.2.1. Mass-particles and mass

After the Introduction, Hertz's *Mechanics* is divided into two main parts. The first part introduces the physical concepts and theorems without reference to the external world; all propositions express, according to Hertz, judgments *a priori* in the Kantian sense. They are affirmed by "laws of inner imagination" and the forms of logic. Only in the second book are the physical concepts thus defined related to events in the external world (PM §1, §296).

In the beginning of the first book Hertz defines the three fundamental notions of physics: space, time and mass. The notion of mass is introduced in the first group of definitions. One should be very careful with the translation of the set of technical terms related to masses, because they are an easy source of confusion. Consequently, I quote translations of key passages and comment on them.

Definition 1. A mass-particle [*Massenteilchen*] is a characteristic by which we associate without ambiguity a given point in space at a given time with a given point in space at any other time.

Every mass-particle [*Massenteilchen*] is invariable and indestructible. The points in space which are denoted at two different times by the same mass-particle [*Massenteilchen*], coincide when the times coincide. Rightly understood, the definition implies this. (PM §3)

In the original German, Hertz defines a *Massenteilchen* as a characteristic property [*Merkmal*] of space and time. The English translation of *Massenteilchen* as "material particles" is misleading and easily confused with "material points" as defined in definition 3. Since a number of *Massenteilchen* is identified with the numerical value of mass, one should prefer the translation "mass-particles." For this reason I have changed the English translation of *Massenteilchen* from "material particles" to "mass-particle."

Mass-particles are space-time locations with a particular property; they are not material objects in space and time. Mass-particles are *attributes* [*Merkmale*] of space and have themselves no spatial extension. It is quite unusual to define the basic concepts of mass as properties of space and time, and not as some kind of entities in space and time. What does it mean for a space-time location to have that particular property? Since the notion of a mass-particle is basic to Hertz, and cannot be reduced to other physical concepts, one cannot say that a particular space-time location being a mass-particle is a location with a property X. It is interesting to note that the common associations with the definition of mass, e.g., the property of being heavy, are not used in the definition of mass-particle. The function of mass-particles at this point is just to mark uniquely a space-time location, so that such points are countable. That is all that is required to define the concept of mass. Mass-particles are not objects in the world besides being called particles. Hertz introduces them to define the property of mass attributed to objects, which he calls *material points*.

Mass is a measure of the (relative) number of mass-particles. The mass of a space volume is defined as the numerical ratio of mass-particles compared to a reference space volume. According to this definition it is not possible to determine the mass of one object without a standard of reference. The definition of mass given by Hertz is:

Definition 2. The number of mass-particles [*Massenteilchen*] in any space, compared with the number of mass-particles [*Massenteilchen*] in some chosen space at a fixed time, is called the mass contained in the first space.

We may and shall consider the number of mass-particles [*Massenteilchen*] in the space chosen for comparison to be infinitely great. The mass of the separate mass-particles [*Massenteilchen*] will therefore, by the definition, be infinitely small. The mass in any given space may therefore have any rational or irrational number. (PM §4)

3.2.2. Material points

According to definition 2, in an indefinitely small imagined volume of space, there is a finite or infinite amount of mass. Hertz understands mass as a numerical quantity attributed to a space (time) region or – as a singularity – to a point in space and time. The mass related to the space-time region is called "material point" according to definition 3.

Definition 3. A finite or infinitely small mass, conceived as being contained in an infinitely small space, is called a material point.

A material point therefore consists of any number of mass-particles [*Massenteilchen*] connected with each other. This number is always to be infinitely great: this we attain by supposing the mass-particles [*Massenteilchen*] to be of a higher order of infinitesimals than those material points which are regarded as being of infinitely small mass. The masses of material points, and in particular the masses of infinitely small material points, may therefore bear to one another any rational or irrational ratio. (PM §5)

Physics rarely if at all applies mechanics to singular material points. Typically, huge sets of material points are considered as objects over which mechanical laws apply. Hertz calls such sets *systems*:

Definition 4. A number of material points considered simultaneously is called a system of material points, or briefly a system. The sum of the masses of the separate points is, by §4, the mass of the system.

Hence a finite system consists of a finite number of finite material points, or of an infinite number of infinitely small material points, or of both. It is always permissible to regard a system of material points as being composed of an infinite number of mass-particles. (PM §6)

3.3. The Fundamental Law

Hertz defines mass-particles as the smallest, unchangeable and indivisible attributes of space-time. A number of them in a space region is called a material point. Does

Wittgenstein's use of the same term match the Hertzian understanding? When Wittgenstein speaks of material points he means external things. In the second book of the *Mechanics* Hertz treats the application of his mechanical concepts to the external world. The preliminaries of the second part clarify the relation between the notions as defined in the first part and external experience.

Prefatory Note. In this second book we shall understand times, spaces, and masses to be symbols for objects of external experience; symbols whose properties, however, are consistent with the properties that we have previously assigned to these quantities either by definition or as being forms of our internal intuition. These statements are based, therefore, not only on the laws of our intuition and thought, but in addition on experience. (PM §296)

In what follows, Hertz emphasizes that his theory makes statements about the relations of time, space and their masses in general, not about singular masses in space and time. This is exactly Wittgenstein's point in *Tractatus* 6.3432. In no section of the *Mechanics* does Hertz's theory contain names of individual material points or systems of them.

How do we apply the a priori defined theoretical concepts to the world of external experience?

Time, space, and mass in themselves are in no sense capable of being made the subjects of our experience, but only definite times, space-quantities, and masses. Any definite time, space-quantity, or mass may form the result of definite experience. We make, that is to say, these conceptions symbols for objects of external experience in that we settle by what sensible perceptions we intend to determine definite times, space-quantities, or masses. The relations which we state as existing between times, spaces, and masses, must then in future be looked upon as relations between these sensible perceptions. (PM §297)

After the introduction of the physical notions for the external objects and their relations, Hertz defines the classical concepts of energy and motion. Hertz proceeds with the introduction of his Fundamental Law [*Grundgesetz*]. It is the only underived, principle proposition which can be falsified by empirical data. The Fundamental Law is not agreed upon by conceptual convention, nor can it be justified by other theorems. It is assumed as a principal law of nature. Hertz reduces the entire mechanics, i.e., the general scientific theory about the structure and events of the external world, to a large set of well-defined conceptions and one empirical principle. Here it becomes apparent how elegantly Hertz succeeds in realizing his own methodological requirements. Besides the conceptual definitions, Hertz uses only *one* empirical principle, which is only justified by the correctness of the theory; his theory is optimally distinct because it deduces all scientific propositions and it is also optimally simple, since it is just one proposition, from which everything follows. Hertzian *Mechanics* is supposed to describe all phenomena of the external world; even those, as emphasized by Boltzmann in his inaugural lecture, which are commonly described by biological and chemical laws.

Hertz formulates the Fundamental Law both in German (English translation given here) and Latin:

Fundamental Law. Every free system persists in its state of rest or of uniform motion in a straightest path.

Systema omne liberum perseverare in statu suo quiescendi vel movendi uniformiter in directissimam. (PM §309)

Hertz adds the Latin translation of the Fundamental Law to emphasize the proximity to and differences from Newton's Lex I:

Corpus omne perseverare in statu suo quiescendi vel movendi uniformiter in directum, nisi quatenus a viribus impressis cogitur statum illum mutare.[18]

In these very carefully crafted sentences Hertz removes all the opacity of Newtonian *Mechanics*, and highlights the architectural differences. We leave aside the fact that Newton's system, unlike Hertz's, requires more than one fundamental law and that Newton's definition of the concepts of mass and force are unsatisfactory. We instead restrict our analysis to the differences between Hertz's Fundamental Law and Newton's Lex I.

While in Newton's theory physical laws reduce the motions and forces to those of *single bodies*, Hertz's Law is formulated for *free systems*. In Newton's theory the absence of forces guarantees that single bodies are either at rest or in uniform linear motion. Forces which cannot be introduced independent of mass and space-time, in Hertz's understanding, consequently, also are not part of Hertz's Fundamental Law. In order to avoid the introduction of the concept of force, Hertz needs to introduce the notion of a system of material points, which is *free* if and only if it moves on the straightest path defined over all individual motions.[19] The motion of parts of a free system can only be indirectly determined. First one has to find a system whose overall motion satisfies the Fundamental Law. From the free system, and the Fundamental Law, one can deduce the motion of parts, if complementary parts are determined by experience, or they can be assumed from the mechanical form of the system. The motion of all parts of a free system always follows as a system the straightest path (which is defined for the system of material points, not for the individuals). The important architectural difference between Newton's and Hertz's *Mechanics* lies in the direction of analysis: Newton deduces motions of the system from the individual particles, while Hertz infers the motions of parts from the entire ensemble of particles. In the *Tractatus* there is an astonishing parallel.

3.4. Similarities with the Tractatus

In this short exposition of Hertz's *Mechanics* we recognize a wide range of views which Wittgenstein articulates in the last part of the *Tractatus* (6.3ff). These sections have been widely recognized as written under the inspiration of Hertz. They cover the following:

(a) the criterion of simplicity for a preference between correct theories,
(b) the fallibility of theories,

(c) the motif of conceptual criticism, which dissolves the "bewitching of the mind" and its pseudo-problems caused by hidden conceptual confusion,
(d) and the notion of causality.

In all these methodological theses Wittgenstein accords with Hertz. Once Wittgenstein even refers the reader to Hertz's *Mechanics* in order to express his thoughts in a different voice (*Tractatus* 6.361). Similar explicitly approving references to Russell and Frege are rare.

4. WITTGENSTEIN'S ADAPTATIONS

4.1. Wittgenstein's material points

How closely are the mechanical concepts of Hertz and Wittgenstein related? Hertz distinguishes the notions of mass-particle, mass and material point. Wittgenstein makes use only of the notion of *materieller Punkt*, e.g. in *Tractatus* 6.3432:[20]

6.3432 Wir dürfen nicht vergessen, daß die Weltbeschreibung durch die Mechanik immer die ganz allgemeine ist. Es ist in ihr z.B. nie von *bestimmten* materiellen Punkten die Rede, sondern immer nur von *irgendwelchen*.

There is a large variation between the first translation by Ogden (assisted by Ramsey) and the second translation by Pears and McGuinness. Ogden's translation juxtaposes the English translation with the German original as wished by Wittgenstein. This clear request by Wittgenstein is disregarded in all later English editions. Here are both translations:

(Ogden) 6.3423: We must not forget that the description of the world by mechanics is always quite general. There is, for example, never any mention of *particular* material points in it, but always only of *some points or other*.

(Pears/McGuinness) 6.3423: We ought not to forget that any description of the world by means of mechanics will be of the completely general kind. For example, it will never mention *particular* point-masses: it will always talk about *any point-masses whatsoever.*

Compare the translations of the first sentence. Here Wittgenstein characterizes the general structure of mechanics as a physical theory. *Die Weltbeschreibung durch die Mechanik* is a singular term referring to the one description of the world by means of mechanics. As we have seen, in Wittgenstein's time mechanics was considered to be a general theory applicable to all empirical propositions; mechanics is the scientific means to give an all-embracing picture of the world. In the first sentence, Wittgenstein qualifies this world description, namely that it is always quite general: the theory never contains names referring to individual things – it is *immer ganz allgemein*. The theory contains only variables and class terms for those names, which are instantiated when the theory is applied to concrete situations. Wittgenstein's *immer* emphasizes that the generality holds for all theorems within mechanics (i.e., within one single scientific theory). "Always" is not meant in a temporal sense or as a quantifier for various scientific theories. The translation of Pears and McGuinness – "any description of the world by means of mechanics" – is ambiguous. It could be taken to refer to any scientific theory of the mechanical type, maybe besides others; yet Wittgenstein never considered other scientific theories. It could also be understood to say that any *application* of mechanics to particular worlds at particular situations in space and time is quite general. Yet, to state that such an application does not contain singular terms is clearly wrong. Any application of mechanics, e.g., to the motions of a particular pendulum in the world, contains singular terms. The logical difference between the two translations can be further elucidated by the second sentence. It is clear that the "X is always quite general" is equivalent to "X never has particulars." Hence the "always" of the first sentence corresponds to the "never" of the second and cannot quantify different world descriptions.

The biggest flaw in the translation by Pears/McGuinness is the inexplicable rendering of *materielle Punkte* as "point-masses." In the Preface both translators state that the authorized first translation by Ogden and Ramsey "has been revised in the light of Wittgenstein's own suggestions and comments in his correspondence with C.K.Ogden about the first translation." In the correspondence we find nothing to justify the changes in this passage 6.3432. The fine differences of the three Hertzian terms ("mass-particle," "mass," "material point") are confused in the translation of Wittgenstein's term of *materieller Punkt* as "point-mass," which comes closer to the Hertzian notion of *Massenpunkt*. The term "mass" applies to the property of a space point or a collective of them as an *a priori* concept. Hence "mass" is used in contexts of *numbers* only. When we talk about the external world and the *matter* in it, Hertz, like Wittgenstein, speaks of material points. Incidentally, Wittgenstein himself gave his translator Ogden a hint how to translate the technical terms: "To get the right expression please look up the English translation of Hertz's *Principles of Mechanics*."[21]

4.2. The logicistic interpretation

At the center of the *Tractatus* resides the *picture theory*. Sentences give one kind of picture such that elementary components of language can be unambiguously correlated to the elements of the world; furthermore, their syntactic combination is reflected in the structure between things in the world (*principle of correspondence*).

There are different possible emphases in the construction of a full theory of possible pictures of the world. After the principle of correspondence is accepted, one needs to develop only one side of the picture, since thereby the other side's structure is determined. McGuinness – in the tradition of Griffin and others – acknowledges the heritage of Hertz (or Boltzmann) concerning the principle of correspondence. In his interpretation – and in that of many others – Wittgenstein developed the full picture theory by an initial elaboration of the symbolic side of the picture.

This interpretation can be supported by a very authoritative witness. Russell was very concerned to assist publishing the *Tractatus*. After a meeting with

Wittgenstein in The Hague in 1919 he offered to write an introduction to the *Tractatus* in order to help the reader with the difficult and condensed style.[22] As a former teacher of Wittgenstein, and in virtue of their intense discussions, Russell was probably the person at that time who knew Wittgenstein's views best.

Russell starts his introduction by surveying the sequence of subjects:

> Starting from the principles of Symbolism and the relations which are necessary between words and things in any language, it applies the result of this inquiry to various departments of traditional philosophy, showing in each case how traditional philosophy and traditional solutions arise out of ignorance of the principles of Symbolism and out of misuse of language. The logical structure of propositions and the nature of logical inference are first dealt with. Thence we pass successively to Theory of Knowledge, Principles of Physics, Ethics, and finally the Mystical [*das Mystische*].[23]

In Russell's presentation of Wittgenstein's main theses a theory of Symbolism precedes any other subject. Russell leaves it open whether Wittgenstein – after the development of the symbolic structure of elementary sentences – provides at least one example of the corresponding structure in the world. I call an interpretation of this type, which puts the burden of philosophical theory building on the symbolic side of the picture, *logicistic*.

There is an alternative interpretation. One could reject the rigorous logicistic view and concede that metaphysical assumptions contribute to the structuring of elementary sentences.[24] Instead of reflecting first about language, one starts with *metaphysical* assumptions about simple objects and their combination in a state of affairs. Using the correspondence principle of the picture theory, such a metaphysical structure must then be reflected in the symbolic forms which describe it.

According to the majority of commentators, Wittgenstein follows the strictly logicistic approach. If Wittgenstein had succeeded in doing so there would be no reason to specify the metaphysical side of the picture. He could have left it to others to apply his logical theory to specific languages and thereby decide which elementary sentences depict which structure of the world.

Let me sketch the main features of the picture theory applied to language in a few lines. All sentences either are complex or elementary, with complex sentences being truth functions of elementary sentences. A sentence is necessarily composed of symbols. By convention, symbols correspond to components in the world. The possible arrangements of such components in the world manifest possible states of affairs [*Sachverhalte*]. Whether an elementary sentence matches a state of affairs is not a question of convention, since elementary sentences are true or false by virtue of their correspondence to a state of affairs. The comparison assumes the correlation between simple names and simple objects; otherwise a sentence would be senseless. At the very heart of Wittgenstein's conception lies the theory of simple objects. What types of objects are there? Are simple objects individuals or universals? Is there at least one example of a simple object to exhibit the functioning of Wittgenstein's picture theory?

Like many others, Kenny states that Wittgenstein never provides even one example of what the corresponding structures of the symbolic forms in the world might be.

The lack of examples is not accidental. Wittgenstein believed in the existence of simple objects and atomic states of affairs not because he thought he could give instances of them, but because he thought that they must exist as the correlates in the world of the names and elementary propositions of a fully analyzed language.[25]

In this view, despite the adaptation of the Hertzian picture theory, Wittgenstein refrained from an analysis of what simple objects could be and how their configuration establishes a state of affairs. As evidence, a letter of Wittgenstein to Russell has often been pointed to, where he confesses that he has not the slightest idea of what the simple components of *thought* might look like. Since thoughts are expressed by language, it should follow that Wittgenstein could not provide an example of an elementary sentence.[26] In this letter sent from the prisoners' camp in Monte Cassino, Wittgenstein went into a detailed elaboration of Russell's interpretation of the *Tractatus.* The content of Wittgenstein's reply to Russell will be analyzed in more detail in the last section.

Wittgenstein's friend from the late forties and his later literary executor von Wright cites an anecdote about what he understood as the beginning of the picture theory.

There is a story of how the idea of language as a picture of reality occurred to Wittgenstein. It was in the autumn of 1914, on the eastern front. Wittgenstein was reading in a magazine about a lawsuit in Paris concerning an automobile accident. At the trial a miniature model of the accident was presented before the court. The model here served as a proposition; that is, as a description of a possible state of affairs.[...] It now occurred to Wittgenstein that one might reverse the analogy and say that a *proposition* serves as a model or *picture*, by virtue of a similar correspondence between its parts and the world.[27]

In a footnote to the first sentence of the quotation von Wright alludes to a different origin of the principle of correspondence.

There exist several somewhat different versions of it. The story as told here is based on an entry in Wittgenstein's philosophical notebooks in June 1930. It would be interesting to know whether Wittgenstein's conception of the proposition as a picture is connected in a way with the Introduction to Heinrich Hertz's *Die Prinzipien der Mechanik*. Wittgenstein knew this work and held it in high esteem. There are traces of the impression that it made on him both in the *Tractatus* and in his later writings.

Such a late dating of Wittgenstein's adoption of the picture theory would pose severe problems for an interpretation critical of the logicistic approach. If the picture theory had been developed after Wittgenstein's work with Russell and his reading of Frege, one could hardly maintain that it was inspired by the physical theory construction of Hertz.

The passage can be found in the recently published *Wiener Ausgabe* of Wittgenstein's *Philosophische Betrachtungen*:

Ich bin seinerzeit auf die Bildtheorie der Sprache durch eine Zeitungsnotiz gebracht worden worin gesagt war daß man in Paris bei einer Gerichtsverhandlung über ein Straßenunglück dieses Straßenunglück durch Puppen und kleine Omnibusse vorgeführt wurde. Wie unterscheidet sich nun so eine Vorführung von einem Spielen mit Puppen etc? (Natürlich durch die Bedeutung) aber worin liegt die? (Die einen würden sagen: durch seine Wirkung die allein ist seine Bedeutung.)[28]

The anecdote also is mentioned in a shorter *Notebook* entry of September 29th, 1914. Here it does not illustrate the principle of correspondence, as suggested by von Wright. Instead, the model in the lawsuit is an analogue of an experimental composition of symbols denoting objects in the world so that they succeed or fail to represent a state of affairs. The same subject is mentioned in *Tractatus* 4.031. Wittgenstein was struck by the observation that the states of affairs in the world do not determine the sense of propositions. Instead one is free to design a model (or proposition) by assembling all its components, correlate them to the objects in the world and state a possible configuration of these objects. The facts then determine the truth value of the proposition: it is true if its sense coincides with the facts. The wooden model of the car stands for the real car, small figures for the people involved in the accident and lines on the paper render the borders of the street. By an act of convention one agrees upon these correlations between the symbols and their denoted objects in the real world. Every participant in the lawsuit can easily come to agree whether the small figure with the red hat represents the policeman who happened to witness the incident. After one agreed about these conventions, it might be disputed whether the situation displayed by the model actually occurred – whether the description of the scenario is true. It is not the external world which determines the structure between elements of the model. "The picture must now in its turn cast its shadow on the world."[29]

Hence, von Wright's alleged moral of the anecdote – that Wittgenstein was inspired to the principle of correspondence as late as 1914 – is refuted. In the quotation, Wittgenstein ponders the *creation* of pictures, not their general existence. Already in the 1913 *Notes on Logic* and in his *Notebook* Wittgenstein explicitly refers to sentences as pictures of the world. The very first known philosophical theses Wittgenstein ever wrote are the Preliminaries of the *Notes on Logic*, where we find the following characterization of philosophy:

In philosophy there are no deductions; it is purely descriptive. The word "philosophy" ought always to designate something over or under, but not beside, the natural sciences. Philosophy gives no *pictures of reality*, and can neither confirm nor confute *scientific investigation*. It consists of logic and metaphysics, the former its basis.[30]

Although philosophy is characterized negatively with respect to pictures of reality, it appeals already to the Hertzian picture theory. It is only science which judges the truth of an empirical sentence as a picture of a fact. Philosophy provides the logical and metaphysical preconditions that such judgements can be made.

The characterization of philosophy goes hand in hand with a remarkable talk he gave in the Moral Science Club on November 19, 1912:

Mr. Wittgenstein read a paper entitled "What is Philosophy?" The paper lasted only about 4 minutes, thus cutting the previous record established by Mr. Tye by nearly 2 minutes. Philosophy was defined as all those primitive propositions which are assumed as true without proof by the various sciences. This definition was much discussed, but there was no general disposition to adopt it.[31]

McGuinness starts his exposition of the incident stating: "Perhaps the most significant thing here is the implicit identification of 'philosophy' with logic." Compared with what Wittgenstein writes in the Preliminary of the *Notes* the account is wrong on an interesting point. Wittgenstein identifies philosophy with logic *and metaphysics*. Neither can be refuted by scientific investigation. Philosophy provides the necessary ground for any scientific inquiry to be undertaken. Wittgenstein sets the philosophical task in contrast to, but also in relation to, scientific investigation.

Von Wright's suggestion that Wittgenstein might have been inspired exclusively by the Introduction of the *Mechanics* cannot be maintained. With his early interest in physics, Wittgenstein should have been motivated to read the main parts of Hertz's book; his studies should have enabled him to understand its scientific implications and many direct and indirect references to sophisticated concepts of the *Mechanics* show that his enthusiasm did not stop with page 41. There is a clear reference to the hidden and sophisticated concept of dynamical models in the *Mechanics*. In *Tractatus* 4.04, Wittgenstein states that the proposition should have the same number of distinguishable parts as the situation it represents: "The two must possess the same logical (mathematical) multiplicity. (Compare Hertz's *Mechanics* on dynamical models.)" Following Wittgenstein, a reader of the *Tractatus* should read §418 in the second book of the *Mechanics* to understand what Wittgenstein wants him to understand. Wittgenstein considered more Hertzian ideas than are outlined in the Introduction, but which possibly attracted his thoughts?

4.3. Wittgenstein's use of Hertzian metaphysics

In the Preface to the *Tractatus*, Russell says that Wittgenstein's work is primarily logically motivated and that the sequence of themes starts from considerations about the structures of symbolism. In fact, Russell ascribes a sequence of themes to Wittgenstein which are his own. Russell's *Principles of Mathematics* starts with the definitions of pure mathematics and continues with theorems of symbolic logic. Only in part seven of the book does Russell briefly discuss matter, motion and causality – and concludes with a brief exposition of Hertz's dynamics.[32] Russell overlooks Wittgenstein's reversal of themes. In the first theses Wittgenstein does not mention symbols at all. The opening statement defines what makes up the world: it is all that is the case. Its specification follows in the subsequent statements. Wittgenstein does not start with a definition of elementary propositions and their logical composition. Instead he introduces some metaphysical structures of the world.

In the *Notebooks*, Wittgenstein describes the analytical process of dividing complex bodies into their components in a Hertzian terminology:

The division of the body into *material points*, as we have it in physics, is nothing more than analysis into simple components.[33]

Wittgenstein embraces the mechanical analytical method of Hertz by the identification of the most simple components as material points. There are other places in the *Notebooks*, where Wittgenstein considers spots in our visual field as simple objects (18.6.15). Objects in our perception might be the second class of objects which for Wittgenstein exemplified simple objects of the *Tractatus* and he gives a clear reference to them during the lectures later in Cambridge.[34] The Hertzian heritage, however, is very strong for simple objects in the external world:

(a) Things, as simple objects in the external world, are material points.
(b) Two things stand in a relation aRb and constitute thereby matter. All other more complex constellations can be reduced to them.

4.4. The first theses of the Tractatus

The *Tractatus* starts with the following three statements:

1 The world is all that is the case.
1.1 The world is the totality of facts, not of things.
1.11 The world is determined by the facts, and by their being *all* the facts.

According to the first sentence the world is a collection of components of which one can say that they are the case. These components are facts, not things, 1.1. Why is the world not made up by the set of things which apparently populate the world? First of all, they do not unambiguously characterize the world. Knowing just the simple things in the world – material points – does not help to differentiate between worlds with the same material points in different relations to each other. Taking into account that, for Wittgenstein, simple objects are unchangeable and indestructible, our world has always had the same set of simple objects. Only the arrangements of these objects has changed. Hence the set of simple things is a cosmological constant which, once set, stays steady. According to the definition of mass it is not possible to form a proposition about one space-time location alone without reference to a standard measure to another material point as required in the definition of mass. Mass is a relational concept which requires the minimum of two space-time locations set in relation to each other. The first group of theses in the *Tractatus* introduces the metaphysical conception of things and their concatenation. The second group elaborates the nature of the concatenated things in more detail:[35]

2 What is the case – a fact – is the existence of states of affairs.
2.01 A state of affairs is a combination of objects (entities, things).
2.011 It is essential to a thing that it can be a constituent part of a state of affairs.
2.012 In logic nothing is accidental: if a thing *can* occur in a state of affairs, the possibility of the state of affairs must already be prejudged in the thing.
...
2.013 Each thing is, as it were, in a space of possible states of affairs. This space I can imagine empty, but I cannot imagine the thing without the space.

Tractatus 2 defines a fact to be the existence of states of affairs. A fact is necessarily complex with states of affairs as elementary components. States of affairs are intrinsically structured by a combination of objects. Wittgenstein distinguishes objects into *Sachen* and *Dinge* – entities and things. When we reformulate 2.01 for the first kind of objects, things, we obtain:

2.01a A state of affairs is a combination of things.

We, then, can tentatively substitute "material points" for "things" – the simple external objects:

2.01a′ A state of affairs is a combination of material points.

Material points (things) are denoted by their space-time locations. Thus, simple external objects – things – can be named in the following form:

$$\text{Material point } a = \langle x,t \rangle$$

A state of affairs composed of simple external objects, which can be described by an elementary sentence, consists of a combination of material points:

$$\text{State of affairs } aRb = \langle x_1,t_1 \rangle R \langle x_2,t_2 \rangle$$

Wittgenstein repeats on various occasions, e.g., in a letter to Ogden commenting on the English translation, that the relation sign "*R*" has no additional sense beyond what the singular expressions denote. "2.03: Here instead of 'hang one on another' it should be 'hang one in another' as the links of a chain *do*! The meaning is *that there isn't anything third* that connects the links but that the links *themselves* make connexions with one another."[36]

"*R*" is a sentence forming operator which combines two simple names to form an elementary sentence. Material points are sufficiently identified by their spatio-temporal position. Their names need only denote those locations. An elementary sentence states that the material point *a* stands in a *material* relation to the second material point *b*. Hence the relational expression just uses *numbers* for a quantitative value of mass, which is constituted by the combination of two material points. This feature is particularly elegant in Wittgenstein's theory. Because names of material points just use expressions for space and time locations, the *forms* of things are space and time – not mass![37] Their materiality is only exhibited in a combination of material objects. Only here the definition of mass is applicable. In 2.0231 Wittgenstein echoes the fact that material properties of the world are made only through the configuration of objects. In an elementary sentence this configuration is simply pictured by a number. But this number does not denote a property of the world. It is simply part of the syntax of a sentence which thereby expresses sense.

$$\text{Mass Relation } R = \frac{m_1}{m_2}$$

Furthermore, two different states of affairs are independent of each other:

5.135 There is no possible way of making an inference from the existence of one situation to the existence of another, entirely different situation.

Mechanics demands *a priori* that each point in space-time can have any range of masses. It is interesting to realize that a term $<x,t>$ denotes a material point, but that this point has not yet any properties! One might think that there is a true sentence "point x at time t has x number of mass-particles." Yet the relational definition of mass requires the comparison with another material point. Otherwise a mass would be undefined. Without reference to another material point the term $<x,t>$ is (only) a name of a material point.

4.5. Atomic propositions and simple things

A successful, i.e. true, description of the world by means of language relies on the correct depicting of states of affairs by elementary sentences, which are composed of names. Those names denote simple objects.

Tractatus 4.0311: One name stands for one thing, another for another thing, and they are combined with one another. In this way the whole group – like a tableau vivant – presents a state of affairs.

Yet, what is a thing? Is it a simple object? Did Wittgenstein know an example of a thing and how one refers to a thing? The key problem for an adequate interpretation of the *Tractatus* is the determination of the types of objects and their concatenation in a state of affairs. In a much quoted passage from the *Notebooks* Wittgenstein seems to address these difficulties and to answer them straightforwardly.

Our difficulty was that we kept on speaking of simple objects and were unable to mention a single one.

If a point in space does not exist, then its co-ordinates do not exist either, and if the co-ordinates exist then the point exists too. That is how it is in logic.

The simple sign is *essentially simple*.

[...] It always looks as if there were complex objects functioning as simples, and then also *really* simple ones, like the material points of physics, etc.

That a name stands for a complex object can be seen from the indefiniteness of the sentence in which it occurs.[38] This comes of the generality of such propositions. We *know* that not everything is yet determined by this proposition. For the generality notation *contains* a proto-picture.

All invisible masses, etc. etc. must come under the generality notation.[39]

What appears in the first sentence to be explicit evidence for Wittgenstein's lack of examples for simple objects, turns to the opposite in the following illustrations. In which sense Wittgenstein takes material points as samples (notified by "etc.") will be discussed in the last sections.

The quoted passage uses "material points" and "invisible masses" as examples for simple things. The notes in June 1915 are most densely written. They contain long argumentative passages in daily entries; apparently Wittgenstein manages to escape the war situation and he works intensely on central theses of the *Tractatus*. Many sentences which he wrote during these days were excerpted later for the first version known as *Proto-Tractatus* and the finally printed version – the *Tractatus*. On the day before the previous quotation was written, Wittgenstein asks the rhetorical question:

> Can we justly apply logic just as it stands, say in *Principia Mathematica*, straightaway to *ordinary propositions*?[40]

Whoever analyses expressions of ordinary language by means of logic must determine whether they have meaning, or – as Wittgenstein says – to take into account that "there is a possibility of failure" to express sense with complex sentences.[41]

What does it mean for a sentence to fail to express sense? An empirical sentence must refer in its fully analysed form to external objects and their relations. Only then is the truth value of the sentence determined. Hence, to judge the meaningfulness of a sentence requires the analytical decomposition of complex objects into their atomic components.

> The division of the body into material points, as we have it in physics, is nothing more than analysis into simple components.[42]

Wittgenstein emphasizes the words "material points" and "simple components." How would the meaningfulness of physical sentences be decided by a mechanical theory like that of Hertz? Sentences in ordinary language often talk about complex objects, their properties and their relations. Yet the physical language is defined for systems of small objects with atomic dimensions, for one has to reduce a physical language to expressions for atomic objects – the things in the world – which are part of the complex body. In the quotation Wittgenstein struggles with the difficulty, how to reduce propositions about the external world to physics. Even when the physical concept of material points and their kinetics is theoretically solved, it is an arduous task to explain with it the physics of complex bodies denoted by ordinary language. At least in principle, the physical behavior of a complex body can be deterministically predicted, when all its components are specified in terms of space, time and mass. This system of material points together with the conditions of the environment is then subject to the mechanical theory.

It would be practically impossible to analyse each sentence to the level of its atomic components. There are feasible shortcuts. One should not interpret Wittgenstein's principle of analysis as an idealistic dream of a logician without any possible practical bearing. A physicist treats an extended material body made out of a huge number of material points as one (idealized) material point. Typically the complexity of the object is then reduced to simple cases.

Wittgenstein's insertion "as we have it in physics" is an elucidation of that procedure: the division of bodies into material points *as defined in physics* amounts to an analysis of complex objects into simple components. Wittgenstein does not ponder the physical analysis of complex bodies as an example among other known methods of analysis. It is the other way around: all ordinary bodies of the external world are analysed in such a way that their elementary components are made out of material points as we know them in physics. Could there be a closer alliance between Wittgenstein's obvious use of a physical terminology and Hertzian physics?

4.6. Wittgenstein's simple objects

Up to now the introductory theses of the *Tractatus* have been interpreted by reformulating the Wittgensteinian notions for simple objects and elementary propositions in Hertzian terminology. Can we find in the *Tractatus* a direct reference to Hertzian material points as examples for simple objects? Many interpreters deny that Wittgenstein ever provided such an example. In the *Prototractatus* we find a surprise. There *Tractatus* 2.013 is supplemented by a thesis elaborating on the thing as simple object:

Prototractatus 2.0141	Let the thing be the material point surrounded by infinite space. It is obvious that the material point cannot be imagined without infinite space.
Prototractatus 2.01411	The point in space according to this view is a place of argument.[43]

One cannot imagine a more unambiguous statement about simple objects! Wittgenstein uses the Hertzian notion of material points and defines them as things – the simple objects of the external world. The connection between simple names and their counterparts in the world is made by convention. Only after this convention has been agreed upon are statements about states of affairs possible and either true or false. Wittgenstein precisely expresses the normative nature of the assignment of material points as being simple external objects by using the imperative form "Let the thing be the material point" [*Das Ding sei der materielle Punkt*] instead of "The thing is the material point" [*Das Ding ist ein materieller Punkt*]. When, in the discussed passage of the Notebooks, Wittgenstein speaks of things being material points *for instance*, he again emphasizes the conventional status of his statement. The *application* of logic decides which elementary sentences exist, and *a fortiori* which simple objects exist (*Tractatus* 5.557). This choice is decided by science and here Wittgenstein had a clear prototype in Hertz's *Mechanics*.

When Wittgenstein's understanding of material points as simple external objects is so obvious, how could Wittgenstein's letter to Russell 1919 often be interpreted as proving the opposite? In the postscript to that letter, which is longer than the letter itself, Wittgenstein tries to clarify Russell's questions in nine points. The first point is:

1) "What is the difference between *Tatsache* and *Sachverhalt*?" *Sachverhalt* is, what corresponds to an *Elementarsatz* if it is true. *Tatsache* is, what corresponds to the logical product of elementary props when this product is true. The reason why I introduce *Tatsache* before introducing *Sachverhalt* would want a long explanation.[44]

Facts are distinct from states of affairs because of their complexity. After the division of bodies into smallest, indivisible components, one obtains states of affairs corresponding to elementary propositions. Facts are expressed by complex sentences which are logical products of the elementary propositions. Astounding is the rudeness with which Wittgenstein refused to explain to Russell – his close friend and former teacher with best intentions to help publish his book – why the notion of a fact is introduced before that of a state of affairs. It certainly must be a long explanation, possibly with reasons not entirely shared by Russell.

There is a striking parallel to Hertz and his holistic conception of the Fundamental Law. The only empirical principle of the *Mechanics* defines the laws of motion for free systems and not for individual particles. Only when such a free system is considered can one deduce the motions of its parts using the Fundamental Law. Hertz introduces *invisible masses*, which cannot be determined directly. Their existence is hidden from direct perception. Invisible masses can only be indirectly determined by the motion of the system as a whole. Hertz had to include the concept of invisible masses in order to account for, e.g., potential energies. From the Fundamental Law the preservation of energy can be deduced. In traditional terms the movements of a pendulum can be described as a repeated transformation of kinetic to potential energy and vice versa. When a pendulum is at its highest point with no motion for a moment, all the energy is transformed to potential energy. In the Hertzian system all energy is kinetic, hence one has to assume *invisible material points* which get into motion and take over the kinetic energy of the visible pendulum (PM §605). Yet the existence of the invisible material points cannot be determined individually. Their existence is indirectly assumed by the motion of the pendulum and the Fundamental Law. Consequently, simple objects and their relations – expressed by elementary propositions – can only be determined once the complex, free system of material points, of which they are a part, is known. In analogy, Wittgenstein's analysis of sentences has to start from complexes and their corresponding components in the world: facts.

The second point in Wittgenstein's postscript to the letter to Russell is often quoted as evidence that he never pondered examples for elementary sentences.

2) "...But a *Gedanke* is a *Tatsache*: what are its constituents and components, and what is their relation to those of the pictured *Tatsache*?" I don't know *what* the constituents of a thought are but I know *that* it must have such constituents which correspond to the words of Language. Again the kind of relation of the constituents of the thought and of the pictured fact is irrelevant. It would be a matter of psychology to find out.[45]

In 2) Wittgenstein does not discuss the pictorial relation between elementary sentences and simple objects in the external world. Instead he sends Russell answers to the third block of theses in the *Tractatus*. This block opens with

Tractatus 3, "A logical picture of the facts is a thought." In September 1916 he notes about the pictorial relation between thoughts and language:

Now it is becoming clear why I thought that thinking and language were the same. For thinking is a kind of language. For a thought too is, of course, a logical picture of the proposition, and therefore it is just a kind of proposition.[46]

Thoughts can be expressed by sentences. In thinking one makes sense of a sentence. In *Tractatus* 4 Wittgenstein expresses this by "*Der Gedanke ist der sinnvolle Satz*," which clearly lets Russell wonder whether a thought consists of words. Wittgenstein negates this with answer 4 in his letter.

4) "Does a *Gedanke* consist of words?" No! But of psychical constituents that have the same sort of relation to reality as words. What those constituents are I don't know.[47]

In his letter to Russell Wittgenstein elucidated the relation between thoughts and the world. Thoughts are logical pictures of facts, but they have to have a material basis. They can be expressed in sentences, or exhibited in picture models, but they can also be "thought" in the mind by psychical complexes. "But a *Gedanke* is a *Tatsache*" – thoughts as facts are then psychical states of the mind which are unknown to Wittgenstein. Nonetheless they are models of the world like sentences which depict external states. For example, mental images of the external world can be caused through perceptions. The composition of these facts cannot be determined by a philosopher just as the determination of the atomic composition of a chair is beyond his reach. Only psychology provides theories about their composition.

Wittgenstein did not imagine what a psychological theory of thoughts would look like. Yet he had very a clear idea of simple objects and their concatenation in states in affairs. Thoughts are their logical pictures.

5. PICTURE THEORY

5.1. Pictures and models

The characterization of the picture theory in *Tractatus* 4.01 explains the relation between a proposition [*Satz*] and reality in the first sentence and between propositions and our thinking about reality.

4.01 A proposition is a picture of reality. [*Der Satz ist ein Bild der Wirklichkeit*]. A proposition is a model of reality as we imagine it. [*Der Satz ist ein Modell der Wirklichkeit, so wie wir sie uns denken*].

Why is the proposition in the second case a model of reality, different from being a picture? Wittgenstein's terminology refers to another interesting Hertzian concept.

In *Tractatus* 3 Wittgenstein defines "thought" as a logical picture of facts. Pictures are introduced in *Tractatus* 2.1 "we make to ourselves pictures of facts." It is interesting to note that a subject – we – is doing something in order to obtain pictures. Pictures require an active subject for coming into existence. Pictures themselves are facts (*Tractatus* 2.141) like Leonardo's painting of Mona Lisa which is an assortment of colour pigments on a canvas. Wittgenstein's pictures require that the pictorial elements of the painting have to have a projective connection to elements in reality.

The elements both of picture and depicted must stand in certain relations to each other. These relations are called the structure of facts, state of affairs or the picture. In the world the number of objects cannot change. Simple objects cannot be destroyed and they cannot come into existence from nowhere. What can change in the world is the configuration of things, hence its structure.

As an example one can imagine a billiard table with two balls on it. The table represents space and time of the world and the balls two things in the world. The balls can never evaporate from the table, nor can another ball enter the game. Yet the positions of the balls on the table and the corresponding relation can change with time. Such a simple relation between the balls is a simple fact: a state of affairs. The balls stand for simple objects: things.

Leonardo's painting can qualify as a Wittgensteinian picture, if each colour spot on the canvas relates to a *possible* scenery of a person in front of a landscape. The picture does not need to depict a real situation; it could be the case that Mona Lisa never existed. Nonetheless the painting is a picture as long as it depicts a *possible* fact. What is required for a fact, e.g., a collation of colour pigments on a canvas, to depict another fact?

Tractatus 2.16: In order to be a picture a fact must have something in common with what it pictures.

In *Tractatus* 2.17 Wittgenstein answers his request by stating that the common feature of picture and depicted is the *form of representation*. Such forms are introduced in *Tractatus* 2.15, where they are defined as the *possibility of structure*. The structure is, as said before, the relation in which elements stand to each other. The range of all possible relations, in which these elements relate to each other, is then the form of the fact. If that fact is a picture, the method of projection translates the structure between the elements in the picture to a structure of the elements in the world. This translated possible structure of elements is a form of representation (*Tractatus* 2.15). Hence, in *Tractatus* 2.17 Wittgenstein requires that the form of representation of a picture is the same as the possible relation between objects in the world.

5.2. *Logical form*

There can be a spatial relation between the elements of a painting, e.g., that the red dot in the left corner is 5 cm from the bright spot on Mona Lisa's nose. This is translated into a relation between the position in the depicted landscape and the assumed point of Mona Lisa's nose, which might be 500 m apart. This fact – two points being 500 m apart – is represented in the picture by two colour spots being 5 cm apart, hence through a *spatial* relation. According to 2.171 a picture can represent every reality whose form it has. One can now abstract from the type of relation between the elements in the picture (here spatial) and restrict all information to this: *that* the elements in the picture can represent a state of affairs. This possibility is the *logical form of representation*. Hence the logical form of representation just allows to state that a particular fact is or is not the case.

5.3. Dynamical models

Hertz was deeply influenced by the methodology of his teacher Helmholtz, who developed a sophisticated theory about the physiological impressions from the physical state of the external world. This theoretical background is to be considered when Hertz introduces the concept of dynamical models.

Definition. A material system is said to be a dynamical model of a second system when the connections of the first can be expressed by such coordinates as to satisfy the following conditions:
(a) That the number of coordinates of the first system is equal to the number of the second.
(b) That with a suitable arrangement of the coordinates for both systems the same equation of condition exists.
(c) That by this arrangement of the coordinates the expression for the magnitude of a displacement agrees in both systems.

Any two of the coordinates so related to one another in the two systems are called corresponding coordinates. Corresponding positions, displacements, etc., are those positions, displacements, etc., in the two systems which involve similar values to the corresponding coordinates and their changes. (PM §418)

A dynamical model is a system of material points like the system of which it is a model. The definition of a dynamical model allows isomorphic relations between a material system and the dynamical model of it. In Observation 2 about his definition Hertz regards pictures of the mind as models of the structures in the external world:

The relation of the dynamical model to the system of which it is regarded a model, is precisely the same as the relation of the images which our mind forms of things themselves. For if we regard the condition of the model as the representation of the condition of the system, then the consequents of this representation, which according to the laws of this representation must appear, are also the representation of the consequents which must proceed from the original object according to the laws of this original object. The agreement between mind and nature may therefore be likened to the agreement between two systems which are models of one another, and we can even account for this agreement by assuming that the mind is capable of making actual dynamical models of things, and working with them. (PM §428)

Once the mental picture is a dynamical model of the state of the world, a necessary congruence obtains between external structures and its images. As dynamical models these pictures preserve the relations between the objects in the world. As guaranteed by the optical laws, a projection of the external object through the human eye preserves its geometrical topology. Visual experiences are dynamical models of the combinations of objects in the external world and they can be expressed by describing our perceptions.

CONCLUSION

Hertz's conception of models is an integral part of Wittgenstein's picture theory. The material points of the *Principles of Mechanics* are paradigm cases of simple objects in the world, which by different configurations make up all possible facts of reality. With these starting points Wittgenstein understood his book as a continuation of what Hertz did not pursue: the logical and philosophical foundation for any knowledge of the world.

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NOTES

* An earlier version of this paper appeared in the *British Journal for the History of Philosophy* (Graßhoff 1997).

1 Translated in Brian McGuinness *Wittgenstein: A Life* (Berkeley: University of California Press, 1988), p. 84. Wittgenstein admired Kraus and his *Die Fackel*, and he was particularly struck by a series of articles on Kraus published in 1913. He first sent the manuscript of the *Tractatus* to Jahoda, the publisher of *Die Fackel* because he considered it a contribution to its aims (McGuinness, p. 266). After Kraus asked for support of *Der Brenner*, Wittgenstein donated much of his property to the intellectual periodical and Austrian artists. Among them is Adolf Loos, to whom Wittgenstein was introduced by Ficker. In 1914 Wittgenstein became engaged in vital discussions about Loos' controversial architecture (McGuinness, p. 209). In 1917 Wittgenstein intensively discussed Weininger with his circle of friends at Olmütz; this is reflected in the later passages of the *Tractatus* on solipsism and ethics.

2 Fania Pascal describes the climate of the confessions in Rush Rhees (ed.), *Ludwig Wittgenstein: Personal Recollections* (Oxford: Blackwell, 1981), pp. 47ff.

3 McGuinness, *op.cit.* (note 1), p. 84.

4 James Griffin, *Wittgenstein's Logical Atomism* (Oxford: Oxford University Press, 1964), p. 99. Now it is a common view among the commentators to attribute the origin of the picture theory to Hertz, e.g., Anselm Müller, *Ontologie in Wittgensteins Tractatus* (Bonn: Bouvier, 1967), Hacker (1986)), David Pears, *The False Prison* (Oxford: Oxford University Press, 1987), Donald Peterson, *Wittgenstein's Early Philosophy* (New York: Harvester, 1990).

5 G.E.M. Anscombe, *An Introduction to Wittgenstein's Tractatus* (London: Hutchinson, 1959), p. 12.

6 McGuinness, *op.cit.* (note 1).

7 I.M. Fasol-Boltzmann, *Ludwig Boltzmann: Principien der Naturfilosofie* (Berlin: Springer, 1990), pp. 36f.

8 Cited by Fasol-Boltzmann, *op.cit.* (note 7), p. 37.

9 *Ibid.*, p. 39.

10 *Ibid.*, p. 38.

11 McGuinness *op.cit.* (note 1), p. 55.

12 Peter Geach quoted in *ibid.*, pp. 83f.

13 Brian McGuinness and Rudolf Haller (eds.), *Wittgenstein in Focus* (Amsterdam: Rodopi, 1989), pp. 5–33.

14 "Notes on Logic" in Ludwig Wittgenstein, *Notebooks 1914–1916* (Oxford: Blackwell, 1961), p. 93.

15 McGuinness and Haller, *op.cit.* (note 13), pp. 19–20.

16 Leave aside the fact that Newton defined masses in terms of the density of particles.

[17] It remains to be discussed whether the theory can be falsified and why it was soon superseded by modern developments.
[18] Isaac Newton, *Philosophiae Naturalis Principia Mathematica* (Glasgow: Duncan, 1822), p. 16.
[19] Hertz describes it slightly differently; a detail which should not obscure the general structure of his theory.
[20] With the exception of the letter to Russell from January 1914, in which he asks him to imagine a world with "a single *Massenteilchen*" for reflections on relative space and time.
[21] Ludwig Wittgenstein, *Letters to C.K. Ogden* (Oxford: Blackwell, 1973), p. 35.
[22] McGuinness *op.cit.* (note 1), p. 290.
[23] Ludwig Wittgenstein, *Tractatus Logico-Philosophicus* 2nd ed. (London: Routledge and Kegan, 1933), p. 7.
[24] This can be done without presupposing the untenable thesis that all logical forms follow from metaphysical assumptions.
[25] Anthony Kenny, *Wittgenstein* (London: Allen Lane, 1973), p. 74.
[26] *Ibid.*, p. 58.
[27] Georg Henrik von Wright, *Wittgenstein* (Minneapolis: University of Minnesota Press, 1974), p. 20f.
[28] Ludwig Wittgenstein, *Wiener Ausgabe: Philosophische Betrachtungen, Philosophische Bemerkungen*, vol. 2 (Wien: Springer, 1994), p. 279.
[29] Wittgenstein, *op.cit.* (note 14), p. 27 (6.11.1914).
[30] *Ibid*, p. 93, emphasis by the author. This quotation is from the version which Costello got from Russell in 1914 and which was published in the first edition of the *Notes*. It was replaced in the second edition by an earlier version of the text. There "the Preliminary" relates to passages in the fourth manuscript.
[31] McGuinness, *op.cit.* (note 1), pp. 143f.
[32] Russell 1938, chapter 59.
[33] Wittgenstein, *op.cit.* (note 14), p. 67.
[34] Ludwig Wittgenstein, *Vorlesungen 1930–1935* (Frankfurt: Suhrkamp, 1989), p. 139.
[35] Ogden and Ramsey render "*Sachverhalt*" with "atomic fact." Again I don't agree with the translation by Pears/McGuinness. They translate 2.01 as "A state of affairs (a state of things) is a combination of objects (things)." The German reads *Der Sachverhalt ist eine Verbindung von Gegenständen (Sachen, Dingen)*. Nothing is said there about a nonsensical "state of things." The difficulty here, of course is the translation of "*Sachen*" in contrast to "things." Ogden translates it as "entities" to obtain an affinity to things as kinds of objects denoted by singular terms. In ordinary language "*Sache*" is often used synonymously for "*Ding*". Wittgenstein divides the class of objects which form atomic facts or state of affairs into two parts: those of things (*Dinge*) and those of *Sachen*. It is very implausible to assume that Wittgenstein tried to clarify the meaning of the technical term "object" by common language synonyms. As we shall see, things are external objects. It is not clear what *Sachen* are.
[36] Wittgenstein, *op.cit.* (note 21), p. 23.
[37] Cf. 2.0251. Colour is the only other form of simple objects, which is not mentioned in the *Proto-Tractatus*.
[38] My translation.
[39] *Notebook* entry of June 21st, 1915, Wittgenstein, *op.cit.* (note 14), pp. 68f.
[40] *Ibid.*, p. 66.
[41] *Ibid.*, p. 67.
[42] *Ibid.*
[43] Ludwig Wittgenstein, *Proto-Tractatus* (Ithaca: Cornell University Press, 1971), my translation.
[44] Wittgenstein, *op.cit.* (note 14), pp. 129f.
[45] *Ibid.*
[46] Notebook entry of September 12th, 1916, Wittgenstein, *op. cit.* (note 14), p. 82.
[47] *Ibid.*, pp. 129f.

JED Z. BUCHWALD

REFLECTIONS ON HERTZ AND THE HERTZIAN DIPOLE

It's one of those rare times in physics when you discover a really new effect. It makes you feel kind of strange – you're seeing something that nobody else has ever seen before. (Physics graduate student Marc-Olivier Mewes's reaction on realizing that his group had succeeded in creating the first atom laser.)[1]

Heinrich Hertz has for some time attracted the attention of philosophers of science who are interested in the impact of his highly abstract *Principles of Mechanics.* Yet he has not until recently been much investigated by historians of physics, who, in considering electrodynamics, have for the most part concentrated on figures such as Kelvin, Maxwell, or Lorentz. There is a nice symmetry between the philosophers' interest and the historians' lack of it, because both interests exhibit a long-standing concern with figures who were deeply engaged in the production of new theories or who developed influential abstractions. Hertz himself never did produce a theoretical system comparable to Maxwell's or to Lorentz's, but he did generate an elaborate scheme for the foundations of mechanics that had a substantial impact on foundational thinking in late 19th and early 20th century philosophy.

One might ask who at the time would have taken the trouble to read the *Principles* if they had been written by an obscure German physicist with little previous work to his credit? It is of course dangerous to speculate about what might have been, but in the light of contemporary reaction to the *Principles* it seems probable that they were so widely discussed precisely because Heinrich Hertz, the discoverer of electric waves and heir-apparent to the doyen of German physics, Hermann von Helmholtz, was their author. "Anything written by Hertz" the Irish physicist George Francis FitzGerald remarked in the very first sentence of his 1896 review of Hertz's *Miscellaneous Papers*, "is of interest" (FitzGerald 1896, 6). It is hardly likely that FitzGerald's opinion on this point was unique. Yet why was this so? Why did Hertz's contemporaries consider his physics to be so interesting, if in fact he, unlike, e.g., his teacher Hermann von Helmholtz, had not produced major theoretical innovations?

One might after all argue that Heinrich Hertz was at best engaged in confirming the existence of something, namely electric waves, that had long been thought to exist, and that his theoretical work amounted to the presentation of Maxwell's field theory to a new audience with a few changes introduced primarily to avoid questions that Hertz did not deem significant (such as the qualities of the ether or the nature of charge). What is so wrong with this picture that the last half decade has seen the publication of several books on Hertz, including my own, as well as numerous articles? Is it merely that the higher ground that had been occupied for so long

D. Baird et al. (eds.), Heinrich Hertz: Classical Physicist, Modern Philosopher, 269–280.

by people like Maxwell and Lorentz has been cleared, so that less influential figures, such as Hertz, are being turned to for lack of more interesting subjects? Or is it perhaps that contemporary historiographic fashion, which emphasizes the significance of less well-known figures for reconstituting the practice of an era, has at last brought Hertz to the center of historians' attention?

One cannot easily quarrel with the claim that Hertz did not produce a major system of his own, that many of his contemporaries outside of Germany (and even within it) did think that he had confirmed something which others had predicted, and that his own excursions into electrodynamic theory seem in retrospect to have been consolidations rather than innovations. Nevertheless, our interest in Hertz is not at all misplaced, nor are the assessments of his contemporaries surprising, once we recognize that the electromagnetic world of the early 1900s was produced by people who worked within an instrumental universe that Hertz himself had created in the laboratory and on paper in the years from 1887 through 1890.

To put Hertz into proper perspective, it is essential first to recognize that before his creation in 1887 of the dipole oscillator and resonator no one, including most British Maxwellians, had any clear idea of how artificially to produce freely-propagating electric waves. In Britain, optical radiation constituted the only known instance of these sorts of waves, and, therefore, they were generally associated with optical instrumentalities. Furthermore, until the mid-1880s at least some British Maxwellians, in particular FitzGerald, did not even think it possible to generate such waves at all by means of electromagnetic devices. FitzGerald eventually changed his mind about this, but other views militated against any Maxwellian conceiving of a suitable way to generate sufficient power for electric waves that detach themselves from the radiating object to be detectable.

We might with justification assert that before the mid-1880s no one, whether Maxwellian or otherwise, had any clear notion that electric waves in air could be manufactured by means of the sorts of devices that might be found or made in the typical laboratory of the day. In Berlin, where Hertz learned the technical practice of electromagnetics as Helmholtz's apprentice, the situation was in one major respect even more obscure than it was in Britain, since the depths of Maxwell's field theory remained unplumbed by nearly all German physicists, who otherwise differed greatly from one another. Helmholtz had himself produced a scheme that could yield waves in structures that were capable of electric polarization, but neither he nor anyone else in Germany considered whether or, better, how this might be done artificially. Instead, Helmholtz, like his British contemporaries, evidently considered optical radiation to be the paradigm for, and perhaps the only proper instance of, electric waves, except for processes that are confined to or on conducting media. Moreover, the hypotheses that (on Helmholtz's system) yielded electric radiation in non-conducting media raised questions that did not have altogether straightforward answers during Hertz's Berlin years. Indeed, Helmholtz tried to convince his young apprentice to devote himself to their experimental elucidation.

Hertz's route to the production of artificial electric waves was, not surprisingly, hardly straightforward. Indeed, even after he became convinced that he had observed propagation he did not at first imagine that he had also produced waves in the fullest sense of the term. That is, he did not initially think that what he had produced constituted a particular instance of a well-known natural kind (namely optical waves), with all of the latter's inherent properties, albeit ones that could be accessed only through devices that were foreign to optical practice (such as wire grids, or huge, opaque prisms of pitch). He did soon come to this conclusion, but he then focused his technical discussion on the new form of radiation *per se* and not at all on the entities and processes that produced the radiation in the first place. It is here that Hertz's laboratory experience merged synergistically with his considerable skills in analysis to produce a novel system that did have a substantial influence at the time and not merely among physicists. For Hertz's analysis of dipole radiation presented the new, and intriguing, case of an elaborate mathematical theory for an effect which is produced by a laboratory object that itself eludes theory's grasp. On the one hand the dipole was a real entity, a construction of metal, that Hertz worked with in the laboratory in order to produce an appropriate effect. On the other hand, it was an abstract, paper object that did not appear at all in the equations that Hertz had built to analyze the effect.

1. THE ABSENT DIPOLE

Physical schemes often live in and through schematic images. Think for example of Newton's diagrams in the *Principia*, whose lines are drawn to exemplify the concepts of force and motion with which he worked to generate a new paper world. Or consider diagrams of Augustin Fresnel's wave surfaces for crystals, which to several generations in the 19th century embodied the essential properties of optical radiation. In both of these instances there is something missing from the image, something that must be absent in order for the image to convey an appropriate physics. In Newton's case, centers of force are not present as physical entities; they are in effect simply points. Fresnel's optical surfaces likewise have no physical origin. They emerge, like Newton's forces, from diagrammatic points. In both cases the diagram warns the viewer away from the unimportant, just as much as it attracts the viewer's attention to the important. Do not wonder about force centers, Newton's diagrams implicitly warn; they are simply the loci from which distances are measured in calculating forces. Do not ponder the origin of light, Fresnel's diagrams warn; think only about how light behaves after it is born in a mathematical point. What is enjoined can be made apparent by its absence from a canonical drawing, which may accordingly serve as an exemplification of a system. In both of these cases the analytical system gains considerable power from what it forbids or ignores.

In 1888 Hertz drew an influential series of diagrams to accompany his 1889 paper on dipole radiation, which was translated as "The Forces of Electric Oscillations, Treated According to Maxwell's Theory" (EW 137–159). Hertz took a great deal of care with these drawings. They have been reproduced innumerable times since their first appearance, often directly from Hertz's originals, though also from redone computations as well. The original drawings, which are reproduced

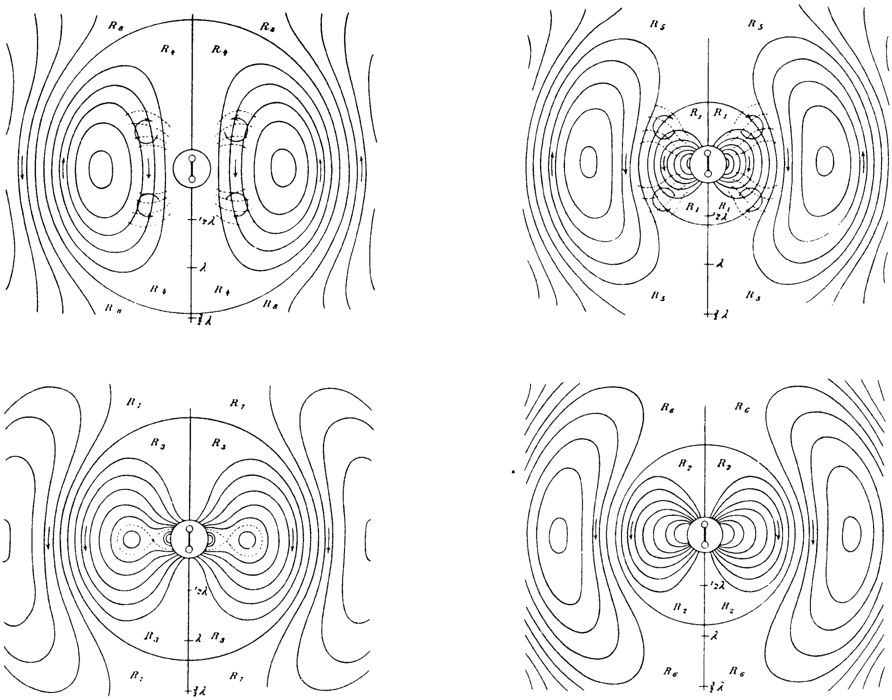

Figure 1. Hertz's 1989 diagrams of dipole radiation

here, contain three kinds of curved lines. There is the innermost dumbbell. It is immediately surrounded by a circle of variable size, on whose surface sometimes lie the termini of a sequence of nested curves. These are in turn surrounded by another, much larger circle, and past it exist nested sets of closed, distorted ovals. The terminated lines, as well as the ovals, represent Hertz's physical reality, the field. The large circle sets a boundary: it demarcates the outer, radiation-containing regions of space from the inner, non-radiation containing regions. These lines – ovals and larger circle – accordingly refer to processes that either have immediate physical reality for Hertz, or that delineate one type of process-containing region from another. They embody or differentiate the positive essence of the Hertzian radiation field. The innermost, small circle is different. Like its larger sibling, this circle also demarcates a region. But the smaller region does not contain a known physical entity. It contains instead a small drawing that represents what cannot profitably be investigated, namely the very device that produces the propagating field in the first place, Hertz's oscillating dipole.

Although Hertz's field lines are constructions that he infers from his theoretical system, whereas the oscillating dipole is a material object, nevertheless in Hertz's diagram the material object remains unknown, whereas the inferred field is known. This diagrammatic inversion encapsulates the originality and power of Hertz's physics. Because Hertz ignored the physical character of the object that produced his radiation – because he boxed it in with a mental quarantine against asking questions about it – he was able to make progress where his British contemporaries had not been able to do so. They had concentrated closely on the shapes of radiating bodies, for to the British the canonical instance of electric radiation was what was later termed wave-guidance, in which radiation does not depart from the conducting boundary but, as it were, slips over the surface. For the British the geometry of the surface was critical in building a theory, and situations that eluded analysis of this sort (such as isolated conductors that yield up their energy to far-distant surroundings) were not thoroughly probed (at least in connection with radiative processes). Furthermore, British analysts already thought that an object like Hertz's dipole would reach electric equilibrium so rapidly that the radiation it emitted would simply flash away in an essentially undetectable burst. It is therefore not at all surprising that Maxwellian reaction to Hertz's experiments centered principally on his detecting resonator, and not on his (mathematically intractable) oscillator. Even today the oscillator remains an alien presence. One well-known text, Charles Papas' *Theory of Electromagnetic Wave Propagation* for example, notes that "the determination of the antenna current is a boundary-value problem of considerable complexity", and proceeds to develop the circumstances under which the problem can be bypassed.[2]

Hertz, who knew nothing about such things, did not think at all about the surface behavior of his oscillating dipole. Nor did he consider the effects that it produces to be beyond the reach of analysis or experiment. For him the paper analog of the material dipole was in itself a nuisance, and he immediately reduced it to a pictogram. The very object that enabled Hertz to investigate electric waves does not exist at all in the mathematical account that he himself developed for its field. The effects of this removal of the experimental object were far-reaching and can be followed through the literature of physics and electrical engineering during the next half-century at least. Hertz's missing dipole evolved (as objects) into the antennae of an emerging technological regime; and they evolved (as symbols) into the unknown entities that were responsible for natural radiation, in particular Max Planck's resonators.

In 1890 Hertz published two papers on the fundamental equations of electromagnetics (EW 195–240 and 241–268). They were widely read in Germany and elsewhere during the few years that remained to him. Many contemporary references indicate that these articles had a deep impact on German physicists, which is hardly surprising since Hertz here introduced many of his German contemporaries to the broad range of electromagnetic processes from the viewpoint of field theory. However, he had already presented the field equations in conjunction with their solutions for the dipole in 1889 (EW 137–159). Whereas the 1890 articles contained no diagrams of any kind, the 1889 piece contained several, including the sequence of field maps. In the immediate aftermath of Hertz's discovery, this article was frequently used as a basis for understanding Hertz's work and indeed for developing a pragmatic understanding of a new scientific object, the radiation field.

In this influential, eventually canonical, presentation Hertz abstracted completely from the dipole itself. Instead of considering it to be a physical object, he removed it from his analysis and represented it by the product of a "quantity of electricity", *E*, and a "length", *l*. This product multiplies a fraction that contains a sinusoidal wave in the numerator (sin(*mr-nt*)), and, in the denominator, the distance *r*. Hertz then shows that this function works as a solution to a special form of his "Maxwell equations".

For more than a half-century before Hertz the field of a permanent (static) dipole, magnetic or electric, had been calculated directly from an object consisting of two equal but oppositely-charged electric (or magnetic) point masses located a given distance apart. The resulting expressions contain a vector that represents the electric (or magnetic) moment of the object, defined as the product of the magnitude of the charge by the distance between the charges. Hertz's function does contain a similar product (*El*), and, in fact, that product reduces to the static dipole if, in his solution, the ratio *n/m* vanishes (which corresponds to a zero velocity of propagation for the waveform). One might therefore think that Hertz had merely replaced a static dipole (*El*) with a non-static one (*El* sin(*mr-nt*)), otherwise retaining the form of the static solution. This would not in itself constitute a deduction of an appropriate solution from the physical characteristics of the oscillating dipole, but it would at least be a reasonable analogical move to make.

However, the product *El*sin(*mr-nt*) cannot represent a non-static dipole, because it represents a propagation. In fact, Hertz's product *El* has little physical significance for him because the "Maxwell equations" that he used in 1889 do not contain source terms at all: they apply only to free space. In order to give the dipole a clear analytical presence, Hertz would have had to introduce it as an oscillating current source into his equations, presumably in some form such as *El*cos(*t*). Had he done so, he would have been faced with a thorny mathematical situation that defies easy solution, and that, in later years, was dealt with in several, quite difficult ways (often through the explicit introduction of retarded solutions to the fundamental equations).

Hertz's own approach remained quite common through the mid-1890s, appearing for example in Paul Drude's *Physik des Aethers* (Drude 1894a). The second volume of Henri Poincaré's *Électricité et Optique* takes a slightly different tack, in that Poincaré does explicitly introduce an oscillating dipole as a source (Poincaré 1891a). Yet here, too, Poincaré proceeds rather by demonstrating the adequacy of an assumed solution to satisfy a set of equations than by the explicit construction of a solution (and Poincaré's presumed solutions, which have certain peculiarities that make their relation to the dipole less than transparent, did not pass subsequently into the literature, whereas Hertz's did).

Hertz himself pointed out only that his assumed solution "corresponds" (in the region that was later termed the near field) to an oscillating electrostatic dipole, and to an oscillating current as well. There is a powerful sense in which Hertz's critical distance from the dipole proper passed eventually into engineering practice, where attitudes among specialists in the design and analysis of antennae have usually been strikingly similar to Hertz's own views of the dipole. Consider for example the following passage by H. Bremmer of the Philips Research Laboratory:

Our first question now is, to what kind of idealized model a radio transmitter, in its most simplified form, does answer. This ideal transmitter may be represented by a line element *L* through which passes a current Ie^{-it} ... the length L of the aerial being infinitely small and the amplitude of the current infinitely great, in such a way that the product IL (the 'moment') has a finite value. In practice such a transmitter already resembles a real one whose dimensions are small with respect to the wavelength. Such a source of electromagnetic waves was studied for the first time by Hertz, and it is therefore called the Hertzian dipole. An actual antenna, of finite length and carrying a current not necessarily uniform, may be regarded as a superposition of such dipoles.[3]

Bremmer in fact followed Hertz's own presentation quite closely, and he was not alone in doing so among antenna engineers, though by the 1940s physicists normally approached the problem through retarded fields.

In the 1890s, before antenna engineers had come into being, Hertz's dipole constituted a new kind of scientific object, one that was at once conspicuously absent from the analytical structure of the effect that it produces, and that was nevertheless physically present as an actual device in the laboratory. Among physicists the dipole never did become an object of intrinsic interest or significance because it did not, from their point of view, produce something altogether novel; it just generated, as it were, a kind of artificial light. Nevertheless for physicists the dipole did serve as a useful tool, as a canonical source for electromagnetic radiation, and it was often inserted without much discussion into radiation calculations during the 1890s and early 1900s. For the evolving coterie of radio engineers during these years, the dipole constituted the sole material method for manipulating the new (and entirely artificial) electromagnetic spectrum. As such it was essential as a technological object, but it remained a tool that was to be used for the effect that it produced, and not itself an object of analysis.

Only Heinrich Hertz was likely to have produced such a multivalent device, because only he among all his contemporaries had combined Helmholtz's approach to physics with superb laboratory acumen and analytical finesse, all mixed finely and potently with an intense desire for professional recognition. From Helmholtz Hertz learned to watch for novel interactions between objects in the laboratory without worrying overmuch about the hidden processes that account for the object's effect-producing power. His dipole and detecting resonator evolved out of attempts to investigate interactions of that sort. Neither device required or attracted analysis from Hertz, because he had learned from Helmholtz to probe rather the character of the interaction between the devices than their inherent, perhaps deeply hidden, structure. British Maxwellians worried intensely about what occurred at the surfaces of conductors set free to achieve electric equilibrium. Most German physicists, gripped by the ethos of exact measurement, would not have dealt so cavalierly as Hertz with the numbers his experiments produced. Other Germans, convinced that conductors were mere containers for hidden, active entities, would have been much more concerned than Hertz was to understand as rapidly as possible the processes that must take place within the dipole and resonator proper.

2. MECHANICS AND ELECTRODYNAMICS

Many of the articles in the present collection are not directly concerned with the universe of wires, induction coils, dischargers, capacitors and batteries that populated Hertz's laboratory. They discuss instead Hertz's *Mechanics*, including both its technical structure and its extraordinarily influential conception of *Bild* – Hertz's belief that the connection between a scientific system and its natural referent has much the same character as that between signifier and signified. The universe of Hertz's *Mechanics* was an abstract world, far removed from the laboratory, and indeed one that he was investigating not in order to produce knowledge of new effects (which was ever his aim in experiment) but rather to achieve as great a consistency and clarity as possible among the signs and their connections in mechanics.

Nevertheless, an intriguing similarity links Hertz's *Mechanics* to his dipole. One might even say that Hertz's analytically-absent dipole functioned in respect to the physical reality of the electromagnetic field rather as the second-order *material particles* of his mechanics functioned in respect to his first-order *material points*, which are collections of material particles. The purpose of the material particles in the *Mechanics*, Lützen suggests, was to justify (on Euclidean grounds) the Riemannian metric that enabled Hertz to produce a satisfyingly coherent system. Similarly, the purpose of the dipole in Hertz's electrodynamics was to justify the solution that he had developed for his field equations.

The material particles of Hertz's *Mechanics* came in two varieties: those that populated the phenomenal world (or, at least, its analog in Hertz's *Bild*), and those that, though linked by rigid connections to their siblings, were not themselves directly accessible to experiment but were known only through their effects. Hertz's dipole did not, properly speaking, come in varieties, but it did have a dual character. Like the accessible material particles of the *Mechanics*, it was an object of experience. Yet it was also inimical to analysis – just as the concealed particles of the *Mechanics* also escape analysis. Of course, the dipole is a single entity that has a two-fold character and not (like the particles) a single type with two distinct sub-kinds. Nevertheless, the multiple valences between Hertz's dipole and his particles are sufficiently striking to suggest a degree of commonality between them.

3. FIELDS WITHOUT INTERACTIONS

That commonality may itself reproduce a pattern that characterized Hertz's work as his electrodynamic researches evolved beyond Helmholtz's physical conception of lab-oratory objects. Hertz absorbed from his mentor the notion that proper and effective physical theories are built on the basis of potential functions that represent the interaction at a given moment in time between two physical objects. Such a potential can be a function solely of the distance between the objects and the states that they are in at that specific moment. In order to determine how the objects behave, the potential function must be subjected to a virtual change. If the change involves solely the spatial coordinates of the interacting objects, then the variation will yield expressions that determine their accelerations, i.e. bodily forces. If the change involves only time, then the resulting expressions determine the (temporal) rates at which the object states themselves change (e.g., electromotive forces). In either case "force" becomes a shorthand way of referring to a function that emerges from a variational calculation that is performed on the potential, which alone, and altogether, embodies the interaction between the objects.

It is almost certainly the case that Helmholtz himself did not think of these functions as inherently fundamental, although it is difficult to be certain about this given the fact that he did not explicitly discuss the point. In the case of electrodynamics, which is the only case for which he worked out an appropriate function, Helmholtz probably thought the potential to derive from some sort of kinetic process that has its seat in the ether (particularly given that the electrodynamic potential itself behaves like a kinetic and not like a potential energy). Nevertheless, in practical terms – in the posing and the solution of problems – Helmholtz treated the potential as an unreduced entity, and he derived all of the acting forces directly and exclusively from it. Anything beyond that involved speculation, and might even lead to the dangerous territory inhabited by such things as Wilhelm Weber's electric atoms. To a student like Hertz, who sought to make his own the latest work in Berlin physics, Helmholtz's way of working – which is essentially what Michael Heidelberger has termed Helmholtz's experimental interactionism, with its reserved attitude towards ultimate causes – would have appeared to be not merely an efficacious method for making progress but a philosophy for doing science.

During the 1880s Hertz did do physics as Helmholtz prescribed, and he seems to have taken that prescription to be a fundamental one. If we consider in broad view Hertz's successive forays into electric circuitry, elasticity, evaporation, and cathode rays in the early 1880s we find a common pattern. Each of the first three forays works in model Helmholtzian fashion by constructing (in the laboratory or on paper) sets of objects in specific states, and proceeds by varying the object states or distances (either by calculation or by experimentation). In his work with circuitry in 1878 and 1879, Hertz acted essentially as a neophyte Helmholtzian, perturbing coupled circuit elements to acquire the information he was looking for (Misc 1–34 and 137–145). A year or so later Hertz was pursuing a thorny question involving an attempt on his part to connect a body's elastic properties to its "hardness". Here, too, Hertz worked in a thoroughly Helmholtzian manner by concentrating on a pair of interacting (in fact colliding) objects. In addition, though, Hertz was probing for the possible existence of an entirely novel state, a body's intrinsic "hardness", which none before him had conjectured. There was no attempt on his part to produce "hardness" out of more fundamental (to say nothing of hidden) processes. On the contrary, he assimilated it entirely to what was later termed the set of a body under deformation and built his account directly on this quintessentially phenomenological effect (Misc 163–183). In all of these areas Hertz was tilling Helmholtzian fields, since he had not gone beyond questions concerning the existence and properties of the states of a pair of interacting, and controllable, objects that existed as such on his laboratory workbench.

When Hertz began working intensely with the extremely rapid oscillations in wires that eventually led him to his experiments with electric waves in air, he initially conceived of wire-wire interactions on this same Helmholtzian pattern: wires in particular electrodynamic states simply interact directly with other wires in similar states. When he turned to the dipole oscillator proper, Hertz still did not think about it in an essentially novel way, not even when his experiments indicated that the interaction between it and other electrodynamic objects might be propagated in time. For Hertz in late 1887, the dipole still constituted a Helmholtzian object.

However, Hertz decided by the spring of 1888 that the dipole could not be treated simply as an object whose interaction with other entities is delayed in time. On the contrary, he was by then convinced that his experimental data required a radically different interpretation, one that admitted the active role of a third entity as a mediator. The interaction between laboratory object *A* and laboratory object *B* was not delayed at all because, properly speaking, it simply did not exist. Instead, each of *A* and *B* must be thought to interact directly only with a third object, the ether, whose state is entirely specified by the electromagnetic field, and which itself is both ubiquitous and unchangeable. Unlike the laboratory objects, the ether in Hertz's conception has no manipulable properties whatsoever, for its qualities remain invariant (though, of course, its state varies).

It is important to understand that this conception differed considerably from one which was advanced as a possibility by Helmholtz himself, and according to which the ether itself behaves like a laboratory object. In such a scheme the ether would modify the apparent interaction between laboratory objects *A* and *B* by working separately on each of them, while *A* and *B* would continue to interact directly and immediately with one another. Hertz was quite familiar with this possibility from Helmholtz's work, and he clearly did not like it, since in 1884 he had produced his version of Maxwell's equations without using the ether at all. Hertz, one might say, wished in 1884 to remove the ether, even if Maxwell's equations were to be admitted, in order to avoid working with an entity that behaved like a laboratory object but that could not itself be directly manipulated (Misc 273–290).

Field theory, as developed in Britain, differed fundamentally from Helmholtz's image of nature. For Helmholtz, the world was filled with interacting objects, among which was the ether itself. Although the Helmholtzian ether was ubiquitous, it was nevertheless in principle an object like any other, with its own states and properties. Among British field theorists, the image of nature was very nearly the reverse of this one, because, strictly speaking, there were no interacting objects at all for them. Instead, the ether's properties might vary from point to point as a result of the local presence of matter, and whatever effects the material objects evinced reflected these local ether states. As a Helmholtzian, Hertz thought of the ether as an object, but he was apparently uncomfortable with its hidden character and wished to avoid introducing it as an object like all others. He wanted, that is, to remain entirely with laboratory objects proper.

In seeking to understand how field theory might be possible, Hertz in 1884 developed a novel way to multiply interactions between laboratory objects proper, thereby yielding Maxwell's equations, but not field theory itself, because the objects (sources) remained critical conceptual elements in this early analysis of his. His route to Maxwell's equations at the time accordingly required an understanding that mixed field theory's refusal to grant sources (material objects) any active role whatsoever in electrodynamics (a belief that is partially reflected in Hertz's 1884 statement that all forms of electric force have the same qualities whatever their physical sources might be) with Helmholtz's interaction potential, which required sources to be directly active entities (Misc 274). Helmholtzian objects, that is, remained critical elements in Hertz's 1884 deduction, but they had there already been deprived of their character as proper sources through Hertz's principle that electromagnetic forces did not bear the imprint of the object that exerted them. Although Hertz put these vexing issues aside for several years, he had in fact already encountered an evocative situation in an altogether different physical regime that bears a striking resemblance to the conceptual solution he advanced in 1888 to the conundrum posed in 1884 by Hertz's unstable mixing of Maxwellian with Helmholtzian elements. That solution will, finally, bring us back to his oscillating dipole.

In the spring of 1882 Hertz had begun experiments on evaporation. Here the standard Helmholtzian pattern is again apparent, but with an interesting difference. Hertz's previous experimental work had concerned unmediated interactions – e.g., an inductor acting directly on another inductor, or two bodies interacting through collision. In his experiments on evaporation, Hertz was again examining an interaction between two objects in particular states, this time between a pair of evaporating surfaces which are surrounded by a common enclosure. Here, however, the interaction is mediated by the evaporate that exists between the two surfaces, with each surface interacting directly only with the neighboring evaporate, and the system as a whole consisting of three entities. But, of these three, Hertz worked only on the two evaporating surfaces, and not on the evaporate itself, which in his experiment acts solely as a mediator between the thermal states of the surfaces, which alone control the system. The two surfaces themselves do not directly interact with one another at all. Moreover, the behavior of each of the surfaces is specified in relation to the mediating substance rather than in respect to each other. At the same time, the properties of the mediator (though not its state) remain in the background (Misc 186–200).

The understanding of electromagnetic radiation that Hertz developed in the spring of 1888 solves the conundrum of 1884 by insisting on the continuing role of the source, but dropping altogether its relation to other sources. Its behavior is instead specified in respect to a mediating entity, namely the ether, whose state in the immediate neighborhood of the source is determined by the source's activity. The Hertzian ether functions in this respect precisely like his mediating evaporate of 1882, with electromagnetic sources replacing the evaporating surfaces. Here there could be no question of a direct connection between sources. Nevertheless, and quite unlike Maxwellian field theory, in Hertz's scheme the source continues to exist as an entity in and of itself, since it is responsible for activating the processes that take place in the field. Where the Maxwellian source in effect merely represents a locus where ether properties change rapidly, the Hertzian source is responsible for activating specific states in an entity (the ether) whose qualities – but not whose states – remain forever the same. On the other hand, the source was not of any more direct interest to Hertz than it was to Maxwellians, except as an emitter or a receiver, because physical activities of note were thought to occur only in the ether itself. Just as Hertz had not in 1882 provided a detailed theory for the behavior of his evaporating surfaces, so in and after 1888 he altogether avoided providing a theory for the dipole oscillator.

4. THE HERTZIAN OBJECT

Like the modern physics graduate student whose remarks were quoted at the beginning of this paper, Hertz was deeply affected by the experience of finding something that no one before him had probed. "It is really at this point", he wrote his parents in March 1888, "that the pleasure of research begins, when one is, so to speak, alone with nature and no longer worries about human opinions, views, and demands" (MLD 255). His training under Helmholtz had put a tremendous emphasis on the detection and probing of novel effects; with electric waves he had succeeded in a measure beyond what Helmholtz or he himself had ever envisioned. His success cannot be separated from the characteristics of his training, background and personality, which palpably influenced as well the specific character of his electromagnetic theory. Hertz's mature physics was certainly not Maxwellian, because it retained sources and refused to play with ether qualities. Neither was it Helmholtzian, because it was not based on interactions between phenomenal objects (whether instantaneous or even delayed), but rather on the fields each such object engendered in the (invariant) ether. Hertz's novel physics emerged out of his creative engagement with Helmholtzian tools, concepts and techniques in the light of his experience in the laboratory. His physics envisioned a world of phenomenal objects that determine local states in an otherwise inaccessible entity, the field-bearing ether. The Hertzian object retained much of the character of its Helmholtzian forebears, since it was not fruitfully to be reduced to hidden structures. But it was not bound in perpetual and immediate connection with other objects.

This character of Hertz's physics distinguishes it quite markedly from H.A. Lorentz's electrodynamics, which began to emerge in detailed form in 1892 (Lorentz 1937a). Lorentz of course worked with microphysical entities, whereas Hertz did not, and this constitutes an immediate and obvious difference between them. There is however another difference, one that runs deeper. Lorentz based his electrodynamics on the proposition that the interaction of entity A with entity B is delayed in time – that a change in the state of A at a specific moment occasions an interaction with B at a later time. This kind of physics deploys the fundamental image of interacting objects, albeit microphysical ones. Hertz's physics does not deploy anything like this image, for it embodied a method for building theories that permitted, indeed that impelled, distance both from the object itself and from its connections with other objects. Hertz's premature death in 1894 ended his own development of this sort of physics, but its impact on others was enduring, both for the specific methods he introduced in dealing with electromagnetic radiation, and for the example of how to build a physical theory for certain effects without analyzing in detail the object that produces them.

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NOTES

1 *MIT Tech Talk* (January 29, 1997).

2 Charles Papas, *Theory of Electromagnetic Wave Propagation* (New York: Dover, 1988).

3 H. Bremmer, *Terrestrial Radio Waves: Theory of Propagation* (Amsterdam: Elsevier, 1949), p. 14.

ALFRED NORDMANN

HEINRICH HERTZ – A BIBLIOGRAPHY

The following bibliography is comprehensive but by no means complete.[1] With the exception of selected works on and by Hertz's teacher Helmholtz, each item in the bibliography relates directly, though not always extensively to Hertz's work. A key is provided to help users decide the relevance of an item. The attempt was made to include everything but the purely incidental, such as brief obituaries or commemorations in the popular press. Preference is given to English editions. The bibliography is current as of January, 1997.[2]

Each entry is keyed to one or more of the following categories:

- [A] Writings by Hertz
- [B] Biographical studies and materials
- [C] Background and context to Hertz's researches
- [D] Contemporary and scientific responses to Hertz's researches in electrodynamics
- [E] Historical and philosophical investigations of Hertz's researches in electrodynamics
- [F] Contemporary and scientific responses to Hertz's *Principles of Mechanics*
- [G] Historical and philosophical investigations of Hertz's *Principles of Mechanics*
- [H] Hertz's philosophy of science
- [I] Instruments and experiments: replications and technological innovations
- [K] Hertz and Wittgenstein

A more detailed breakdown of these categories follows. It includes a listing of all entries which fall under each category.

[A] WRITINGS BY HERTZ[3]

PM: Hertz 1956 (translation of Hertz 1910), **EW:** Hertz 1962 (translation of Hertz 1894), **Misc:** Hertz 1896 (translation of Hertz 1895), **MLD:** J. Hertz 1977; **also:** Braubach 1958, Breunig 1988, Doncel and Roqué 1990, Friedburg 1988b, L. Hartmann 1926, Hertz 1878, Hertz 1879, Hertz 1880a, Hertz 1880b, Hertz 1881, Hertz 1882, Hertz 1883, Hertz 1889, Hertz 1971, H.G. Hertz and Doncel 1995, Höfler 1899, Koenigsberger 1902–03, Kuczera 1984, Mulligan 1994a, Mulligan and H.G. Hertz 1997, O'Hara and Pricha 1987, O'Hara 1988b, Rompe and Treder 1984, Thiele 1968

Four *drafts* and the final *manuscript* for the *Prinzipien der Mechanik*, some geophysical graphs and sketches, and the manuscript for the 1889 lecture "Über die

D. Baird et al. (eds.), Heinrich Hertz: Classical Physicist, Modern Philosopher, 281–305.

Beziehungen zwischen Licht und Elektricität" (Misc chapter 20) are at the Deutsches Museum in Munich. In the collection of the Science Museum in London are *inter alia* two early manuscripts (on Theory of Magnetism and on the Demonstration of Electrical Effects in Dielectricity; excerpts from the latter are in O'Hara and Pricha 1987 and O'Hara 1988c) and the manuscripts for chapters 2, 4, 5, and 8 of EW and chapters 1, 6, 8, 12, 16, and 19 of Misc. The Universitätsarchiv Karlsruhe keeps the original Laboratory Notes of 1887 (Hertz and Doncel 1995) and also some rare Hertziana (as indicated in this bibliography).

Still awaiting publication are Hertz's inaugural lecture on the foundations of the mechanical theory of heat, manuscripts on the theory of magnetism and on electrical effects in dielectricity, lecture-notes on the constitution of matter. Also, Jesper Lützen's recent researches have shown that much would be gained from a critical edition of the various drafts for the *Principles of Mechanics*.

The Deutsches Museum in Munich has 162 letters from and 243 letters to Heinrich Hertz. Locations for further letters include the Staatsarchiv Hamburg, the Royal Dublin Society, Trinity College in Dublin, University College in London, the Royal Society of London, the Institution of Electrical Engineers in London, and The Science Museum in South Kensington (for some of the details see O'Hara and Pricha 1987, pp. 140f.).

The Deutsches Museum also preserves a collection of Hertz's *scientific apparatus* (cf. pp. 55–58 above), for photographs see O'Hara and Pricha 1987, pp. 142ff., Auer and Klemm 1968, Bryant 1988a, Friedburg 1988b, Paul 1958, cf. Maurer 1968 on Hertz's original apparatus at the Deutsches Museum.

[B] BIOGRAPHICAL STUDIES AND MATERIALS[4]

Materials: ***Personal Recollections:*** Bjerknes 1923, Doll 1894, Goldstein 1921, J. Hertz 1977, Kayser 1908, Kayser 1936, Lenard 1943b; ***Documents relating to the biography and legacy of Hertz:*** Elbogen 1956, Fachschaft Physik der Technischen Hochschule Karlsruhe 1938, Friedburg 1988b, Helmholtz 1975, Hoepke 1988, Lehmann 1892, Lenard 1895, Lenard 1957, Ludwig 1894, Maurer 1968, Mulligan 1994a

Obituaries, brief notices, commemorative assessments: Auer and Klemm 1968, Bendt 1899/1900, Beyrer 1991, Blanchard 1938, Bonfort 1894, Bopp and Gerlach 1957, Cath 1957, Doncel 1994a, Doncel 1994b, Ebert 1894, Falkenhagen 1957, Garratt 1994, Gerlach 1958, Gerlach 1970, Gerwig 1956/57, Heidelberger and Hoffmann 1994, G. Hertz 1957, G. Hertz 1971, H.G. Hertz 1988a, H.G. Hertz 1988b, R. Hertz 1957, Iklé 1907, D.E. Jones 1894, Keller 1957, Lampariello 1955, Landgräber 1939, Laue 1977, Lenard 1894, Lenard 1895, Lodge 1894a, Ludwig 1894, Malov 1938, Mirow 1956/57, Missler 1957, P. and E. Morrison 1957, Mulligan 1994b, Nichols 1894, Planck 1894, Rothe 1957, Schimank 1957, Schmidt 1893, Schröder 1994, Sominskij 1957, Soret 1894/95, Steiner 1958a, Steiner 1958b, Stur 1894, Susskind 1988a, Susskind 1988b, Voit 1894, Vvedenskij 1957, Wagner 1930, Wenke 1957, Westphal 1957, Wiesbeck 1988, Wolf 1950, Wolf 1957, Wolf 1959, Wolf 1967, Zenneck 1929, Zenneck 1938, Zenneck 1946

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[C] CONTEXT AND BACKGROUND TO HERTZ'S RESEARCHES

Scientific Institutions: Bennett *et al.* 1993, Buchwald 1987, Cahan 1989, Caneva 1978, Hoffmann and Kant 1988, Jungnickel and McCormmach 1986, Lenard 1943b, O'Hara 1988a, O'Hara 1988b, Olesko 1991, Olesko 1994

Hertz's Training: Buchwald 1992, Mulligan 1987, Mulligan 1989b

Precursors, Compatriots, and Successors[5]: McCormmach 1982; ***Bjerknes:*** Friedman 1989; ***Bohr:*** Chevalley 1991; ***Boltzmann:*** Bouveresse 1991, Broda 1983, D'Agostino 1990, Klein 1974; ***Ehrenfest:*** Klein 1970; ***Helmholtz:*** Buchheim 1971, Buchwald 1993a, Cahan 1993a, Cahan 1993b, D'Agostino 1971, Darrigol 1994, Hatfield 1990, Heidelberger 1993, Heidelberger 1994, Hörz and Wollgast 1971, Hyder 1997, Kaiser 1993, Koenigsberger 1898, Koenigsberger 1902–03, König 1968, Krüger 1994a, Krüger 1994b, Lemmerich 1988, Rechenberg 1994, Rompe and Treder 1984, Röseberg 1994, Schiemann 1994, Woodruff 1968; ***Kirchhoff:*** D.S. Jones 1988; ***Lorentz:*** Hirosige 1969; ***Mach:*** Kleinpeter 1899, Kleinpeter 1905, König 1968, Treder 1974; ***Michelson:*** Swenson 1972; ***Planck:*** Mulligan 1994b; ***Poincaré:*** Darrigol 1995; ***Schrödinger:*** D'Agostino 1992, Mehra and Rechenberg 1987; ***William Thomson (Lord Kelvin):*** Knudsen 1985, Smith and Wise 1989; ***Zöllner, Fechner, Weber:*** Molella 1972, Wiederkehr 1960

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[D] HERTZ'S RESEARCH IN ELECTRODYNAMICS: CONTEMPORARY AND SCIENTIFIC DISCUSSIONS

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The 1888-experiments and their theoretical implications: Boltzmann 1890, Boltzmann 1909, Boltzmann 1974, Drude 1894a, Drude 1894b, FitzGerald 1890b, FitzGerald 1891, FitzGerald 1893a, FitzGerald 1893b, FitzGerald 1970, Helm 1904, Holzmüller 1898, D.E. Jones 1894, Kayser 1908, Le Bon 1908, Lecher 1901, Lecher 1925, Lodge 1894a, Lodge 1894c, Lodge 1970, Lodge 1974, Lorentz 1937a, Neumann 1901–03, Planck 1894, Planck 1899, Planck 1931, Poincaré 1891a, Poincaré 1894a, Poincaré 1894b, Poincaré 1894c, Poincaré 1900, Smith and Wise 1989, Thompson 1912, E. Thomson 1894, W. Thomson (Kelvin) 1962, Trouton 1889, Trunzelmann 1889, H.W. Watson 1889, Wess 1988; ***The Problem of Multiple Resonance (speed of propagation in air and in wires):*** Bjerknes 1923, Cornu 1890, de la Rive 1890, Drude 1894a, FitzGerald 1890a, D.E. Jones 1891, Poincaré 1894c, Poincaré 1954, Sarasin and de la Rive 1890, Sarasin and de la Rive 1892, Sarasin and de la Rive 1892/93, Sarasin and de la Rive 1893, Thorp 1891, Trouton 1890, Trouton 1891; ***Repercussions in 20th century physics:*** Born 1933, Born 1962, Sommerfeld 1918, Sommerfeld 1952b

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[E] HERTZ'S RESEARCH IN ELECTRODYNAMICS: HISTORICAL AND PHILOSOPHICAL INVESTIGATIONS

Assessments of the 1888-experiments: Boerger 1988, Buchwald 1994a, Chen 1989, Chinese Electrotechnical Society 1988, D'Agostino 1993b, Friedburg 1991, F. Fuchs 1958, Grigorjan and Wjaltsev 1968, Hermann 1988, Herneck 1965, Kraus 1988, Kuczera 1987, Lehmann 1914, Rechenberg 1988; ***Situating Hertz's researches in relation to contesting theories of electromagnetism:*** Bauer 1965, Berkson 1974, Buchwald 1985, Buchwald 1990, Buchwald 1993b, Buchwald 1994b, Cazenobe 1980, Cazenobe 1983, Cazenobe 1984, D'Agostino 1971, D'Agostino 1989, Darrigol 1993a, Darrigol 1993b,

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Experiments on cathode rays and the photoelectric effect: Buchwald 1995a, Hon 1987, Stuewer 1971, G. Thomson 1970

[F] HERTZ'S *PRINCIPLES OF MECHANICS*: CONTEMPORARY AND SCIENTIFIC RESPONSES

Kuczera 1984, Lenard 1957; ***The foundation of mechanics (including reviews of PM):*** Boltzmann 1897, Boltzmann 1900b, Boltzmann 1903, Boltzmann 1974, Brill 1900a, Cailler 1895, Classen 1897, Duhem 1980, Ebert 1895, FitzGerald 1895, Hamel 1967, E. Hartmann 1902, Helm 1895, Helmholtz 1910, Heun 1901, Hölder 1896, Lampe 1897, Lindsay and Margenau 1957, Lorentz 1937b, Mach 1960, Poincaré 1952, Poincaré 1960, Russell 1938, Schaefer 1919, Sommerfeld 1952a, Volkmann 1900, Volkmann 1901, Wiechert 1925, Wien 1901, Winkelmann 1929; ***Examples and applications:*** Boltzmann 1899, Boltzmann 1900a, Brill 1900b, Ehrenfest 1959, Friedman 1989, Paulus 1916, Reiff 1900, Samuelson 1965; ***Further mathematical and physical developments:*** Brill 1909, Capon 1952, Capon 1954, Jourdain 1905, Lanczos 1949, Manno 1903, Onicescu 1977, Schrödinger 1916/18, Voss 1901–08

[G] HERTZ'S *PRINCIPLES OF MECHANICS*: HISTORICAL AND PHILOSOPHICAL INVESTIGATIONS

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Hertz on impact, hardness, and elasticity: Lorenz 1913, Mundt 1950/51, Szabo 1977

[H] HERTZ'S PHILOSOPHY OF SCIENCE

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[I] HERTZ'S INSTRUMENTS AND EXPERIMENTS: REPLICATIONS AND TECHNOLOGICAL INNOVATIONS

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[K] HERTZ AND WITTGENSTEIN

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NOTES

1 For example, *J.C. Poggendorff – Biographisch-Literarisches Handwörterbuch der exakten Naturwissenschaften*, supplementary volume VIIa, edited by Rudolph Zaunick (Berlin: Akademie Verlag, 1970), pp. 283–286 lists approximately 50 bibliographic items which are not included here (not counting entries on Hertz's genealogy, on portraits, sculptures, and commemorative stamps). Also, not included is the majority of miscellaneous articles which appeared in 1988 and 1994 on the centennials of the discovery of radio-waves and of Hertz's death.

2 I am indebted to the bibliographies in Boerger 1988, R. Cohen 1956, and Mulligan 1994a, also to "Literaturnachweise aus dem Bestand der Akademiebibliothek [Berlin]" by M. Eggert (1994), finally to information provided by Paulo Abrantes, Ricardo Lopes Coelho, Klaus-Peter Hoepke, Giora Hon, Jesper Lützen, Joseph Mulligan, Brent Mundy, Helmut Pulte, Gregor Schiemann, and many others. Very helpful were the interlibrary loan librarians at the University of South Carolina. In a few cases where prints are rare, information on the location of copies is included.

3 For a more detailed listing cf. Buchwald 1994a, pp. 471f.; Mulligan 1994a, pp. 405–409; and Nichols 1894. Also, cf. the concordance in this volume.

4 Cf. note 1 above.

5 This section includes discussions of Helmholtz's epistemology, philosophy of science, and electrodynamics which implicitly provide background to Hertz's researches. It also includes studies explicitly devoted to the relations between Hertz and a number of other scientists (including Helmholtz). For works authored *by* any of these scientists, cf. the references below.

6 For contemporary problems and issues concerning the replication of Hertz's experiments cf. *Experiments on Cathode Rays* under [D] and [E], and *The Problem of Multiple Resonance* under [D], also Boltzmann 1890 and Atten 1995.

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