THE LOGIC OF SCIENCE

Edited by VINCENT EDWARD SMITH, Ph.D.

ST. JOHN'S UNIVERSITY PRESS

New York · 1964

Library of Congress Catalog Card Number 64-12246

Table of Contents

A PROGRESS REPORT

I

ONE OF the most important preoccupations of philosophy since the time of Francis Bacon and René Descartes has been the nature of the scientific method. This direction of modern thought has taken such emphatic form that many of the philosophers of science in the Anglo-American world during the twentieth century accepted the view of the earlier Bertrand Russell that philosophy is only logic. Among the logical positivists, who stem from Russell even though he disagreed with their positions, one of the most mooted questions has been the so-called verification-principle. The various attempts to formulate this principle are reminders of the tremendous concern that modern philosophy has had with the problem of the scientific method.

Interest in the scientific method has not been confined only to philosophers. In the very century of Bacon and Descartes, Newton made his well-known references to method in both the *Principia Mathematica* and the *Optics.* Book III of the *Principia* opens with a statement of Newton's "rules for philosophizing." Another seventeenth-century giant in science who made at least parenthetical but nevertheless self-conscious references to methods was William Harvey. In our own century, the Heisenberg principle of uncertainty has focused a new and different type of attention on scientific procedures. Einstein has written explicitly on the method of theoretical physics. Cosmogonists like Bondi are wondering about the method for the study of the more remote reaches of the universe; and Simpson, the paleontologist, is concerned with the type of explanatory media, such as purpose, which are admissible into his science. That relatively new field of knowledge, cybernetics, affords still further indications of method as a pervasive modern problem.

II

The essays in the following pages, singly or together, make no pretense of being a definite treatment of the scientific method. One of their collective merits may be to call attention to the tremendous amount of work that remains to be done before a satisfactory synthesis concerning the problem of method can be achieved.

The essays were originally presented as guest lectures in the Philosophy of Science Institute at St. John's University. This Institute, whose primary purpose is to explore the borderland problems between philosophy and science, conducted a special course on the logic of science in the academic year 1961-1962. Among the topics discussed in this course were the nature of mathematics and the relation of mathematics to the natural sciences; the status of scientific theories; induction; statistics; the role of language in science; the character of the laws of nature; and the structure of scientific systems.

The Institute now has a cycle of three special courses, dealing successively with the logic of science, the philosophy of physics, and the philosophy of biology. The courses in this cycle will be repeated in title but not in content. Each time around, new phases of method and of the ontological problems involving physics or biology will be emphasized in the spirit of the continuing research and study required by the vast tangle of problems which the philosophy of science involves.

Let it be repeated then that the reader can expect no definitive treatment of the scientific method from the four essays in this book. Their unity with each other consists in the fact that they were delivered as lectures, in different parts of one course that itself was taught through a problem approach, rather than as an effort to achieve a systematic synthesis.

III

This latest publication of our series affords an opportunity to thank the Administration at St. John's University for its generous financial and moral support of the Philosophy of Science Institute. Grateful acknowledgment is made to the Very Rev. Dr. Edward J. Burke, C.M., president of the University; to the Rev. Dr. Joseph E. Hogan, C.M., former dean of the Graduate School; and to the Rev. Dr. Carl W. Grindel, C.M., chairman of the philosophy department.

Philosophy of Science Institute
VINCENT E. SMITH, *Director*

Mortimer J. Adler
Director, Institute for Philosophical Research

Questions Science Cannot Answer

The position I am going to try to defend here is one which is shared by few, if any, contemporary secular philosophers. The reigning philosophical position with respect to the relation between science and philosophy is the one held by the positivists who relegate philosophy to a realm of opinion, totally precluding it from consideration as a valid kind of knowledge. The dominant school of English and American positivism dates back to David Hume. The contemporary positivists, or analytic philosophers, have a great deal of logistic and semantic apparatus, but in fact, their essential position is the same as Hume's. They claim that the only questions that are to be answered with verifiable or valid knowledge are the questions that science can answer. The questions that science cannot answer are either not answerable, or answerable only by opinion.

I grew up in the beginning of this century in the philosophical atmosphere of pragmatism. I studied at Columbia University under John Dewey and I had a great fondness for William James. I remember in my undergraduate career coming upon the last (unfinished) book that James wrote, *The Problems of Philosophy,* where he takes the position that philosophy is always working in the penumbra of science. James asserts that in every department of knowledge science is at the center, and out beyond the confines of established scientific knowledge there is a shadowy area called philosophy. Here philosophers are at work on questions or problems that science has not yet been able to solve. But as science advances it eventually solves those problems, and the philosopher gets pushed out further into the shadows again, to work desperately at matters that the scientist cannot presently handle.

Even when I was quite young, it seemed to me that if this is what philosophy is, it is hardly a respectable profession. Why should anyone waste his time today on the problems that science is going to solve tomorrow? Why not just wait until science gets there and solves them? Why should the philosopher be a kind of frustrated, futile worker in a field which science will eventually invade? (This by the way is the attitude of some Nobel-prize winners who are called to the lecture hall by virtue of their having been great scientists but who, in their old age and idle hours, have become speculative philosophers.) It is as if there is no special method and no special discipline which are philosophical, as if anyone who has achieved eminence in science then has the authority to speak loosely and freely about problems in other fields.

The consequences of this general atmosphere, where philosophy is in decline and science is in the ascendancy, are tremendous. Not only does philosophy get displaced in our culture, but religion does too. I am happy to say, however, that Catholic philosophers in general take an opposite position. They certainly take an opposite position with respect to the relation between science and philosophy. But unfortunately there are other positions taken by Catholics. I am thinking in particular of the work that is being done at the Albertus Magnus Lyceum in River Forest, Illinois, where the natural sciences are seen as continuous with the philosophy of nature. By various turns and tricks with Thomistic apparatus, the natural sciences are assimilated to philosophy and made continuous with it. I think this view is as wrong as the position taken by the positivists.

Having sort of laid myself open, I will proceed to defend the position which I think is true. Let me state it for you quickly in three simple theses.

First, there are three quite distinct and discontinuous kinds, spheres, or domains of knowledge, for which I will use the words science, philosophy, and either religion or theology. I mean by theology, now, not natural theology but sacred theology. Each of these fields is distinguished by a characteristic method. Each method is adjusted to a certain object of study. According to each method there are answerable and unanswerable questions.

My second thesis is this: there are questions which the scientist can answer, but which the theologian and the philosopher cannot; there are questions which the philosopher can answer but which the scientist and the theologian cannot; and there are questions which the theologian can answer but which the scientist and the philosopher cannot. The reason for this diversification is that each has a method which makes him competent to answer only certain questions, and precludes him completely from answering with competence or validity the questions that lie beyond the scope of his method.

My third thesis is that the basic questions, both speculative and practical, are the questions that science cannot answer. The reason for calling this paper "The Questions Science Cannot Answer" is to make clear that the questions science can answer are the least important questions of all. The fact that science cannot answer the most important questions does not by itself establish the fact that philosophy and theology can answer them. One does not follow from the other. But I do want to establish what kind of questions science can answer and cannot answer, and I shall at least suggest the possibility that philosophy can answer some of these that remain unanswerable by science, and theology still others.

Before I get to the actual arguments for these positions, I would like to tell you the remote background for my present views. Many years ago, in the middle 1930's, I was asked by a lecture bureau to debate with Bertrand Russell whether there are universal principles of education. I took the affirmative. On the occasion of this debate we were in a large auditorium in Chicago. We arrived in our dinner jackets. I had worked hard, thinking of Lord Russell as an eminent philosopher — one to treat with respect — and I carefully prepared the statement of the affirmative position. But Mr. Russell obviously did not regard debating with me as an important occasion: he came with a white cuff on which he made notes as I talked. As I remember, his rejoinder was just a barrage of wit, without much arguing. He began with the remark, "I greatly admire Dr. Adler's rugged simplicity."

I must say that, since I take intellectual issues and debates very seriously, I did not think well of Lord Russell's manners and immediately resolved never to debate with him again. But this was a popular occasion, and the audience enjoyed it so enormously, that a year later I was asked again to meet Lord Russell in a debate. But this time I declined — unless Mr. Russell was willing to take the affirmative on an issue and let me take the negative. The negotiations went on and on; it took a little more than six months for Lord Russell to find anything he could affirm. Finally, we found the question on which he was willing to take an affirmative position. It was, "Is Science Enough for the Good Life and the Good Society?" Lord Russell was going to answer that question affirmatively, and I was glad to take the negative.

Lord Russell got up first and said something to the effect that science represents the only valid knowledge we have. Knowledge, he averred, is incapable of solving any question of value, by which he meant that we cannot, by means of knowledge, answer any question about what is right or wrong, good or bad. "How are these questions solved?" asked Lord Russell. And he answered himself, "Well, clearly by feelings."

Notice at once how Lord Russell had contradicted himself: he started out to affirm that science is enough for the good life and the good society, and in the same breath said that science wasn't enough because it could not answer any questions about good or bad, right or wrong, or how one can conduct the good life in the good society. Such questions can only be answered by "feelings." Well, I got up for the rebuttal and said that, obviously, if questions of value were solved only by feelings, then one had to ask whether or not all feelings were equally good or bad, right or wrong. . . .

By this time I really had Mr. Russell on the run, because he had just come out publicly, for the first time, against Hitler. The German cause was wrong and the English cause was right. I read from his statement that had just appeared in the New York *Times,* and said, "Lord Russell, I gather that you have certain feelings and that Hitler has certain feelings; you said that your side of this matter is right, and that Hitler's is wrong. Then, what is the measure of the rightness of the feeling by which you have made these judgments or taken these positions, and of the wrongness of Hitler's? If, in regard to your feelings, there is no objective measure at all, if it is just a matter that you feel you are right, then Hitler is entitled to feel that he is right. And the only thing that could solve or settle any conflict that involves questions of right or wrong would be might or force, the force of numbers or the force of guns.

"If there is any objective solution to such problems," I continued, "objective in the sense that it is based on something other than one's feelings, then something must exist to measure feelings, as right or wrong, good or bad. And I submit to you that the only thing that could possibly measure feelings is knowledge. Hence, either you must submit to a complete subjectivism and relativism, or you must admit that there is knowledge other than science, because I'll agree with you that science cannot solve any questions of value." Lord Russell was quite willing to sink into the position of complete relativism and subjectivism.

A recent article on Heidegger in *Encounter* reported that Lord Russell, in an exchange of letters in the London *Observer,* said explicitly that his philosophical position would put his dislike for merciless cruelty and his liking for oysters exactly on a par. This indicates to me the seriousness of the question whether philosophy is a body of valid knowledge beyond the scope of science.

Let me illustrate my main thesis again by taking two closely related sciences, pure mathematics and experimental physics, which are joined in the mixed science of mathematical physics. We will designate pure mathematics as science X and experimental physics as science Y. I think it is perfectly clear that the problems the pure mathematician faces can in no way be solved by experimental work in the laboratory; it is equally clear that purely experimental problems cannot be solved by the methods available to the mathematician. Here are two closely related sciences — fused in mathematical physics — yet absolutely distinct because the methods used in mathematics are totally unavailable for solving a purely experimental problem. Conversely, the methods of experimentation are totally unavailable, totally incompetent, for solving a purely mathematical problem.

Notice one further paradox: when the mathematician is unable to solve an experimental problem, he also cannot refute or criticize the experimental solutions of problems. That is, a mathematician cannot criticize an experimenter except by becoming an experimenter himself; and an experimenter cannot criticize a mathematician except by becoming a mathematician himself. In other words, if science X cannot answer, because of its limited methods, the questions which science Y can answer, then science X cannot refute or criticize the answers given by science Y. There is no dialogue between them.

What I've just said about mathematics and experimental physics is illustrated more clearly by taking two bodies of knowledge like botany and history. Now, the methods of history are totally different from the methods of classificatory botany. Whatever the problems of the science of botany are, a historian with his methods must remain silent about their solution. Whatever the problems of historical research are — those which can be approached and solved by the methods of historical research — a botanist must stand on the sidelines and remain silent too. *Qua* historian and *qua* botanist, they cannot deal with each other's problems.

To take a more obvious example of the simple ordering of disciplines and fields: no one in his right mind who had a serious illness would call in a mechanical engineer, and no one in his right mind with the problem of building a bridge would call in a physician. The competence of the physician belongs at the bedside; the competence of the engineer belongs at the riverside where you are building the bridge. No one would make the mistake of supposing that either the engineer or the physician has omnicompetence. You know the limited competence of each.

Now, what I have just written about obvious cases applies to philosophy and science in general. Take all of the sciences, from astronomy to zoology, and compare them to the entire range of philosophical studies: my point is that the whole sphere of science consists of questions that the philosopher cannot touch or answer at all, and he should know he can't. Neither scientists nor philosophers can refute the answers given by the other. There have been, of course, confused questions during the long history of thought. One of the great misfortunes is that there have been many questions that philosophers have thought were philosophical questions when actually they were not. For example, it was naive of Aristotle and St. Thomas to think that the question of the material constitution of the heavenly bodies was a philosophical question. Their methods and their conditions of observation were inadequate to answer this question. Even today there are borderline questions on which scientists and philosophers get confused. With the progress of man's inquiry and knowledge, there is progress in the clarification of a question, in knowing where a question belongs and whether it is truly a scientific question, or truly a philosophical one. And when the question is clarified, so that you know the kind of question it is, the kind of method that it calls for, or the kind of method that is competent to answer it, then the division of the fields of knowledge is going to be clear.

Having laid the groundwork, let me now get to my main job which is to say what the limitations of science are — that is, what questions its methods enable it to answer, what kind of questions belong to it, and what kind of questions it cannot answer because of the limitations of its methods. The particular sciences, of course, have particular differences in method. But I want to talk about science in general and say what is common to the method of science, or the sciences, despite the particular differences as between, let's say, work in an observatory which is not experimental, and work in a laboratory which is experimental. We know that in the field work of a sociologist and the laboratory work of the entomologist the methods are different, but what is common to all of them?

I have a relatively simple answer to that question, and I hope it is the right one. I want to avoid the words "empirical" and "experimental" because not all science is entirely experimental in the strict sense of the word, and empirical suggests that science will appeal to experience. But philosophers appeal to experience as much as scientists do. In my view, experience is just as important to philosophy as it is to science. Hence, I prefer to use the word "investigative." The fundamental characteristic of any science is that *it investigates.*

Every moment of our waking lives we reflect on the experience we have had. We experience and then we think. We do this as naturally and as regularly as breathing. It's a regular function. But we are not naturally investigators, because the data come to us without any design on our part. If you keep your eyes and ears open, you will see, listen, and then reflect. Our intelligence is functioning along with our senses and imagination.

By investigation I mean a deliberate, planned, devised way of getting data beyond the ordinary experience of men. Eskimos **8on't have quite the same experience as the Congolese, but by and large all men see things fall, see things live and die. These are the generic things of common experience. If no one went beyond this, if no one had any experience other than the ordinary, there would be no science whatsoever. Science begins by additions to ordinary experience and gradually moves farther and farther away from the field of general experience. Science uses deliberate, planned effort to observe and measure what men don't ordinarily see and hear. It gets the phenomena that do not come within the common experience of mankind.

I don't mean to imply that the development of science is not deeply dependent on analysis, on theoretical elaborations, on the development of hypothesis, on mathematical analysis and all of that. But the essence of science lies on the side of sense. I think I am borrowing from Maritain, who makes this point in *The Degrees of Knowledge.* All knowledge involves reason and sense, that is, all natural knowledge does. But in the case of science, reason serves sense, not sense reason. The whole apparatus of scientific reflection, of analysis, and of theoretical development, is for the sake of handling what sense apprehends — the stubborn data, the phenomena, if you will. Every advance of science, no matter how extended the theories are, is dependent on sense confirmation. Mathematical physics today is probably fifty years ahead of the experimental, as was the case with Einstein. But as I understand science, the theories are all here begging for the test, and not until the test is made, not until the data are procured, does science really advance. Every real, established advance in science is made here, not in the realm of theory. I am not denying that the scientific theory is necessary, but I am saying that the critical point is on the side of observation. For science grows by adding to the ordinary experience of man the data procured by investigation.

Think, for just a moment, of Aristotle whose experience of the heavens was the experience of any man, perhaps with a little more patient observation. Anyone can look up at the heavens and see them revolve. On a moonless night anyone can see what the poets see, just as Greek scientists could see the configuration of the celestial bodies. Aristotle and Ptolemy did more *thinking* about this than the ordinary fellow did; they did not *see* any more. The science of astronomy as opposed to astrology — the true science of astronomy begins with the investigation of the heavens by apparatus that brings into focus or to awareness data that are beyond the ordinary experience of mankind.

Science begins with investigation. Because science is truly investigative and depends on investigation, it is therefore limited to what can be investigated. Science is concerned with whatever it is that can be investigated — and the only thing that can be investigated is the phenomenal world. Whether the phenomena are sensible to the ordinarily naked faculties of sense, or to those faculties aided by instrumentation, makes no difference. And as a result of this fact, the object of science is the correlation and description of that phenomenal world. The positivist is absolutely right here; the positivist understands science quite well. Whether he understands philosophy is another matter. But he does understand that the scientist, by his methods, can merely give you the correlations and descriptions of the phenomena. He cannot talk about substances and causes at all, except as a practitioner in the laboratory, where he talks like an ordinary man. The scientist has to change character when he goes into the laboratory, but strictly as a scientist he is prevented by his method from dealing with substances and causes.

Let me just give you an indication of the kinds of questions that the method of science, because it is investigative, precludes the scientist from answering. The last thing in the world that a scientist can answer is a question about the ultimate constitution of matter. He may think he can; but this question is beyond investigation. The scientist cannot answer any questions about existence. All the questions about existence — the modes of existing, the grades of being, and essential distinctions among beings — lie outside the competence of the scientist He cannot answer, by his method, the three great questions that Kant said were the great philosophical questions : the immortality of the soul, the freedom of the will, and the existence of God. He cannot answer any questions about the universe as such ; the notion of the universe is not a scientific notion. If there is a universe, that will never be discovered or denied by science. The scientist cannot answer any questions about the nature of knowledge. What knowledge is, is itself a question that is not open to investigation. It is not a scientific question. These are all intelligible questions, but science cannot answer them. Moreover, my position here is not just that science cannot answer them now, but that science cannot answer them ever.

I've mentioned speculative questions. In the practical order, science cannot answer any questions about ends or means, the order of good, questions about what happiness is, the goals of life, questions about virtues and duties. In the field of political or social philosophy science cannot deal with questions about justice, peace, democracy. All these questions are utterly beyond science. Science cannot solve a single moral or political problem now or ever. What is the utility of science? It is very useful, but its utility is entirely technical, as Lord Bacon perfectly understood. Science gives us a mastery of the external world, a technical mastery. It is productive. Lord Bacon said that knowledge is power and that the aim of science is the production of the means. He is absolutely right; that is what science is. It gives us control over the means.

But you recognize that control, through the invention and mastery of means without a right direction of the means to a proper end, can be more dangerous than having no mastery of the means at all. And that, as I take it, is the condition of the contemporary world. We have more and more control and mastery of means, and less and less ordering of the means to ends, than any other century in the history of mankind.

Now I'll turn to philosophy for a moment to emphasize this contrast. In my early years of teaching philosophy to undergraduates I had a familiar experience, and it repeated itself again and again. About the sixth or seventh week of the course a bright student would come up and say, "Mr. Adler, this is a very interesting course and I'm enjoying it very much, but what's the use of it all?" In the beginning when this question was first put to me, I would try to answer it seriously; then I learned a better answer. I'd look that student directly in the eye and say, "Mr. Jones, in your sense of the word *use,* philosophy is of no use whatsoever." I'd say this because the question that Mr. Jones was asking was posed in terms of the general atmosphere in which he lived. To him, the use of knowledge, meant a technical use — production. Philosophy builds no bridges, cures no illnesses, creates nothing, produces nothing, turns out nothing you can live by or on, produces none of the comforts or conveniences of life. It has no use. Then, when I got this point absolutely clear for the young man, I would say, "If you let me suggest that there is another use for knowledge, then I can indicate the use of philosophy. Do you think that road signs are useful?" He would of course agree. Then I would ask whether maps were useful too. Again he would agree. Then I'd say, "When you want to get somewhere, directions are helpful."

In a similar way philosophy is a guide to life, for there is another use of knowledge besides production. There is the moral, the practical. The practical concerns not just making but doing. And philosophical knowledge, if there is any, is useful in the guiding or directing of doing. But it produces nothing. I can't imagine anyone who would deny the proposition that philosophy has produced nothing. In the whole history of philosophy, nothing has been produced, by the whole or any part of philosophical knowledge. Philosophy is totally non-productive. Now this is a very striking fact — especially since science is nothing *but* productive. And this too is a very striking fact. There must be some reason science is productive and philosophy is non-productive, and I think it has to do again with the difference in their methods and with the differences in the questions each can answer.

Let me conclude by sharpening the contrast, and by saying what the method of philosophy is. From here on I am going to be entirely negative, because to say positively what the method of philosophy is would be too difficult to accomplish in a brief essay. But the negative point is sufficient. Philosophy is not investigative. By this I mean, it never needs anything more than the ordinary experience of mankind. This is the experience anyone has just by being awake. Another way of saying this is that the philosopher is an armchair thinker. And there is nothing wrong with his being an armchair thinker — so is the mathematician. The mathematician isn't a scientist in the sense that science is investigative. A mathematician who got out of his chair to look at anything would be no good. Can you imagine a mathematician with a problem saying, "I've got to *go* out and investigate." You would know at once that he wasn't a mathematician at all. If he couldn't solve the problem sitting in a dark room, or without paper and pencil, he isn't a mathematician. That young men can be prodigies in mathematics illustrates the point, for here you have even less experience than in philosophy. You don't need maturity to be a mathematician; in fact, maturity spoils your being a mathematician. Mathematics is armchair thinking par excellence. And so is philosophy. Now let me illustrate that by my favorite example: freely falling bodies.

You see leaves fall from trees, stones roll down hill, things drop out of windows. Now, if your only question is, What is local motion? you don't need more experience than to see a body leave one place and move to a different place. One of Aristotle's questions is, What is motion in place, what is local motion, what is common to all local motion? But suppose you ask another question: What is the acceleration of the freely falling body? I assure you at this point that ordinary experience is not enough. Yes, you can say that the farther it travels .the faster it seems to go, but you aren't sure even of that. Yet suppose you could, with your naked eye, obtain evidence that the greater the distance travelled by a falling body, the faster it seemed to go: would that be a scientific statement? That wasn't the question that interested Galileo in the third day of *The Two New Sciences.* He wanted to know just how a falling body behaved at every infinitesimal point in time or space; he wanted to know whether in each unit of time or space there are definite increments. Is there uniform motion? Is there uniform acceleration? And if there is uniform acceleration, what is the rate of acceleration?

Galileo devised the inclined plane and the little water clock so that he could measure by pulses in the clock the amount of time elapsed during the space-intervals of a falling body. He could solve this problem only by some recourse to investigation. Ordinary men don't watch balls rolling down inclined planes while holding their fingers on water clocks. Yet Galileo did this, and he made crude tables with a tremendous amount of experimental error in them. He had a crude inclined plane with its surface frictions and a crude clock with its irregularities. He thus obtained a very rough set of data. But though the data were rough he did get the answer to the question about the way bodies fall. This is a simple example of science.

Aristotle could not answer questions about the rate of acceleration. These are questions that are scientific, not philosophical. The essential difference between uniform and variable motion, however, is not a scientific question. Galileo answers this question exactly as Aristotle answers it — without investigation. No investigation in the world could tell you the difference between natural and violent motion or what they are, or what uniform and variable motion are. It is a philosophical matter to define in motion what is uniform, variable, natural, and violent; and I assure you that those basic definitions with which Galileo begins are taken from Aristotle. No one is likely to change them until the end of time. They don't depend upon investigation. You may be able to think better than Aristotle did about these definitions. If so, then you can change them, but only by thinking about them, not by experiencing more, by getting new data, or by investigating. In other words, philosophy is the very opposite of science. While both faculties, sense and reason, are used in science and philosophy, in science reason serves sense, and in philosophy the sense serves reason. The senses merely give philosophy the common experience which is the basis for our reflections, analyses, and thoughts; then philosophy develops insight by penetrating the phenomena. Thus, questions about substances and the causes of things, questions the scientist cannot answer, the philosopher may be able to.

If I were talking in this manner to a classroom of positivists, I would, of course, see a big grin on their faces, which says, "That's what you say. You merely *assert* that philosophy can answer questions about the substances and causes of things." And I realize that I haven't established my point — I merely stated it. But I have one reply to these positivists, one example of a question that science cannot answer but that philosophy can answer, even if answers may differ according to philosophical schools. And this is the question we have been discussing here, the question concerning the relation of science to philosophy. I don't care what the *answer* is. I say that *any* answer to that question is *a philosophical answer.* Whatever way one answers it, he answers without an appeal to investigation. So if one has any answer, no matter what the answer is, true or false, he at least has a philosophical answer.

In the practical order, as a consequence of these points in the speculative order, there is a work that philosophy alone can do without the aid of science. It can answer a whole range of practical questions — with knowledge, not with opinion — in the fields of ethics and politics.

If one went on beyond science and philosophy to the problems of theology or religion, one would encounter the distinction between natural and supernatural knowledge, or between that knowledge which man acquires by means of the operation of his natural faculties, and that knowledge which is received by man without effort on his part. What the man of faith claims to have is knowledge which he does not in any way achieve by the exercise of his natural faculties; it is strictly a knowledge that is received as a gift. Now the interesting thing about the claim that the man of faith makes is that it is irrefutable. I assure you that neither the scientist nor the philosopher can say anything about it. If the philosopher could prove that what the man of faith, *qua* man of faith, says is true, the man of faith would be wrong. If the philosopher could prove that faith exists, faith wouldn't exist. The philosopher could show the intrinsic possibilities of faith. He certainly shows that it is possible for some knowledge to get into man's intellect by divine gift. The philosopher can show the possibility of faith, but that is as far as the philosopher can go. If the man of faith does not claim knowledge beyond rational proof, then I don't see the meaning of the word "religion" at all. I think the phrase "Eastern religions" is a self-contradictory phrase, because none of these religions claims to have any revealed knowledge at all. Philosophy and religion in Chinese culture are indistinguishable. I don't know why we call it religion, for nothing is claimed to be known here that is not known by natural means. And unless such a claim is made — that something is known without recourse to natural means — then religion has no claim whatever for a separate status.

Now in the practical order, as opposed to the speculative order, religion and theology have a peculiar character. Take religion rather than theology here. You see that the utility of science is production, and the utility of philosophy is direction. Religion also offers direction, but it gives us, in addition, the grace to follow directions.

In the history of the West, there has been a tendency to confuse the questions that belong respectively to science, philosophy, and religion. I get angry at the so-called orthodox Aristotelians who read Aristotle as if every word were true. In Aristotle, science, philosophy, and religion are confused. Since Aristotle doesn't know their distinctions, they are all inchoately mixed. He doesn't know that he is a different fellow in the *Historia Animalium* and the *De Partibus* from what he is in the *Metaphysics*. We find that the opening part of the *De Partibus Animalium* is quite different from the seventh and eighth books of the *Metaphysics*. But Aristotle doesn't know it, in the sense of saying, "Now look, I said that before and now I'm saying this, but I'm saying two different kinds of things. My methods are different, my problems are different."

When we get to the Middle Ages, we see that the achievement of St. Thomas was his persistent effort to get the line between philosophy and theology clear, to know what the true questions of sacred theology are, and how they differ from those of natural philosophy and metaphysics. But even St. Thomas is very unclear and confused about the proper line between philosophy and science, between what we call the investigative sciences and natural philosophy. His *Commentary on the Trinity of Boethius* (qq. 5 and 6) has lots of insights. But St. Thomas doesn't know about science; it's too early. He doesn't know enough about the Greek science of the Alexandrian period. And above all, he doesn't know about the sciences that were going to develop at the end of his century. When you get to modern times, when things should be better, things get worse because of the rise of scientism and the discard of philosophies and theologies. Yet, let me say that the possibility of a better state of affairs in the twentieth century is clear. We do have science now, existentially, as a quite distinctive enterprise. We can look at what it is doing. We don't have much philosophy left to look at, but we can remember what it was like in the past. Then there are the remnants of theology. It should be possible in the twentieth century to begin to get good order among our disciplines, to become aware of the limitations of man's three main efforts to know the truth. My own feeling is that this good order depends in a special way on knowing the differences between philosophy and science.

Roland Houde
Associate Professor of Philosophy, St. John's University

The Logic of Induction

Induction, in the modern sense of the word, does not resolve itself solely in sensible matter since, from its inception to its conclusion, induction is much more than just exact observation, experimentation, and measurement. Moreover, the problem of induction is not resolved only by reference to intelligible matter.[1] Indeed, in modern science, induction is not a search for the ultimate essence of any category of beings.

The purpose of this paper is to approach the logic of induction first by isolating and examining the genesis of the inductive experience; and secondly by collating a number of texts from Aristotle's writings with cases from the history of science, which bear on what we may call the "subject-bound qualities"[2] necessary to inductive abstraction.

1 Not the "intelligible matter" traditionally associated with mathematics; rather, "intelligible matter" as equivalent to "proper to the intellect" (cf. St. Thomas, *In Boeth. de Trin.,* VI, 2).

2 The expression "subject-bound (or subject-limited) qualities" refers to characteristics and dispositions of the knower. These dispositions are by no means arbitrary or undiscoverable *(infra,* pp. 24-25; 26 if.) Recent scholastic studies on induction in terms of "object-bound qualities" are: J. Sikora, "The 'Problem' of Induction," *The Thomist,* XXII (1959), 25-36; and P. Conway, "Induction in Aristotle and St. Thomas," *ibid.,* pp. 336-65. The former is an attempt to treat of induction by taking into account the specificity of modern science; the latter is an attempt to analyze the "object-bound" texts of Aristotle and St. Thomas which discuss induction in logical (or ontological) rather than in psychological terms.

In the middle ages, as we know, it was customary to incorporate the positive sciences into philosophy. The medieval study of the facts and activities of bodies represented a search for their *quod quid est.* What scientists today call the experimental or positive sciences were considered during the middle ages as arts generically named alchemy. In the *De Mineralibus* Albertus Magnus informs us that "it does not pertain to the physicist to treat of the transmutations of [metallic] bodies, and of the changes of one into another, rather this belongs to the art of alchemy."[3] Albert also goes on to explain that he studied "with the alchemists the transmutations of metals so as to learn something of their nature and proper accidents."[4] "I will not," continues Albert, "report everything that can be said concerning stones since it does not offer any scientific value. The science of nature, indeed, must not simply gather facts. Rather, it must search for the causes of natural phenomena."[5] For Albert and most of the other medieval disciples of Aristotle, this search for the causes is concentrated on final causes. For example, in the twenty-six books of Albert's treatise *De Animalibus,* the study of the causes is taken up in books 9 to 21. This analysis amounts to a study of the purposes of organs, that is, to a purely finalistic explanation of facts. In brief, the general absence of induction in the philosophical sciences of the 13th century is explained by a belief that facts have a value as means, and not as ends.

At the turn of the century, Auguste Mansion summed up the medieval position on the logic of induction in these words: "It was useless to analyse a process which was not employed."[6]

3 Lib. III, tract. 1, cap. 1.

4 *Ibid.*

5 Lib. II, tract. 2, cap. 1.

6 "L'induction chez Albert-le-Grand," *Revue Néo-Scolastique de Philosophie,* XIII (1906), 246. This is not to gainsay studies such as W. Wallace's *The Scientific Methodology of Theoderic of Freiberg* (Fribourg, 1959). Granting, and rightly, that "the term 'scientific' is obviously not to be taken in contradistinction to 'philosophical'" (p. 22), Father Wallace is equally right to subject the methodology of this period of history to investigation in order "to delineate the basic factors leading to the differentiation of science and philosophy in modern thought" (p. 6). Such a work is laudable, and necessary for the

The absence of analyzed induction, for the middle ages, is verified by the absence of treatises on induction. In contrast, modern philosophy gives evidence of attempts at resolving the ground, nature, justification, and conditions of induction. Since 1872 — with the eighth revised edition of his *System of Logic,* published originally in 1843 — J. S. Mill has set the course for an essentially objective analysis of the ground of induction by linking it to the law of uniformity and variety of nature:

> . . . there is a principle implied in the very statement of what Induction is: an assumption with regard to the course of nature and the order of the universe; namely that there are such things in nature as parallel cases; that what happens once, will, under a sufficient degree of similarity of circumstances, happen again, and not only again, but as often as the same circumstances recur. This, I say, is an assumption involved in every case of induction. And, if we consult the actual course of nature, we find that the assumption is warranted. The universe, so far as known to us, is so constituted, that whatever is true in any one case, is true in all cases of a certain description; the only difficulty is, to find what description.[7]

synthetic histories of science and philosophy. Nevertheless, one can question the sleight of hand Father Wallace performs in the following: "Fidelity to the historical aspect of the dissertation would rather demand that the term 'scientific' *be employed in the medieval understanding of scientia,* and would in fact be more applicable to philosophy than it would be to much of modern science. Still, the work is addressed to the modern mind, and there is *no intention* on the part of the author *to employ, from the outset, an understanding of the term 'scientific' which is alien to his readers"* [p. 22, italics mine]. It would seem that the problem is not to know whether Theoderic was a scientist or a philosopher, but whether his science is in specific continuity with modern science. This last problem can hardly be clarified by equating "at the outset" a meaning of science *not "alien" to the modern reader* and a *medieval* meaning of science. For the purposes of this paper, Father Wallace's observations on the traits of character and personality of Theoderic must be noted: "strong dislike for the 'communiter loquentes,' " "no respecter of persons," "merciless in dealing with adversaries," and reliance on "sarcasm and invective . . ." (pp. *18-19).*

7 Book III, ch. III, sect. 1 (New York, 1887), p. 223.

Implied in this analysis is a universe of *objects-observed,* but absent from it is the universe of *subject-observers.* In its own way, *The Logic of Scientific Discovery* of Karl R. Popper emphasizes the importance of the subject-observer by acknowledging that "every discovery contains 'an irrational element,' or 'a creative intuition,' in Bergson's sense," and by asserting bluntly that "there is no such thing as a logical method of having new ideas, or a logical reconstruction of this process."[8]

Let us propose that induction rests on a variable element which is the effort of human imagination or invention, human creativity or freedom, ahd that it can and must resolve itself in psychological matter, or, for want of a better expression at this time, in *subjectism.* This term is needed to distinguish the logic of induction, or analysed induction, from the relativity inherent in *subjectivism.* Just as it has been recognized that emphasis on individual experience characterizes the post-Aristotelian period with its climax in the Augustinian certainty of the inner experience, contemporary minds should be willing to examine the psychological dimension implicit in philosophic and scientific understanding in order to set up a completely methodic formalization of inductive methodology. Awaiting analysis therefore is Chrysippus' philosophy, with its increased feeling for psychological realism and its consequent emphasis upon conditions and environment, as well as upon the reliability of the normal individual and the need for individual training. Why not, then, push or extend the edge of objectivity to the basic subjective powers of identification, transposition, and other preparations for logical explanation? In this deepening of subjectivism into *subjectism,* would seem to lie the phenomenology of induction,[9] that is, in the analysis of the individual, and fixed conditions necessary to the discovery of "new" knowledge or of scientific intuition.

Such a grasp of the fullness of the reflexive act of the *cogito* or of the *ego* would seem to present itself as a proper

6 (New York, 1959), p. 32. The fact that there is no such thing as a logical method of having new ideas does not mean that there is no logical reconstruction possible. How can this "irrational element" be recognized, first, and qualified as "irrational," secondly?

object for the contemporary philosophical conscience. However, the very possibility of comprehending a living act of inductive research has already been challenged. Of course, it is rather easy and common to make synonymous the absence of the experimental proof of a fact and the denial of its possibility. W. H. W. Reade in *The Problem of Inference* maintains that the variable element, "cannot be reduced to rule.'"[10]

How, then, can there be a logic of induction if it is agreed that the assertion of an impossibility as the only possibility renders null and void all other possible possibilities? Independently of such an assertion, obviously, the possibilities are clearly not impossible, since they exist. This is true if one recognizes that the logic of one category is not the same as the logic of another. Furthermore, there is a difference between inductive logic, and the logic of induction.[11] In the logic of this or that category, we must communicate the system of communication as well as the subject matter communicated. At this level of expression, logic reveals its basic formalism, its rules of operations as well as its highly immanent character. This character of immanence is nothing more than the mind's return upon itself, or the mind's awareness of its operation.[12]

As far as the logic of deduction is concerned, this virtue of

9 Subjectivism would differ from subjectism precisely in this: that the former would view the subject-bound qualities as *accidental* in relation to scientific *or* inductive discovery, and vice-versa. (Cf. the account of the "accident" that led to Oersted's discovery of the action of an electric current on a magnetic needle in Ph. E. B. Jourdain, "An Accident That Led to a Notable Discovery," *The Monist,* XXII [1913], 39-40.) The latter would agree that the subject-bound qualities are accidents (all men are essentially only men), but not in relation to inductive knowledge, nor vice versa. One could hardly be happy that a knowledge which purports to be one of the highest developments of mankind be, *of its nature,* grounded in an accident — psychological, objective, or both. The discovery of the *per se* relations between subject-bound qualities and object-bound qualities constitutes the complete logic of induction.

10 (Oxford, 1938), p. 28.

11 Another way of showing this difference *would* be to point out that the histories of inductive sciences — those of Whewell, Duhem, Dampier, Sarton, Thorndike, *et al.* — are not histories of the logic of induction, although they could very well be means out of which a logic of induction could emerge.

12 *Summ. Theol.,* I, 87, 3, ad 2: "Alius est actus quo intellectus intelligit lapidem, et alius quo intelligit se intelligere lapidem."

immanence was illustrated and verified when Professor Miller of McGill University effected the deduction of deduction.[13] We readily concede, of course, that it is true by definition that induction is not deduction, and that modes of discovery differ among themselves just as modes of knowing do. As a field, the induction of induction remains to be explored and delimited. It would seem that logicians, psychologists, and philosophers should be greatly sensitive to the more recent remarks made by G. Holton and D. H. D. Roller in the *Foundations of Modern Physical Science* to the effect that "induction involves a leap of the creative mind; for there is no rigorous 'logic of induction' comparable to the rules that constitute the logic of deduction."[14] The first clause refers to a number of subject-bound qualities of scientists such as: "inspired guess," "intuitive hunch," "flash of imagination," "original ideas," "boldness," "seeing through," "single flash," "chance," "feeling," "good fortune," "grace," "right choice." These existential properties form the ultimate basis of reasoning by analogy, by relation, by contrast, by proportion, etc., which are involved in the logic of discovery. Excellence within these subject-bound qualities seems to characterize the scientific or investigative mind, however mysterious or magical these categories might be considered by the deductive logicians who must rule out all extra-logical elements while themselves partaking of subject-bound limitations.[15]

We must grant that it is immediately more rewarding to reduce logic to mathematics, induction to statistics, to probability theory, or to game theory, than it is to cling to an existential character in logic and in induction. If one holds that logic is a science of permissible inferences, one can readily see that the second clause of the Holton-Roller observation is not consequent upon the first. As such, that "there is no rigorous 'logic of induction' " entails two interpretations with two sets

13 *The Structure of Aristotelian Logic* (London, 1938), p. 25.

14 (Reading, Mass., 1958), p. 255.

15 This is acknowledged by Aquinas when he relates that "if a man syllogizes while asleep, when he wakes up he invariably recognizes a flaw in some respect" *(Summ. Theol.t* I, 84, 8, ad 2).

of possibilities each. The first interpretation is derived from emphasis on the copula, namely: "there *is* no logic of induction yet or as of now"; the second from an emphasis on the qualifier, "there is no logic of induction rigorous enough or exhibiting scientific rigor." By applying the principle of significant negation[16] to the first interpretation, the following set of possible inferences emerges:

a) there can be a rigorous logic of induction
b) there cannot be a rigorous logic of induction
c) there must be a rigorous logic of induction
d) there is a rigorous logic of induction

In this set, possibility (d) entails possibility (c) which entails possibility (a); and (a), (c), and (d) entail the impossibility of (b). The proof that (d) is a possibility entailing possibility (a) rests on the absolute law of the priority of reality over all forms and categories of the possible. Bergson has analyzed this absolue priority in these terms:

> ... le possible implique la réalité correspondant avec, en outre, quelque chose qui s'y joint, puisque le possible est l'effet combiné de la réalité une fois apparue et d'un dispositif qui la rejette en arrière. L'idée immanente à la plupart des philosophies et naturelle à l'esprit humain, de possibles qui se réaliseraient par une acquisition d'existence est donc pure illusion ... La vérité est qu'il faut plus pour obtenir le virtuel que réel, plus pour l'image de l'homme que pour l'homme même, car l'image de l'homme ne se dessinera pas si l'on ne commence par se donner l'homme et il faudra de plus un miroir.[17]

16 *Ibid.,* I, 13, 10, ad 5.
17 *La Pensée et le Mouvant* (Paris, n.d.), pp. 128-29.

Because it is not grounded, (b) is said to be impossible or meaningless. The most (b) could mean would be that it is probable. But when we say that any of the conditions for probability is absent, we can only mean that we have no evidence for its actuality. We cannot mean that it does not exist and that we are certain of its non-existence. If we are sure that it cannot exist, our certainty amounts to the certainty of the improbability of the event. Our denial of the condition on the basis of mere ignorance, therefore, cannot be a grounded denial. Our knowledge of its absence is based only on the absence of knowledge but not on positive proof of the non-actuality or unreality of the condition.

In the second interpretation with the emphasis on "rigorous," the meaning of rigoristic can be:

a) satisfactory
b) complete or general enough to cover most cases of inductive discovery
c) exact or systematic, that is formally detachable from cases of induced generalizations.

Obviously, this set reinforces the first interpretation, and it amounts to the outline of a project worthy of investigation. In such a project, the philosophers of science would be opening new frontiers of the mind in unison with Greek, medieval, and modern thought. And it would seem more rewarding an adventure for the future of mankind than simply showing that scientific progress in any age consists in the transmission, with or without rectifications, of the errors of a previous age. The formalization of induction in its full anthropological sense might prove to be a difficult task, but it is surely not an impossibility. However, as long as the logic of induction is thought of in the frame of deduction-induction, or universal-particular opposition, where the *particular* is considered a negligible inferior, there will be no rigorous logic of induction. Nor can we ever hope to perform an exact analysis of induction, if our basic logic is not existential. Historians of ideas and of logic have neglected long enough the subject-bound character of classical logic. To this day, the subject-bound elements of Aquinas' definition of logic have remained so much unanalyzed as to lead one to wonder by what force they have been transmitted.[13] In this description, would it not seem that the categories: *order, ease,* and *correctness* are projections of subject-bound-qualities?

Suppose that the "rules" of harmony, as observed by a group of composers, were formulated and then presented to an unmusical logician, with an invitation to compose a tune. Does anyone imagine that he could do it by virtue of his logic? Let us suppose now that the "rules" of logic — deductive or inductive — as framed in a system of logic, were presented to a tyro logician with the invitation to be logical. Would anyone imagine that he would be so by the mere virtue of logic? Is it not the case that in the human act of "learning," or any other human act, the abstract logical nexus between principles and conclusions, between subject and predicate, or between potentialities and actualities is only half the story? Moreover, inductive discovery presents this unique feature, that once it has been brought into being and made public by a creative act, the whole level of human understanding is not raised quite to the level of that inventiveness. This, obviously, is the first meaning of "new" truth, or of a "new" subject-predicate combination. The fact that every college student can now use Plato's and Aristotle's logic does not lift him to their intellectual level. The second sense of "new" truth, or "new" subject-predicate relation occurs when the full force of the combination, though well known to science (in the first sense), is for the first time grasped by another mind.

It would seem, indeed, that research into all the conditions of the *new progressio mentis* could be established. In keeping with the power of our age, the experimental method would first have to be set up and evaluated. Such endeavor would amount to nothing more than an application once more of Bergson's apt remark concerning modern science:

13 *In I Anal. Post.,* I, 1, n. 1.

> Que la science moderne ait créé la méthode expérimentale, c'est certain, mais cela ne veut pas dire qu'elle ait élargi de tous côtés le champ d'expérience où l'on travaillait avant elle. Bien au contraire, elle l'a rétréci sur plus d'un point; et c'est d'ailleurs ce qui fait sa force."

Moreover, a synthetic view of induction might reveal areas of basic agreement between philosophers of old and modern scientists. In the absence of such a synthesis, modern science is just as poor as ancient philosophy is rich. And let us note that between modern science and ancient philosophy stand unanalyzed the medieval topics and methodic categories, revealing in their own ways, the very foundation of substantial conceptual advance: the "wonder," the "dubia," the "videtur quod non," the "sed contra," the "responsio," the distinction, contradistinction, and suspension of belief and opinion. Basically, these methodic expressions reveal the initial unanalyzed conditions of the scientist's deep impulse to create or to discover.

In the absence of existing logical reconstruction of the mind's capacity for projecting new and significant hypotheses and correlations, it might be wise to turn to Aristotle and contemporary philosophers of science to discover the detachable and recurring subject-bound conditions leading to new conceptual schemes and new knowledge. At the beginning of the *Ethics* (1095a30-b9), Aristotle sharply contrasts the deductive and inductive methods:

> Let us not fail to notice, however, that there is a difference between arguments from and those to the first principles. For Plato too, was right in raising this question and asking, as he used to do, "are we on the way from or to the first principles?" *(Rep.* 511 B) There is a difference, as there is in a race between the

19 *V énergie spirituelle* (Paris, 1930), pp. 74-75.

> starting-point from the judges to the turning-point and the way back. For, while we must begin with what is known, things are objects of knowledge in two ways — some to us, some without qualification. Presumably, then, *we* must begin with things known to *us*. Hence any one who is to listen with intelligence to lectures about what is noble and just and, generally, about the subjects of political science *must have been brought up in good habits*. For the *fact* is the starting-point, and if this is *sufficiently* plain to him, he will not at the start need the reason as well; and the man who has been *well brought up* has or can *easily* get starting-points.[20]

Apart from this basic division, Aristotle's induction of induction is proposed in three different texts and contexts. The first text is that of *Meta.,* 1048a35-b6, where the context is that of a discussion of primitive or key philosophic terms such as act, whole, and part:

> The meaning of act that we are proposing can be known by induction, with the help of singular examples; since one must not try to define everything, rather one must be content to grasp the analogy; actuality then is as that which is building is to that which is capable of building, and as the waking is to the sleeping . . .

A second and more technical analysis of the limits of induction is given in the *Posterior Analytics,* where it is asked:

> How then by definition shall we prove substance or essential nature? We cannot show it as a deduction

20 The expression "known to us" implies the two levels or meanings of new truth mentioned above. The circumstantial conditions (good habits, well bringing up) should not be overlooked [italics mine].

> from the assumption of premises admitted to be facts — that is the method of demonstration; we may not proceed as by induction to establish a universal on the evidence of groups of particulars which offer no exception, because *induction proves not* what the essential nature of a thing is but that it has or has not some attribute . . .[21]

The third and surely the most important text for our purpose clearly grounds induction in the fullness of sense-perception and of its experimental supplementation. Again, it is found in the *Posterior Analytics* (81a38-b9) :

> It is also clear that the loss of any one of the senses entails the loss of a corresponding portion of knowledge, and that, since we learn either by induction or by demonstration, this knowledge cannot be acquired. Thus demonstration develops from universals, induction from particulars; but since it is possible to familiarize the pupil with even the so-called mathematical abstractions only through induction — since each subject genus possesses, in virtue of a determinate mathematical character, certain properties which can be treated as separate even though they do not exist in isolation — it is consequently impossible to come to grasp universals except through induction. But induction is impossible for those who lack sense-perception, because it is sense-perception that apprehends particulars . . .

Now as sensation is the recognized ground of empirical knowledge, would it not be proper to examine also some of the individual, existential, circumstantial qualities surrounding it? These qualities we have called subject-bound or subject-

21 92a35-bl.

limited units of the inductive-perceptive process. A fair enumeration of such necessary units, even if not a complete list, can be indicated in the logical works of Aristotle. They are:

The Criticism Unit

> Always in dealing with any proposition, be on the lookout for a line of argument both pro and con: and on discovering it at once set about looking for the solution of it; for in this way you will soon find that you have trained yourself at the same time in both asking questions and answering them. If we cannot find any one else to argue with, we should argue with ourselves. Select, moreover, arguments relating to the same thesis and range them side by side; for this produces a plentiful supply of arguments for carrying a point by sheer force, and in refutation also it is of great service, whenever one is well stocked with arguments pro and con: for then you find yourself on your guard against contrary statements to the one you wish to secure. Moreover, as contributing to knowledge and to philosophic wisdom the power of discerning and holding in one view the results of either of two possibilities is no mean instrument; for then it only remains to make a *correct* choice of one of them. For a task of this kind one must possess a certain natural ability, and *real natural ability consists in being* able correctly to choose the true and avoid the false. Men of natural ability can do this; for they *judge correctly* what is best by a *correct feeling* of love or hatred for what is set before them.[22]

This natural critical ability stressed at the conclusion of the *Topics* is also an object of emphasis in the second chapter of the first book of the same work: . . the ability to raise

22 163a36-b16.

searching difficulties on both sides of a subject will make us detect more easily the truth and error about the several points that arise . . . For dialectic is a process of criticism wherein lies the path to the principles of all inquiries."[23] Let this subject-bound unit be signified by *a.*

The Quickwittedness Unit

> Quickness of wit is a sort of flair for hitting upon the middle term without a moment's hesitation. A man sees that the moon always has its bright side facing the sun, and immediately realizes the reason : that it is because the moon derives its brightness from the sun ; or he sees someone talking to a rich man, and decides that it is because he is trying to borrow money ; or he understands why people are friends, because they have a common enemy. In all these cases, perception of the extreme terms enables him to recognize the cause or middle term.[24]

Let this subject-bound unit be signified by *b.*

The Sound State Unit

> ... to the same persons different things are more intelligible at different times — first of all the objects of sense-perception, and then, when their knowledge becomes more accurate, the converse occurs; and so neither would the same definition always have to be given to the same person by those who say that a definition ought to be given by means of what is more intelligible to each individual. It is obvious, therefore, that

23 101a34-36.

24 *Anal. Post.,* 89bl0-15. Aquinas' comment on this is that quick-wit "happens from natural aptitude, and also from practice" (lect. 44, n. 12).

> definitions ought not to be made by means of terms of this kind but by means of those which are more intelligible absolutely ; for only thus could one and the same definition be always produced. Perhaps, also, what is intelligible absolutely is what is intelligible not to everyone but only to those who are intellectually in a sound condition, just as also what is healthy absolutely is what is healthy to those who are physically in a sound condition.[25]

This unit, indicative of a good scale of values, can be symbolized by *c*.

The Common Sense Unit

> . . . beware of maintaining an hypothesis that is generally rejected. There are two ways in which it may be rejected. It may be one which leads to the making of absurd statements, for example, if one were to say that everything or nothing is in motion ; on the other hand, it may be one of those which a bad character would choose or which are contrary to our wishes, for example, that pleasure is the good and that to commit injustice is better than to suffer it. For men hate him who makes such assertions, regarding him not as maintaining them for the sake of arguments but as saying what he really thinks.[26]

Let *d* signify this unit.

75 *Topics,* 141b34-142al 1.
7b *Topics,* 160bl7-23.

The Method Unit

> We shall possess the method completely when we are in a position similar to that in which we are with regard to rhetoric and medicine and other such faculties; that is to say, when we carry out our purpose with all the available materials. For it is not every method that the rhetorician will employ to persuade, or the physician to heal; but if he omits none of the available means, we shall say that his grasp of the science is adequate.[27][28]

This methodic quality will be signified by *e*.

The Economy Unit

> It is also a fault in reasoning when a man shows something through a long chain of steps, when he might employ fewer steps, using material which is already existent in the argument, for example, when he is showing that one opinion is more truly an opinion than another.[20] . . . For reasoning always consists of a small number of premises.[29]

This subject-bound economy will be represented by *f*.

The Symbolism Unit

> For names are finite and so is the sum total of formulae, while things are infinite in number. Inevitably, then, the same formulae, and a single name, have a number of meanings. Accordingly just as, in counting,

27 *Topics,* 101b5-10.
28 *Topics, 162a25-26.*
29 *Topics,* 158a28.

> those who are not clever in manipulating their counters are taken in by the experts, in the same way in arguments too those who are not well acquainted with the force of names misreason both in their own discussions and when they listen to others.[30]

This last subjective ability which can be viewed as being as important and essential as sense-perception itself is signified by *T,* while sense-perception is represented by *S*. Since symbols of some sort are essential in fully conscious knowledge, the logic of induction or of discovery can be formulized in a tentative way if we let "new" knowledge be represented by *D;*

$$\overset{n}{\underset{x=t}{S}} \longrightarrow D, \text{ which expands into:}$$

$$S_t + S_a + S_b \ldots + S_n \qquad D.$$

Accordingly, the formal presentation of the necessary qualitative and quantitative conditions of *induced* knowledge comprises the following subject-bound units:

S = Sense perception (always constant)
x = The range of units (variables: a, b, c, d, e, f,... .) from T to n, where n specifies an unlimited range through the qualitative variables, a, b, c, d,... with T being the minimal unit
and (quantitative)
n being the maximal unit
D = New knowledge or discovery

This formula would seem to apply equally well to the two cases or levels of "new" knowledge or discovery. It goes without saying that this expression in logical language in no way

30 *Soph. Ref.,* 165a10-18.

can reveal the new or future discoveries themselves that will take place in natural science and philosophy. However, it might reveal the forms that future discoveries will take, and it remains a working formula by which the incidence and distribution of the subject-limitations of past discoveries can come to be discovered.

By way of conclusion we suggest that an ideal testing ground for this formalization of inductive research presents itself in the second volume of the *Harvard Case Histories in Experimental Science,* since the structure of the work, as well as the introductory remarks of the general editor, J. B. Conant, implies a recognition of the existential limitations of subject-observers,[31] and of an ideal model of logical analysis. Indeed, without an existing model of logical analysis, how would the following statement be logically meaningful or justifiable? "The origins of the working hypotheses are to be found almost without exception in previous speculative ideas or in the previously known observations or experimental results. Only rarely, however, do these broad working hypotheses seem to have been the product of a careful examination of all the facts and a *logical analysis of various ways of formulating a new principle."*[32]

31 Especially the auto-biographical information of Robert Boyle setting out to confirm the plants-atmosphere investigation of Van Helmont, pp. 330ff. (Cambridge, Mass., 1957).

32 Introduction, p. xi, vol. I [italics mine].

LÉON Lortie
Director of the Extension Department,
University of Montreal

Physico-Chemical Methods and the Philosophy of Nature

Although experimental science can trace its methods back to the techniques and empiricism of the seventeenth century, the lonians were the first to understand that nature is intelligible. By observing and reasoning, they looked for natural, as opposed to mythological, causes. Eventually they sought a principle without which nothing could come to be and out of which everything that exists comes into being and behaves in the way it does. From the fragments that have remained of their works, we see that the lonians were not concerned with moral problems and that their intellectual outlook, at least for the oldest among them, was limited to an understanding of the physical world. They were the fathers of the philosophy of nature, and except for Anaximander, whose thought is still a subject of debate, they searched for a material principle of all things.

It was Pythagoras, the founder and leader of a religious sect, who was the first to say that his pursuit was a love of wisdom (ignoring his moral teachings, which are mostly legendary). He stated that harmony is perfection, and here indeed is the very foundation of his physics and cosmology. Very little remains of the teaching of Pythagoras, and it has been so much tampered with that we cannot be sure what the master really said or taught. However, we know enough to enable us to discover what relationships he established between numbers, geometry, music, and astronomy. He distributed dots, representing the finite or the limited, separated by a void representing the unlimited, in such a way as to form a straight line, or an area such as an equilateral triangle, a square, or a rectangle, or a solid such as a three-sided pyramid.

Triangular Numbers	Square Numbers	Rectangular Numbers

All triangular, square, and rectangular numbers were regarded as the summation of simple arithmetical progressions.

$1 + 2 + 3 \ldots + n =$ Van $(n + 1)$ triangular numbers
$1 + 3 + 5 \ldots + (2n - 1) = n^2$ square numbers
$2 + 4 + 6 \ldots + 2n = n(n + 1)$ rectangular numbers

Pythagoras was thus able to show that the arrangements of the dots proceeded in a natural order having its own particular and internal logic and giving rise to regular geometrical areas and solids — the foundation of mathematics and geometry. He believed that this mathematical and geometrical arrangement was responsible for the existence of all that is harmonious in nature: “Everything is number.”

But this might have been passed off as a mere play of the mind had it not been for a physical phenomenon which was completely reducible to mathematical formulation. The lengths of the strings that produced the consonant intervals of the diatonic scale, those of octave, fifth, and fourth — which Pythagoras established in the first experiment to be recorded in the history of science—were in the proportions $\frac{2}{1}$, $\frac{3}{2}$, and $\frac{4}{3}$, in which he found the sum of the first four integers to be 10, the "perfect number" or *tetractys.* This was represented as the perfect triangular number in the form of an equilateral triangle. Such a physico-mathematical relationship is said to have led Pythagoras to one of his most treasured discoveries, that of the harmonic proportions, one of which is 12:8:6. The harmonic mean of 12 and 6, is 8, and the octave is given by strings in the proportion $\frac{12}{6}$, the fifth by $\frac{12}{8}$, and the fourth by $\frac{8}{6}$.

What we have seen here is the first known example of a physical theory based on observation, formulated in coherent mathematical language, and verified by an experiment. Whether the experiment or the mathematical theory, comes first does not matter. Mathematics can and indeed does develop by itself, and a physical phenomenon can be represented by existing mathematical logic. But the data obtained in the experimental study of a natural or man-made phenomenon often lead to the discovery of hitherto unknown mathematical developments and new ways of handling numbers and dimensions.

The doctrines of Pythagoras, and of the Milesians as well, were soon subjected to the devastating dialectic of Parmenides. In his "Poem," he visits a goddess who warns him that he shall "learn all things, as well as the unshaken heart of the well-rounded truth, as the opinions of mortals in which there is no true belief at all." The Way of Truth is the only conceivable, valid way of search. Starting with the affirmation of the principle of identity, Parmenides dismisses as unthinkable and false the Pythagorean void and the Milesian principles of perpetual change (motion and becoming) as contrary to that which we now call "being." The Way of Opinion deceives us by the appearances of our senses.

The Poem of Parmenides is the parting of the way between philosophy, based on ontology, and science, based on the mathematical study of motion. Democritus alone will challenge Parmenides by affirming that the void between his atoms is necessary to explain the structure of things and that the atomic motions are eternal. His atomic theory is coherent and, fundamentally, it is still used by modern scientists. The remarkable achievements derived from the atomic theory have only been possible, it has been said, because scientists were bad metaphysicians.

Most scientists, when they think of philosophy, have only Aristotle in mind — his *Physics* and his *a priori* method of deduction. Not so long ago it was the fashion to despise the *Physics* and to accuse Aristotle of having unduly delayed the advent of experimental science. A more favorable attitude is now exhibited by many of those who have greatly contributed to the rise of modern physics and by most, if not all, of those who have cared to study Aristotle's works and to look at them in their historical context and background. One cannot deny, however, that the *Physics* of Aristotle is obsolete, at least in that part dealing with the four elements and the four qualities from which they arise, with his distinction between hot and cold, heaviness and lightness, and all that is derived therefrom. The same thing is true of his *De Caelo.* His *History of Animals* and *Parts of Animals* are often models of painstaking observation. He is rightly recognized as the father of zoology and even as a forerunner of the theory of evolution. But apart from good factual observations, very little still remains that is in accordance with the views of modern biologists. Obsolescence is the fate of many scientific theories. Those of Descartes, Newton, and Darwin, even if they fared much better, have also been subjected to criticism. Their theories were more or less superseded by more advanced views derived from the study of new phenomena.

When a scientist thinks of Aristotle's philosophy, he is concerned only with his philosophy of nature, and, according to special interests, with his physics or his zoology. The *Metaphysics,* or first philosophy, is usually a scientist's nightmare, and he deplores the fact that the *Physics* is so much dependent on it. How does it happen that these two disciplines, which appear so irreconcilable to the mind of a modern scientist, are inseparable in Aristotle's approach to the study of nature? It seems hard to understand how a man who really created the scientific method of analysis, synthesis, and reduction to practice, should have been led to use two methods of approach which appear so irreducibly opposite.

Aristotle was evidently a man of his time, and he tried to solve the main philosophical problems that had baffled his predecessors since Parmenides. He was the first to study systematically the opinions of his predecessors, and he tried to reconcile the views of those who believed in the reality of "things which are more knowable and certain to us," and of those who believed in the reality of things "which are clearer and more certain in themselves."

Like his contemporaries, Aristotle was limited to the observation of natural phenomena, but his observation was often superior. His only tools were his senses and his logic. Many, including Roger Bacon and Francis Bacon, forgot that Aristotle was the first to use induction as a method of discovery. But valuable as this tool may be, Aristotle was not equipped to use it as we use it today. His analysis of natural phenomena stopped short after he was satisfied with the obvious. From the scanty information he had gathered, he went on directly to generalize, coming for instance to his concepts of perfect and imperfect bodies, hot and cold, wet and dry, light and heavy. All of these notions, in his time, were matters of common sense. Aristotle's physics is in accord with that "common sense" which makes us believe that the sun is circling the earth. A French physicist, Jacques Cabannes, once said that "common sense is often the old habit of a long established error." When Aristotle confronted his synthesis with what he saw around him, common sense was again satisfied.

Let us examine one particular case. All sublunar bodies were divided into heavy and light, and as any well-ordered house wife or secretary would say, there is a place for every thing and everything should be in its place. The natural motion of an imperfect heavy body is to seek its own place, which is the center of the earth, i.e., downward. Once a body is there, it is at rest, and because it is inert, it cannot be moved by itself. A heavy stone cannot be moved unless we push or pull it (forced motion), and according to Anaxagoras, the motor is separated from the mobile. The harder we push, the faster the stone moves. We need to overcome the effect of the weight of the body and of friction, which account for the resistance to motion. From this observation Aristotle concluded that speed is proportional to force and inversely proportional to resistance. At first sight, that looks very good. Coming back to falling bodies, Aristotle still separates the mobile from the motor — heaviness — which exerts its force on it, and concludes that the heavier the body, the faster it falls. This is supposed to be readily observed by anyone. From this, Aristotle will argue against the existence of a vacuum. If a vacuum existed, all bodies would fall with infinite velocity, he says, because there would be no resistance to their motion downward. Or still better, if they did fall with a definite speed, their velocities would all be the same! The erroneous conclusion of Aristotle is partly due to faulty observation and partly to a very inadequate analysis of the phenomenon, but it also rests on the use of metaphysical principles. Science has its own way of doing things and has achieved quite different results.

But what about the very basis of Aristotelian philosophy of nature — matter and form? It is not involved in the example we have given and, without resorting to it, Aristotle could have come, less easily perhaps, to the same conclusion as he actually reached. The fact is that one of his disciples, Straton, made a more careful analysis of falling bodies and observed that heavy bodies descending from a greater height struck the ground with greater violence than those falling from a lesser height. But the place of matter and form in this whole problem is another question to which we shall return. Matter and form were long rejected by scientists as useless notions and, in fact, they are — until the scientist comes to think of what lies beyond his own analysis of nature. That is where and when science becomes a real philosophy of nature as opposed to the empiricist natural philosophy in which scientists have believed for close to three centuries.

We must distinguish between science as a complete understanding of nature and experimental science. Without the work of the latter, the reasoning and theories of the former would not be possible. Many authors have written on the experimental method, on the logic of scientific discovery, and on the philosophy of science. It is a vast subject which cannot be reduced to a few paragraphs, and I shall not attempt to describe it. The best way to know something about it is to see it at work. I have chosen a few examples where we may discover how it is applied and how it calls, in each particular case, for special methods of dealing with the subject. Some of these examples will lead us from the early stage of the experimental study of a phenomenon to the concepts of modern physics.

We shall begin with heat. For Aristotle, hot and cold were two different sensible qualities. But everybody could know that metals, water, and even air expand when they are heated, and that they contract when they are cooled. Galileo and, later on, the members of the Academia del Cimento, in Florence, made an experimental study of this phenomenon, and they noticed that they could determine the temperature of the body of a person in good health and of one who was sick, by observing first the expansion of a certain quantity of air confined over water, and then by the expansion of water in a narrow tube. They also found that air, or water, contained in a flask with a long and narrow neck, which was brought from room temperature to that of ice water, kept on contracting until their volume remained constant as long as ice and water were in contact. In this way, the Florentine academicians were able to show that the transition between hot and cold was continuous and that a cold body was only less warm than a warmer body. They also discovered two fixed points of the thermometer: the ice point and body temperature.

In the 18th century, Fahrenheit went to Spitzberg to measure more intense cold, obtaining such temperatures by mixing ice with ammonium chloride. Using tubes with a smaller bore, filling them with alcohol and then with mercury, he sealed them at the top and devised the awkward kind of thermometric scale that is still used in English-speaking countries. The Swedish scientist Celsius later devised the centigrade scale. Thus physics was provided both with a proper scale and a working instrument to determine the degree of heat of a body.

Not only could the degree of heat be measured but a Scottish chemist, Joseph Black, also measured quantities of heat in his calorimeter. He showed that water and alcohol, when heated in the same manner, do not reach the same degree of temperature in the same time. It takes less heat to warm alcohol than water, if the volumes or weights of both are equal. When he immersed equal weights of iron, copper, mercury, and silver, heated to the same temperature, in the water of his calorimeter, Black found that in each case the temperature of the water was different. He reasoned that heat could be transferred from one body to another and that the quantity of heat which was being lost by one was *completely* taken up by the other. He could claim this because heat was considered a fluid, an immaterial substance, which flowed like water and which filled receptacles. This view dates back at least to Newton, whose corpuscular theory of the nature of light inaugurated the idea of imponderable particles and fluids. Heat fluid was called "caloric"; there were also electric and magnetic fluids. Such concepts were the result of Descartes' mechanistic philosophy and of Newton's use of hydrodynamics in his study of Cartesian vortices. The general idea of a fluid in this context also goes back to the medieval interpretation of Aristotelian philosophy which prompted people to materialize the "essence" of a substance and try to isolate it by destructive distillation or by solvent extraction. We still speak, for instance, of the essence of vanilla — a relic of the attempt by the alchemists to find philosophical answers to the problems they encountered in the practice of their craft. The belief in the concrete existence of fluids was so firm during the 18th century that many physicists tried to store electric fluid in a pail of water. (The Leyden jar originated in this way.)

If a substance can be filled with caloric, or with electric fluid, that substance has a given capacity, like a bottle. Hence the name *heat capacity,* which Black gave to the quantity of caloric that a certain substance can hold. Here we see in action one of the mainsprings of the experimental method, reasoning by analogy with something familiar. This requires imagination, leading to a central hypothesis and to a working model. The caloric fluid was hypothetical, and it was eventually submitted to an experiment which would show whether it was to be retained or rejected. Benjamin Thompson, who was supervising the making of cannons for the king of Bavaria, could not understand why so much water was needed to cool the metal in which a hole was bored with a metallic drill. If an alloy like bronze could be filled with caloric, there could only be a definite quantity of it in a given quantity of metal. But the "caloric" that was driven out of it seemed to be inexhaustible. At the same time, Thompson noticed that there was a certain proportionality between the work performed by the horses who drove the boring machine, and the amount of water that was heated when the drill worked its way into the metal. He did not draw any definite conclusion, and he did not pursue his experiments or his reasoning.

Flame is another aspect of heat. Boyle defined it, in the 17th century, as a burning smoke. But why does a substance burn? The German physician Ernst Stahl asked himself that question, and true to the Aristotelian tradition of hidden qualities, he answered that it was because it had the quality, or *vis,* of combustibility, which he called "phlogiston." This reminds us of a scene in Molière's *Le malade imaginaire* in which one of the comic characters asks in macaronic Latin:

Mihi docto Doctore
Domandatur causam et rationem quare
Opium facit dormire:
A quoi respondeo,
Quia est in ea
Virtus dormitiva
Cujus est natura
Sensus assupire.

Stahl's followers, in good 18th-century fashion, materialized his phlogiston as one of the imponderable, invisible substances and even invested it with a negative weight. When a substance like tin or lead burned, it was considered to have lost its phlogiston but at the same time, it gained weight. Such an anomaly was represented by the equation $W - (-w) = W + w$.

Lavoisier made a thorough analysis of the phenomenon of combustion in air confined over water in a closed vessel. He found that in each case the same volume of air disappeared, a fact which, a century earlier, Boyle, Lower, and Mayow had already noticed. Mayow had postulated the existence of "nitro-aerial particles" in air but Boyle, true to the title of his book, *The Skeptical Chymist,* would not advance any hypothesis as to the nature of the part of air which had disappeared. Lavoisier then found that the weight of a substance, after combustion, was equal to its weight before combustion plus the weight of the part of air that had disappeared. Sometimes in Lavoisier's experiments, the combustion left no visible residue, but he found that it had dissolved in water, as an acid; he neutralized it and found that its weight agreed with his other results. He was not happy, however, because while he was sure that a part of the air had taken part in the process of combustion, he did not know what part it was since he had not isolated it. When Priestley told Lavoisier that he had isolated from a substance derived from mercury an "air" in which a candle burned more brilliantly than in common air, the French chemist was able, first, to produce that substance by burning mercury in air and, then, to obtain from the product of that combustion the gas that had previously eluded him.

From these experiments, and basing his arguments on a principle that had already been stated by Anaxagoras, that of the conservation of matter, Lavoisier drew the well-known conclusions that combustion is the combination of a substance with part of the air, accompanied by production of heat; that air was a mixture; that substances previously called "earths" by chemists were the results of combustion. As to water, Cavendish had shown that it resulted from the burning in air of "inflammable gas." Lavoisier found other ways of proving the same thing and, in the preface of his *Elements of Chemistry,* he could write:

> The notion of four elements, which, by the variety of their proportions, compose all the known substances in nature, is a mere hypothesis, assumed long before the first principles of experimental philosophy or of chem-

> istry had any existence. ... All that can be said about the number and nature of elements is, in my opinion, confined to discussion entirely of a metaphysical nature. The subject only furnishes us with indefinite problems which may be solved in a thousand different ways, not one of which, in all probability, is consistent with nature. I shall therefore only add upon this subject that, if by the term *elements,* we mean to express those simple and indivisible atoms of which matter is composed, it is extremely probable we know nothing about them; but if we apply the term *elements* or *principle of bodies,* to express our idea of the last point which analysis is capable of reaching, we must admit, as elements, all the substances into which we are capable, by all means, to reduce bodies by decomposition. Not that we are entitled to affirm, that these substances that we considered as simple may not be compounded of two or even of a greater number of principles; but since these principles cannot be separated, or rather since we have not hitherto discovered the means of separating them they act with regard to us as simple substances, and we ought never to suppose them compounded until experiment and observation has proved them to be so.[1]

This excerpt from Lavoisier's preface is most important not only because it is the real breaking point between the Aristotelian tradition of the four elements and the modem concepts of chemistry, but also — and still of greater significance — because it is a beautiful example of experimental philosophy. It affirms the provisional nature of scientific knowledge, and why it is provisional. Science can always be improved, but the basis must be such that it can accommodate change when new experimental facts are discovered, or that it can be amended, if necessary, to fit unexpected developments.

[1] Lavoisier, *Elements of Chemistry,* XXII-XXIII, translated by Robert Kerr (Edinburgh, 1790).

Lavoisier's statement is much more adaptable to such changes in our science than was Dalton's concept of the indivisible atom. Of course, Dalton was following an old established tradition and his merit rests less on the atomic hypothesis than on his theory that each simple body or element has individual atoms; that each of the atoms has a weight of its own and is responsible for the chemical properties of the element; and that chemical combinations are due to a combination of individual atoms according to certain rules which he derived from the chemical analysis of compound substances.

When Dalton stated that 1 atom of A plus 1 atom of B produce 1 atom of AB, it should have appeared awkward that an atom of AB could be indivisible. It did not occur to him that it was a contradiction in terms. He was even more surprised and shocked when Gay-Lussac's experiments showed that 1 volume of chlorine plus 1 volume of hydrogen produced 2 volumes of HCl. His reaction was to state that the experimental results were wrong. There was a way out of this troublesome situation. It was found by both an Italian magistrate who was an amateur chemist, Amadeo Avogadro, and the French physicist, Ampère. Translated into modern language, it consisted in distinguishing between the atom and the molecule, the latter being composed of two or more atoms, even in the case of simple bodies if they were in the gaseous state at room temperature. In this way, Dalton's theory was reconciled with experiment: 1 vol. H_2 + 1 vol. Cl_2 2 vol. HCl; or more simply $H_2 + Cl_2 \rightarrow 2\,HCl$.

No other scientific hypothesis is as coherent and consistent as the molecular theory. From further experimentation, calculation, and reasoning, it has been possible to study the shape and structure of every complex molecule, (x-ray diffraction analysis has confirmed, as if they had been actually photographed, the shape and structure of the molecules.) Complete synthesis of a great many natural substances have been achieved, based on the model that had been deduced from their physical and chemical properties. In this way, chemistry has gained an intimate knowledge of things that cannot even be seen under an optical microscope.

Once the molecular hypothesis was accepted by chemists, further developments led to the formulation of the kinetic molecular theory of gases, which linked the heretofore unrelated domains of matter and energy. This theory had been developed in the 18th century by Daniel Bernoulli, from mathematical considerations, to explain the behavior of gases under pressure. Internal pressure is exerted on the walls of a container by the impact of molecules in constant agitation striking the walls. The temperature of the gas is not only linked with the greater or lesser agitation of the molecules but is measured by, and identified with, the kinetic energy of the molecules. We must notice that Francis Bacon had already come, qualitatively, to a similar conclusion in his celebrated example of inductive reasoning: "Heat is an expansion restrained, and striving to exert itself with the smaller particles."

The further history of the atomic and molecular theory is a fascinating, even exhilarating one; the theory is still firmly established. As long as the atom is considered as the smallest particle of a given simple body, nothing can challenge its validity. It is the result of a mechanical philosophy of nature based on Newtonian principles, on Lavoisier's definition of a simple body, on Dalton's atomic individuality, and on Avogadro's distinction between the atom and the molecule. In this respect it is not an exaggeration to say that chemists do not deal so much with an atomic *theory* as with actual facts about atoms.

Far from objecting, when physicists proposed that the atom was a complex structure, chemists were happy. They expected that this would provide them with new tools to discover why atoms behave as they do. Investigation of subatomic phenomena was then initiated. That is where and when the trouble started because most of these investigations concerned subatomic energy rather than particles themselves.

Our knowledge of energy is factual so long as we do not venture to ask questions as to the very nature of energy itself. To the layman, the term "energy" is far from being unequivocal. Its simplest meaning is the ability to produce work. There are many forms of energy, all transformable one into another according to the principle of the conservation of energy. Heat, electricity, magnetism, chemical reactions, light, sound — all can be put to work; and it has been shown experimentally that a given quantity of each can be transformed into an equivalent quantity of the other, with the reservation that heat cannot be entirely transformed into any of the other forms. This leads to the second principle of thermodynamics, that of the degradation of energy. We do not know anything about the nature of energy nor of its absolute value. All that we can measure is the difference in energy between two states of a system. But this is in keeping with other concepts of physics. We can measure length only as the distance between two points, and time as the interval between two instants.

All forms of energy are properly studied in different chapters of physics, but the concept of energy has been integrated through the principle of its conservation. Heat is studied in three different ways. Thermodynamics is the strictest positivistic approach to the study of the properties of heat without any asumption as to the nature of heat. It studies only the relations between thermic and mechanical phenomena, and it is based on the two above-mentioned principles of transformation and conservation—and a third one, the concept of entropy. This is a mathematical concept required for the coherence of the system. Thermodynamics is essentially a mathematical theory, and it requires that all transformation and variations, in order to be treated by the methods of infinitesimal calculus, be continuous. Heat is also studied in the kinetic theory of gases. Finally, heat is studied as a radiation, because it is an important part of the spectrum of solar light and also because it can be produced and studied as radiant heat emanating from a hot but non-luminous body.

Light, and for that matter heat considered as radiation, is the subject matter of optics and gives rise to two different sets of phenomena. All the facts that were known up to the end of the 19th century were beautifully explained by the wave theory. This theory originated in the 17th century and was based on the analogy with the nature of sound, which had been experimentally demonstrated to be due to longitudinal vibrations of air. Newton was not satisfied with this undulatory view and substituted a corpuscular theory. But Thomas Young, using the particle theory of light, was not able to interpret the interference patterns produced when light is diffracted through a very small hole. Nor could he explain such patterns with the wave theory as proposed by Huyghens. But with a theory based on transverse vibrations the French physicist Fresnel was able to explain the patterns. This theory relied on calculus and hence required that variations be continuous. It rested on the hypothesis that there is a vibrating medium, the ether.

Electricity and magnetism developed separately during the 18th century, but they were definitely shown to be intimately related when the Danish physicist Oersted found, in 1819, that the needle of a compass deviated from its natural position when an electric current was brought close to it. Ampère was quick to establish a physical and mathematical theory of electromagnetism which was highly successful both in its theoretical consequences and its useful applications. To all intents and purposes, electricity and magnetism could no longer be separated from one another after Faraday had discovered the phenomenon of electromagnetic induction and explained it by the hypothesis of magnetic lines of force.

Up to the middle of the 19th century, studies of light, electricity, and magnetism had developed independently. But about that time, many attempts were made to unify concepts and to establish a system of units that would make it easier to correlate various fields of knowledge. Thermodynamics was one of the syntheses brought about by this effort to unify concepts and standardize measuring systems. Clerk Maxwell was carrying out a commission to establish a system of electric and magnetic units of measurement when he was struck by some analogies between the velocity of light on the one hand and some electrostatic and electromagnetic units on the other. He was also impressed by the fact, discovered by Joseph Henry, that the discharge of an electric condenser, such as the Leyden jar, was in the nature of a wave and could extend to a great distance. Out of this and, of course, after a thorough mathematical study of the actions of electric charges and magnetic poles, Clerk Maxwell proposed his electromagnetic theory of light. It was predicted that electromagnetic phenomena would be found to be propagated in waves, like light, in an hypothetical ether and with the speed of light. If, instead of producing only damped waves, a condenser could be made to produce standing waves, Maxwell's theory would be confirmed. His prediction came true when Hertz succeeded in producing the type of waves which are now transmitted in space as radio and television signals.

The "Gay Nineties" was not only a brilliant social era but, even more, a period of elation for scientists who thought that, with the atomic and the electromagnetic theories, and also the evolutionary theory, they had delivered nature of its secrets. Every single phenomenon could be explained in terms of two fundamental units: matter, under the form of individual atoms; and energy, the variation of which was in a continuous flow. Life resulted from the interplay of matter and energy, and mind itself, in the words of Hippolyte Taine, "secreted thought just as the liver secreted bile." All that could be discovered in the future would be mere consequences of the body of scientific knowledge then accumulated and expressed in a few principles and theories. A French writer, Léon Daudet, has written a book entitled *Le stupide 19ème siècle.* Although he mainly discussed literary and philosophical theories, we are tempted to say that he was even more perspicacious than he thought, when his views are applied to the overconfident scientists of that period. Their faces were soon to become red when they were confronted with a series of unexpected scientific discoveries for which they were completely unprepared.

The discovery of X-rays could not have been predicted by any of the then reigning theories. It opened new avenues in uncharted scientific domains. The discovery of radioactivity was one of the unsuspected consequences of Poincare's interpretation, wrong but fruitful, of the nature of Roentgen's x-rays. After Marie Curie had discovered radium as an outcome of her study of the rays emitted by uranium, the validity of the principle of the conservation of energy was contested and the concept of the indivisible atom was also questioned. The prevailing view of the atom had already been questioned when J. J. Thomson had announced his discovery of the electron. Fortunately for the atomic theory, Rutherford's idea of atomic disintegration of radioactive substances, as the emission of electrons and helium nuclei, gave a firm footing to a new concept of the atom as a complex structure. According to this view, the components of atoms were held together by intra-atomic forces, the breaking of which produces thermal and gamma radiations. Later on, Mendeleev's periodic classification of the elements, which had much earlier been proved to be right with its prediction of the discovery of new elements, was further shown to be a real law of nature when a whole family of elements, the inert gases, came from nowhere and filled an unsuspected gap in the list of known elements. Mendeleev's periodic chart was finally consecrated when radioactive elements and the existence of isotopes were discovered.

On the other hand, the 19th century views of thermodynamics and the electromagnetic theory of light fared more poorly. They were found to be incompatible with the laws of black-body radiation and with photoelectricity. When Michelson and Morley were unable to detect any effect on the velocity of light due to the movement of the ether, more doubts were cast upon this hypothetical fluid, the theory of which already suffered from many inconsistencies in the formulation of its properties.

In order to explain the experimental behavior of the black-body, Max Planck devised his theory of quanta in which energy was regarded as being emitted in small particles, or quanta, instead of in a continuous wave. Einstein showed that light quanta or photons could also explain the photoelectric effect. In the same year (1905) he further took the world of physics by surprise with the announcement of his theory of relativity, a consequence of which was the equivalence of mass and energy. Many of these new concepts were incorporated by Niels Bohr in his model of the atom, which explained the emission of lines in atomic spectra. This would have been impossible if, like Rutherford, he had remained within the resources of Newtonian mechanics.

Why was it that classical physics and mechanics, the use of which had been so successful in explaining natural phenomena, suddenly broke down between 1890 and 1900? It did not really break down completely. It only fell short of explaining what it was intended to explain. However, 19th century scientists were carried away by their enthusiasm at the truly remarkable achievements of classical physics and, in their celebrations, they overlooked certain phenomena which did not fit into the body of theory which they had used as a basis. In short, their analysis was far from adequate. The discrete constitution of line spectra and the discrete charges of electrolytic and gaseous ions and of cathode rays could not be explained by Maxwell's theory. Scientists in the 19th century also had forgotten the provisional nature of experimental knowledge, which is always subject to the discovery of unforeseen experimental phenomena. They had too literal a belief in Newton's thought that . synthesis consists in assuming the causes discovered and established as principles, and by them explaining the phenomena proceeding from them, and proving the explanation." They forgot however that in the same paragraph of his *Optics,* Newton had said:

> . . . analysis consists in making experiments and observations, and in drawing general conclusions from them by induction, and admitting of no objections against the conclusions, but such as are taken from experiments or other certain truths. For hypotheses are not to be regarded in experimental philosophy. And although the arguing from experiments and observations by induction be no demonstration of general conclusions, yet it is the best way of arguing which the nature of things admits of, and may be looked upon as so much the stronger, by how much the induction is more general. And if no exception occur from phenomena, the conclusion may be pronounced generally. But, *if at any time afterwards any exception shall occur from experiments, it may then begin to be pronounced with such exceptions as occur.*

Scientists of the 19th century had avoided hypotheses so often that, when they had to deal with that of the ether, they did not know how to handle it. They did not see the internal contradictions it contained and put too much faith in the value of one confirmation, forgetting that one successful prediction is not proof of the truth of an entire system based on the hypothesis in question. They were concerned only with the coherence of the calculations derived from a set of interrelated units defined with great precision. This gave them a false sense of security. They were very confident in Newton's statement — "But to derive two or three general principles of motion from phenomena, and afterwards to tell us how the properties and actions of *all* corporeal things follow from those manifest principles *would* be a very great step in philosophy ..." — but they did not pay attention to the last words of this sentence . . *though the causes of these principles* were not yet discovered; and therefore I scruple not, to propose the principles of motion above mentioned, they being of very general extent, and *leave their causes to be found out."*

This must not be taken to mean that 19th century physics is devoid of value or significance. It is generally agreed that as long as classical physics does not deal with subatomic phenomena or particles moving at approximately the speed of light, and when it is concerned with facts that come within its terms of reference, we have not found any other theory that fits experimental knowledge so well. The concepts of classical mechanics are those of common sense, more refined than those of Aristotle because they are submitted to a rigorous mathematical formalism. These concepts are not defined according to their nature but only as dimensions that can be measured with suitable instruments in a three dimensional space. Modern physics cannot avoid using the very concepts of classical physics, such as wave and particle, position and speed, mass and energy. But instead of being applied to two different sets of phenomena, those of wave and particle are used to describe the properties of one single thing — the electron. Evidently, unless one forgets entirely the principle of identity, the electron cannot be at the same time a wave and a particle, two concepts that are mutually exclusive. We come back to the same opposition with which the first Greek philosophers were confronted, and it is not only a problem of logic, it is a mathematical opposition as well. The mathematical formalism that is used for describing the wave aspect is the classical one of differential calculus; it requires that a process be continuous and that from one point the next one can be known with a certain accuracy. The formalism that applies to the particle aspect is statistical, which only lets us know the probability of an individual event.

Until Niels Bohr, in 1920, introduced them in his attempt at building a model of a hydrogen atom, the quantum and the relativity theories had only been used in special cases to get better agreements between theoretical calculations and experimental data. In his model Bohr used these devices to remove some of the difficulties which Rutherford had not been able to resolve by relying only on classical mechanics. But Bohr's model was soon found to be far from adequate: it could not explain an increasing number of experimental facts. The orbiting electrons and their "jumps" from one orbit to another were only crude attempts at representing what goes on inside the atom.

Mathematical physicists were desperately trying to find a unified concept of the electron. Louis de Broglie, in 1924, was the first to reconcile the wave and particle aspects: a moving particle of the size of the electron corresponded to a "matter wave." This concept was mathematically formalized by Schrodinger in his wave mechanics and soon confirmed experimentally by the discovery that a beam of electrons could be diffracted — a wave phenomenon — by a thin sheet of metal. Thus, an almost pure mathematical construct led to an important experimental discovery. At the same time, many other theoretical physicists were busy developing quantum mechanics (which is also exclusively mathematical in its language).

Actual experiments are still performed by modern physicists, but theoretical physicists, in order to test the validity of their theories, resort to ideal experiments. They submit the concepts of classical physics, under conditions imposed by quantum mechanics, to their mentally devised schemes in order to test the coherence of their constructs. Quantum mechanics is a most abstract branch of mathematical physics and allowance must be made for the part that the scientist himself plays when he applies it to the study of phenomena. All measurements are made with instruments the readings of which are in units based on classical concepts. The precision of these readings is far from absolute, and the physicist knows that he cannot depend entirely on their accuracy. Furthermore, it is now known that, because of the smallness of the objects that are submitted to an experiment, the tools — like x-rays and gamma rays — which are acting on an electron, disturb it. This is further reason to doubt the accuracy of a measurement. Whatever this accuracy may be, the very nature of quantum mechanics, which is statistical and relativistic, has led theoretical physicists to the conclusion that, though they can know with a certain precision (or lack of precision which they can calculate) the position of an electron at a certain time and its position at a later time, they cannot know what has happened to it in the meantime, nor can they know its speed. Conversely, if the physicist concentrates on the determination of that speed, he loses sight of its position. What precision is gained or achieved in the measurement of one, is lost in the measurement of the other. And the product of these two inaccuracies is a constant — Planck's constant — divided by the mass of the particle. This strange equation was called a relation of uncertainty or indeterminacy by its discoverer, Heisenberg, who concluded that "the old concepts fit nature only inaccurately." Bohr, who had a more solid faith in the validity of these concepts, considered them to be true at the same time and mutually complementary.

The method of modern physics and the knowledge of nature that can be derived from it have completely changed. Because no physical picture of quantum phenomena is possible, quantum physics is not representative. Because of the part played by tools like x-rays or other radiations, quantum physics is not as objective as classical physics was thought to be. In the words of Heisenberg, the experimental sciences describe nature as our method of investigation reveals it to us. Because it deals more and more with "concepts," modern physics has entered the field of abstraction and is considered to be idealistic rather than realistic. In a curious way, however, a man like Heisenberg, who displays a vast knowledge of philosophical ideas, hit upon something that is very close to one of Aristotle's most important concepts: the principle that he used to explain change and local motion. In their many attempts at unifying the wave and particle aspects of the electron, Bohr and his associates had developed the concept of "probability waves," which led to the incorrect result that the laws of the conservation of energy and of momentum were only statistical laws. But, according to Heisenberg:

> The probability wave of Bohr, Kramers and Slater, however, meant ... a tendency for something. It was a quantitative version of the old concept of "potentia" in Aristotelian philosophy. It introduced something standing in the middle between the idea of an event and the actual event, a strange kind of physical reality just in the middle between possibility and reality.[2]

Furthermore, since, according to Einstein's equation $E = mc^2$, elementary particles can be, at very high energy, created from kinetic energy or annihilated into energy, Heisenberg concludes that

> all the elementary particles are made of the same substance which we may call energy or universal matter; they are just different forms in which matter can appear.
>
> If we compare this situation with the Aristotelian concepts of matter and form, we can say that the matter of Aristotle, which is mere "potentia," should be compared to our concept of energy, which gets into "actuality" by means of the form, when the elementary particle is created.[3]

2 W. Heisenberg, *Physics and Philosophy* (New York, 1963), p. 41.
3 *Ibid.,* p. 160.

Does this mean that philosophy and science are coming closer together? Heisenberg's approach is only one of the many philosophies of science. We cannot help noticing that the present situation has much in common with that which existed at the time of Parmenides. He was confronted with the opposition of the continuous, the *plenum,* and the discontinuous, which required the theory of the void. We are also facing the opposition between waves, the variations of which are continuous, and particles, which are essentially discrete. Furthermore, even if we do not accept Eddington's idea of science as being made of pointer-readings, we must admit that all that we know of space and time is the difference between two points or two instants; that mass is defined only by the equation $F=ma$, m being the constant factor of proportionality between the variables F and a. We know that substances are composed of atoms which are complex structures of electrons, protons and neutrons. We know that mesons are intermediate between electrons and protons, but we do not know what their nature is. No wonder then that in this predicament, scientists like Heisenberg turn to Aristotle, not as a model perhaps, but as a thinker whose study of nature can provide some help in man's search for the understanding of the deepest recesses of nature.

The study of modern physics is governed by one of two principles — uncertainty and complementarity. (The term "indeterminacy" is preferable to "uncertainty" because it does not imply that we shall never be sure of anything in our knowledge of the physical world.) While Heisenberg himself feels that this may be due to the imperfection of our present methods of investigation, Niels Bohr, on his part, takes for granted that the wave theory and the particle theory are both true, each in its field of application, and that we cannot reconcile them except by considering them as complementary.

The more we look at Aristotle's method and at Newton's, the more we notice that they are composed of exactly the same steps. But the tools as well as the spirits of approach are completely different. It is inevitable that the results obtained should be different, though not opposite or contradictory. Philosophical method cannot be encumbered with experimentai tools and mathematical analysis, which are completely alien to its ultimate aim. On the other hand, the scientific method cannot be put to work unless it defines quantities and dimensions strictly in terms that can be measured materially. In this sense, it reminds us of a coffee mill, in which we put grains at one end only to collect ground coffee at the other. When modern physics is concerned with concepts like the ones now in use, it cannot help coming in closer contact with philosophy but without being resolved into an existing philosophy. The results of both methods, the philosophical and the scientific, can only be reconciled by considering them as complementary.

James A. Weisheipl, O.P.
Professor of the History of Science,
Albertus Magnus Lyceum

The Evolution of Scientific Method

TO THE BEST of my knowledge the history of scientific method has not yet been written. While there are many histories of science and many excellent studies of method for particular periods and individuals, no full history of scientific method has yet been attempted.[1] One major obstacle to such a history, no doubt, is the vagueness, or perhaps confu-

1 Besides the classical works of William Whewell, Ernst Mach, Pierre Duhem and Karl Pearson, a number of recent studies of particular methods can contribute to an eventual history of scientific method. Special mention should be made of the following: R. McKeon, "Aristotle's Conception of the Development and the Nature of Scientific Method," J. *Hist. Ideas,* VIII (1947), 3-44; Emile Simard, *La Nature et la Portée de la Méthode Scientifique* (Quebec, 1956); Herbert M. Evans (ed.), *Men and Moments in the History of Science* (Seattle, 1959); R. M. Blake, C. J. Ducasse, E. H. Madden (ed.), *Theories of Scientific Method: The Renaissance through the Nineteenth Century* (Seattle, 1960); Neal W. Gilbert, *Renaissance Concepts of Method* (New York, I960); Robert McRae, *The Problem of the Unity of the Sciences: Bacon to Kant* (Toronto, 1961).

sion, which exists concerning the very concept of scientific method itself. It is by no means uncommon to find the scientific method defined in such vague terms as "observation" and "experimentation." The closest Henri Poincaré came to defining the expression in his famous *Science et méthode* (1909) was in his opening line: "The scientific method consists in observing and experimenting; if the scientist had at his disposal infinite time, it would only be necessary to say to him, 'Look and notice well.' "[2] But such a declaration fails to define either "scientific" or "method."

More commonly in current studies the scientific method is defined as "the exact measurement of physical properties and the formulation of hypotheses in equations which permit the mathematical manipulation of these quantitative results." But this definition unreasonably restricts the concept of scientific method to the field of modern physics, thereby excluding vast areas of modern science from having a method which is truly "scientific." The life-sciences for the most part are not amenable to the method of mathematics, and we have not yet succeeded in reducing substantial parts of chemistry, paleontology, geology, minerology and climatology to useful equations and mathematical hypotheses.

Since the seventeenth century, theoreticians of science have assumed that there must be only one method which is truly "scientific." In other words, it is assumed that the concept of scientific method is a *ttnivocal* one. No attempt is made today to justify such an assumption. Descartes, at least, argued that his method had worked for analytic geometry, and that therefore it should work for all knowledge. Descartes' argument, of course, is logically unsound. Apparently, however, it has been universally accepted as a necessary postulate in modern methodology. In order to obtain this univocal concept, philosophers and historians of scientific method have had to select one particular aspect of scientific procedure: observation of facts, induction, experimentation, measurement and mathematical deduction, hypothetical postulation, predictabil-

2 H. Poincaré, *Science and Method,* in *The Foundations of Science* (Lancaster, Pa., 1946), p. 359.

ity, or possibly verification and falsification. The attempt to form a univocal concept necessarily entails the exclusion of other recognizably essential elements. The real tragedy of this univocation is not so much the exclusion of some particular characteristic, however, for the excluded element is usually rescued by some other author. The real tragedy is rather the loss of the concept of method itself.

Consequently, before examining the three stages of evolution which I have selected, it would be well for us to determine what we mean by method, and specifically what we mean by scientific method.

I. *The Concept of Scientific Method*

Our English word "method" comes directly from the post-classical Latin transliteration of the Greek μζΌο , which does not even occur in Aristotle's great methodological work, the *Posterior Analytics,* although it does occur elsewhere in Aristotle.[3] Derived from *μετά,* meaning "after" or "according to," and οδὸ , meaning a "way," the Greek compound originally was taken to mean the "way," "order" or "logic" of rational inquiry *(ratio inquirendi).* In this sense it signified the rules, or norms according to which logical inquiry was to be conducted. In this first sense logic itself was said to be a method. The word was then transferred to signify the actual discussion or inquiry conducted according to a logical plan. In this second sense we speak of the Socratic "method." Finally, the word was taken to mean any doctrine or science obtained as a result of this logical inquiry. In this last sense there would be as many "methods" *(μέθοδοι)* as there are sciences or even schools of doctrine. We find this last sense of the term used frequently by Galen and by early ecclesiastical writers when they refer to Platonic, Peripatetic, Epicurean, and Stoic philosophies as so many different "methods."

The classical Latin term for the concept of method, and one which St. Thomas Aquinas invariably uses, is *modus.* Cicero had no need to coin a new philosophical term to express the nuances of the Greek term. It was not until the sixteenth

---

3 *See* H. Bonitz, *Index Arislotelicus* (Berlin, 1870), pp. 449.450.

century that *methodiis* came into common Latin usage under the egis of humanist learning.[4] By that time the subtleties of *modus* had been lost. The Latin *modus* originally meant a "measure" or "norm" according to which something is measured, for example, its size, circumference, quantity, or bulk; in this first sense a mode was a standard of measurement for qualities as well as for quantities.[5] Soon, however, the term was taken passively to mean the determination within a thing because of an extrinsic measure; thus a mode was taken as a limit, a restriction imposed by some standard, as when we speak of a mode or manner of life, and of moderation in activity. It was in this second sense that St. Augustine said, *"Mensura modum praefigit."*[6] Invariably it was this sense of the term that St. Thomas used whenever he defined *modus*.[7] In other words, there are two uses of the Latin term *modus* corresponding to the first two uses of the Greek *μέθοδοι.* Strictly speaking, the Latins did not use *modus* in the third sense employed by the Greeks; instead, they used such terms as *doctrina, scientia* and the like.

Considering both the Greek and Latin uses of the term, we must distinguish two fundamental senses of "method" or "mode," namely the objective and the subjective uses of the term corresponding to its first two meanings in classical usage.

"Method" or "mode" can be taken as the objective measure or norm to be followed in any procedure or endeavor. In rational inquiry this objective guide is none other than logic, the

4 N. W. Gilbert, *op. cit.,* p. 69, fn.4. In this otherwise excellent study Gilbert completely neglects the more common Latin term *modus* used by Cicero and the schoolmen. Ironically, the author blames Cicero for not transliterating the Greek *μέθοδοι:* "Evidently Cicero did not consider the concept worth baptizing with a new Latin word. . . . The result of Cicero's omission was that the specific Greek concepts of method were lost in the vagueness of circumlocution in Latin philosophy, only to be regained when writers using Latin once more had access to Greek works" (*ibid.,* p. 49).

5 *See* A. Ernout and A. Meillet, *Dictionnaire étymologique de la langue Latine,* 4th ed. rev. (Paris, 1960), pp. 408-409.

* St. Aug., *De Gen. ad lit.,* IV, 3. PL 34, 299.

7 *Sum. Theol.,* 1, 5, 5; I-II, 49, 2. For a more detailed listing of this usage in St. Thomas, see the *Tabula Aurea* of Peter of Bergomo (Rome, 1960), *s.v.* 'Modus.'

art of right reasoning. Both the Greeks and the Latins recognized Aristotle's *Organon* as the general method of all scientific knowledge, *"quia logica tradit communem modum procedendi in omnibus aliis scientiis."* St. Thomas goes on to say, "It is absurd for a man to seek simultaneously science and the methodology belonging to science; for this reason he should learn logic before learning the other sciences."[8]

More commonly, however, *modus* is taken in the subjective sense of a modification, a determination, a modality within a subject because of the objective norm. This corresponds to the second usage in Greek and Latin. In English we have many expressions reflecting this sense of the word: one's manner or mode of life, musical modes, modes (or moods) of speech, different ways or manners of viewing, and moderation as a characteristic of virtue. Philosophy even speaks of various "modes" of being. The Latin word *modus* is rendered in many different ways in English, but fundamentally they all express the same sense of the term. We could render it as "method" — but then there are as many methods or moods as there are, let us say, virtues.

Even a casual consideration of the virtues in general shows that the method of one virtue differs greatly from that of every other virtue. The method of prudence, for example, differs considerably from the method of speculative knowledge. Prudence aims at determining the means suitable for a composite end to be attained; for this reason the ancients called its method "synthetic" or "compositive" *(modus compositivus)*. Speculation, on the other hand, even in practical sciences, starts with a complex problem and attempts to resolve it into its simple principles, causes and elements; for this reason the ancients called the speculative method "analytic" or "resolutive" *(modus resolutivus)*.[9] There should be no need here to

[8] St. Thomas, *In II Meta.,* l. 5, n. 335 (ed. Cathala).

[9] "Necessarium et enim in qualibet scientia operativa, ut procedatur *modo composito.* E converso autem in scientia speculativa, necesse est ut procedatur *modo resolutorio,* resolvendo composita in principia simplicia." St. Thomas, *In I Ethic.,* lect. 3, n. 35 (ed. Pirotta). Also *Sum. Theol.,* I-II, 14, 5. However, even practical sciences, sciences like ethics, economics, and politics,

point out that "wisdom" has a unique method proper to itself, and that the method of sacred theology is radically different from the method of all the philosophical sciences. The great differences in *modus* are seen even more conspicuously in the moral virtues as discussed by St. Thomas. All the virtues belonging to justice derive their mode from *ius,* which is a *medium rei,* while the virtues belonging to fortitude and temperance derive their mode from a *medium rationis.*

In this paper we are concerned specifically with methods of scientific knowledge, and the first point to be established is that each science has its own modality or method. The problem here has nothing to do with general logic, for it has already been established that logic proposes the general method of all scientific inquiry. The present question has to do with the special methodology of individual sciences. The discussion of this methodology does not even belong to general logic, but rather to the individual sciences themselves. St. Thomas notes that "the method proper to the individual sciences ought to be discussed at the beginning of each science."[10]

St. Thomas, following Aristotle, frequently points out that one cannot expect to use the same method in all the sciences, nor can he expect to find the same certitude.[11] Commenting on Aristotle's statement that "the mathematical method is not that of natural science,"[12] St. Thomas explains that the clarity and precision of mathematics ought not to be expected in all areas

use the speculative method *(modo speculativo)* when they search for definitions, divisions and arguments in their theoretical part. This use of composition and resolution should not be confused with the convertibility of demonstrations *"quia" per effectum convertibilem* (resolution) and *propter quid* (composition) discussed by Aristotle in *Post. Anal.,* I, 13. 78a 30-40. On the various senses of *compositio* and *resolutio* in St. Thomas, see L.-M. Régis, O.P., "Analyse et synthèse dans l'oeuvre de s. Thomas," in *Studia Mediaevalia* in honor of R. J. Martin (Bruges, 1948), pp. 303-330, and S. E. Dolan, F.S.C., "Resolution and Composition in Speculative and Practical Discourse," *Laval Théologique et Philosophique,* VI (1950), 9-62.

10 "Modus autem proprius singularium scientiarum in scientiis singulis circa principium tradi debet." *In II Meta.,* l. 5, n. 335.

11 *Boeth. De Trin.,* expos, c. 2, ed. B. Decker (Leiden, 1955), pp. 158-160.

12 Arist., *Metaph., a,* 3. 995a 16-17 St. Thomas, *In II Meta.,* l, 5, n. 336.

of research, but only in those areas which are treated abstractly and quantitatively. Consequently, the mathematical necessity found in mathematical definitions and demonstrations cannot be found in natural phenomena, which regularly occur *"ut in pluribus."* St. Thomas goes on to say:

> Since that most certain method of argumentation [found in mathematics] does not befit natural science, one must first examine carefully what is nature *(quid sit natura)* in order to discover the proper method of natural science. It will be obvious then what sort of reality natural science is concerned with. Further, one ought to consider whether natural science is one science, and whether the investigation of all causes and principles belongs to it or to different sciences. In this way one can know the method of demonstrating proper to natural science. Aristotle himself discusses this method in the second book of the *Physics,* as is evident to the perceptive reader *(ut patet diligenter intuenti)*.[13]

Thus it is clear that Aristotle and St. Thomas, at least, recognized different kinds of scientific method. It is not so easy, however, to understand what they meant by a "method" proper to each science. Defining *modus* in general, St. Thomas simply states that it is a *"determinatio sive commensuratio principiorum, seu materialium, seu efficientium ipsam [formam]."*[14] In this definition there are three factors to notice: (1) it is an intrinsic determination or configuration imposed by some objective norm: *"mensura modum praefigit"*; (2) it is both the material and the efficient principles which are thus modified; and (3) this radical molding is responsible for the type of reality *(species)* under discussion, and hence it is prior to the form.

[13] *ibid.,* n. 337.

[14] *Sum. Theol.,* 1, 5, 5: "Utrum ratio boni consistat in modo, specie et ordine."

The reality or *species* under discussion is scientific knowledge, and scientific knowledge is an intellectual habit of perceiving causal connections between true “reasons” and true facts. For this reason Aristotle defined scientific knowledge (èrurr^) as knowing “that the cause from which the fact results is the cause of that fact, and that the fact cannot be otherwise.”[15]*Following the thought of Aristotle, the schoolmen distinguished two essential principles of scientific knowledge : the active principles, which they called the *ratio formalis obiecti,* and the material principles of the intellectual habit, which they called the *obiectum materiale.*[16] The former designated the proper illuminating principles of the science; the latter represented any particular conclusion illuminated by the principles. Together they constitute the proper object of a science, for within that totality a truth is known *(scitum)* through its scientific causes. The radical modification, or configuration of these essential principles gives to each science its unique character, so that truths known in mathematics or in ethics, let us say, have an entirely different character from those known in the natural sciences. This is the function of the intrinsic *modus* determined by objective reality. Because of this radically unique modality, mathematics, ethics, and natural science are specifically different scientific habits.

At this point it must be explicitly acknowledged that a given method or *modus* belongs not only to scientific knowledge already possessed, but also to the acquisition of that science. In modern parlance the word “method” is more commonly used to designate the procedure or manner of acquiring scientific knowledge. But it would be a mistake to limit the concept of method merely to the acquisition of science, since knowledge acquired continues to bear the seal of its origin. It is the same extrinsic “measure” which determines both the manner of acquiring and the manner of knowing scientific truths.

One further point remains to be clarified in order to understand the concept of method in general. We must examine

15 Arist., *Post. Anal.,* I, 2. 71b 10-12, trans. H. Tredennick, LCL, p. 29.

16 *Sum. Theol.,* II-II, 1, 1; 9, 2 ad 3. See also I II, 54, 2 ad 2; *De Virt.,* 13; *In I Post. Anal.,* l. 41, n. 11.

the factors which actually determine the method of a particular science. These factors are complex. St. Thomas wisely observes that

> the method followed in investigation must be appropriate *(congruere)* both to things and to us: for unless it is appropriate to the things studied, these could not be grasped, and unless it is appropriate for us, we could not comprehend.[17]

The appropriate method of a particular science, therefore, is determined both by the subjective requirements for comprehension and by the objective nature of the field to be investigated.

Among the subjective requirements of a fitting method there are at least three obvious determinants. The first is logic itself, since logic is the indispensable art or tool *(organon)* for acquiring all scientific knowledge. As an indispensable tool, it implants a common method on all science, and for this reason it was antonomastically called "method" among the Greeks. The second determinant is the appropriate faculty of mind used; thus observation and reason are said to be the method of natural science, imagination the method of mathematics, intellectual intuition the method of metaphysics, introspection a convenient method of psychology, and so forth.[18] The third determinant is the knowledge already possessed by the investigator. Whatever is already known must be taken into consideration, since human knowledge must proceed from what is known to what is unknown. For this reason the teacher must choose examples carefully and present problems clearly in reference to the pupil's experience. There are perhaps many more subjective determinants of method, but these are sufficient for our purpose.

The objective determinant of method is also complex, but in a different way. The method of a science is objectively

17 *In Boeth. De Trin.,* expos, c. 2.

18 Cf. *In Boeth. De Trin.,* VI where St. Thomas examines Boethius' statement: "In naturalibus igitur rationabiliter, in mathematicis disciplinaliter, in divinis intellectualiter versari oportebit neque diduci ad imaginationes, sed potius ipsam inspicere formam."

determined not only by the object studied as a whole, but also by its various parts. Scholastic writers commonly recognized that the objects studied in natural science actually exist in sensible matter and motion, and that consequently every definition in natural science must be in terms of matter and motion.[19] But it makes a great difference whether one examines this reality in general or in particular. For the basic problems of common experience, ordinary observation is sufficient; for the internal biology of animals, however, dissection is necessary; for chemical reactions, experimentation is required; and for random sub-atomic particles, a statistical method is called for. A similar latitude in methods is required in the moral sciences, mathematics, metaphysics, and even in history. Therefore, the proper method employed in the various parts of a single science depends on the type of thing considered.

In general we can say that the basic method of all speculative sciences is *resolutive,* or *analytic,* proceeding from a complex whole to its causes, principles and elements. This common method of scientific knowledge is inevitably due to human logic. The proper method, however, is a further modification derived from the type of reality studied, both as a totality and as a complex whole made up of many different parts. This situation is similar to the case of the historical sciences. All the historical sciences use the "historical method." But this method is further modified and varied, depending upon whether one is doing economic history, political history, intellectual history, or the history of science.

With these preliminary observations in mind we can now consider three particularly notable stages in the evolution of scientific method: (1) the Aristotelian method, (2) the scholastic method, and (3) the Galilean method of the seventeenth century.

II. *The Aristotelian Method*

In my preliminary remarks I have assumed that Aristotle's

[19] Boeth., *De Trin.,* c. 2. Cf. St. Thomas, *In Boeth. De Trin., N,* a. 2; *In I Phys.,* l. 1, n. 2; *In I De Caelo,* l. 1, π. 2; *In HI De Caelo,* l. 3; Zπ *VI Meta.,* l. 1, nn. 1156-59; *In XI Meta.,* l. 7, nn. 2256-58; *Sum. Theol.,* I, 84, 1.

Organon is to be considered the general method of all scientific knowledge. Following the great Greek, Arab, and Latin commentators, I view the *Organon* as a single, complex tool of science, wherein discussion of the first and second operations of the mind *(Categories, Peri hermeneias)* is directed to an understanding of argumentation. The *Prior Analytics* discusses the formal structure of all argumentation, whether the argument be demonstrative, dialectical, or sophistical. The material structure of demonstrable matter, dialectical matter, and sophistical matter is then discussed in the *Posterior Analytics, Topics,* and *Sophistici Elenchi* respectively. In this view, the *Posterior Analytics* is the principal part of logic and its crowning glory, since it deals with the acquisition of "scientific knowledge" strictly so called. For this reason St. Albert says: *"Est ergo finis et perfectissima et sola simpliciter desiderabilis inter logicas scientias et sola nobilior et aliis certitudine probationum excellentior."*[20] Scholastics generally, and Averroes, Grosseteste, St. Albert, and St. Thomas in particular, accepted Aristotle's *Posterior Analytics* as an authentic treatise in scientific methodology.

There are two modern views, however, which reject this scholastic interpretation. Some modem philologists and logicians claim that the *Posterior Analytics* is a disjointed, unintelligible hodgepodge of possibly distinct treatises. When it was recently pointed out to one such logician that St. Thomas, for one, had no difficulty in seeing the unity, the logician retorted, "But St. Thomas even saw unity among the letters of St. Paul !" It might have been pointed out to this logician that the two cases are not at all the same.

The second and more serious view suggests that the *Posterior Analytics* represents a beautiful Platonic ideal of science, but insists that it has nothing to do with the procedure Aristotle himself follows in actual investigation. John Herman Randall, Jr., for example, believes that "this Aristotelian conception of science, as set forth in the *Posterior Analytics,* is still the Platonic Idea, the ideal, of our modern scientific enter-

20 Albert, *Lib. I Post. Anal.,* tr. I, c. 1, ed. Borgnet, II, 2b.

prise."[21] But he maintains that the work was never intended to be a description of scientific method. For Randall, Aristotle's actual method of scientific investigation is to be found in his scientific treatises. This method is seen to consist of five steps:[22]

1. "to determine the object of investigation," for example, the soul in *De anima,* natural motion in the *Physics,* human happiness in the *Ethics;*
2. "to examine previous opinions or hypotheses as to the best way to understand the subject matter in question";
3. "to undertake a dialectical examination of proposed *archai* or *endoxa* ... to bring out all the difficulties and problems";
4. "to find the relevant facts"; and
5. "to explain the subject matter, to exhibit the intelligible structure of facts."

The obvious scholastic reply to Randall's view is that the very purpose of the *Posterior Analytics* is to analyze steps 1, 4, and 5 for any science whatever. Steps 2 and 3 are dialectical preparations for scientific knowledge, and they are governed by the general principles of the *Topics.* Therefore, logic does discuss and offer the general method for all scientific investigation, as we have been saying.

After describing Aristotle's actual scientific method Randall goes on to say:

> Why then did Aristotle not only fail to make discoveries that seem to us through long familiarity obvious; why did he make positive mistakes? He had a fruitful method, what most scientists would still today call the "right" method . . . The answer is clear: Aristotle was

21 J. H. Randall, Jr., *Aristotle* (New York, 1960), p. 42.
22 *Ibid.,* pp. 51-55.

> too much of an empiricist. He was clearly the greatest observational scientist until the nineteenth century ; and our modern scientific enterprise was born in the rejection of such "empiricism" for some form of “rationalism” — in the rejection of trust in sheer observation for faith in mathematical demonstration.[23]

Whatever may be said of Randall’s enthusiasm for mathematical demonstration, one can find much wanting in his presentation of Aristotle’s *Posterior Analytics.*

Undoubtedly there are many obscurities in Aristotle’s *Posterior Analytics,* as even Themistius discovered.[24] For John of Salisbury, the book “contains almost as many stumbling-blocks as it does chapters.”[2526] However, the essential points of Aristotle’s work are not impossible to understand; at least they were not beyond the comprehension of the schoolmen. At the risk of over-simplifying, I would like to summarize the essential points in three paragraphs.[25]

First, scientific inquiry consists in asking questions — in asking the right question at the right time. The answer to the question can be found, not by remembering (Plato), nor by the addition of another fact (Sophists), but by investigation of the matter in terms of the question asked. One does not ask a scientific question unless one has a scientific problem : some contrariety of fact, view, or opinion. In every question the inquirer already has some knowledge. He knows at least the existence of and probably some definition of the subject, and presumably the predicate conveys some meaning, otherwise he could not and certainly would not ask the question. Furthermore, the questioner knows the basic truths of human intelli-

=3 *Ibid.,* p. 56.

24 Themistius, *Paraphras. in lib. Post.,* praef., ed. M. Wallies, *Comm, in Arist. Graeca,* V, 1 (Berlin, 1900), p. 1; trans, by Gerard of Cremona, ed. J. R. O’Donnell, C.S.B., *Mediaeval Studies,* XX (1958), 242.

25 John of Salisbury, *Metalogicon,* IV, c. 6, ed. Webb (Oxford, 1929), p. 171.

26 For a more detailed presentation, see my commentary on the *Post. Anal.,* entitled *Aristotelian Methodology* (pro manuscripto), River Forest, Ill., 1958.

gence. Therefore, the questioner does know a great deal when he asks about what he does not know. Of course, he does not yet know the answer or the reason for the answer, but these can be found only in the light of what he knows. In other words, an answer to a scientific question is not found in spite of the question but precisely in terms of the question and the problem. We know scientifically when we know not only the correct answer but also the *precise reason* for that answer, and that there is no other explanation. Thus, whether we are right or wrong, we claim to have scientific knowledge "when we believe that we know (a) that the cause from which the fact results is the cause of that fact, and (b) that the fact cannot be otherwise" *(Post. Anal.,* I, 2. 71 b 10-12).

Second, there are only four scientific questions: *An sit? Quid sit? Qualis sit?* and *Propter quid sit?* While it is true that the principal concern of scientific investigation is the answer to *Propter quid sit?,* the ultimate answer cannot be found except within the nature, essence, or *quod quid est* of the subject responsible for the phenomenon under investigation. For this reason the cornerstone of Aristotle's scientific method is the search for definitions which can serve as the middle term in a scientific demonstration. Just as the entire force of a demonstration lies in the middle term of the syllogism, so the ultimate explanation of a phenomenon lies in the nature of the subject. Clearly it is not just any definition which will serve this purpose. Plato had already given the two basic methods of finding definitions, namely by division and comparison. These are merely the starting point for the discovery of "demonstrative" definitions: *propter quid* definitions of the predicate (phenomenon or attribute) involving the subject as cause. Once this kind of definition has been found, perhaps after much research, one need go no further. Such a definition is itself an implicit demonstration, "differing from demonstration in grammatical form" *(Post. Anal.,* 11,10. 94 a 12-13).[27]

Third, and most important, proper and adequate demonstrations can be found — if they are to be found at all — only within the proper subject matter of the science. For Aristotle, arguments drawn from common sense or from common principles are not scientific demonstrations, although they may be highly indicative, that is, dialectical. Of great importance is Aristotle's insistence that arguments drawn from another area cannot be properly demonstrative. In other words, the answer to a mathematical problem must be found in mathematics, and not in ethics or in history. The solution to a problem of natural science must be found in the realm of natural science, not in theology, metaphysics, or mathematics. Here is the rub in the history of scientific method. For Aristotle, a mathematical middle term could not give the *propter quid* answer to a problem in natural science, for that middle term belongs to another area of study (to yews τo *υποκείμενον).* The application of mathematical principles, or middle terms, to natural phenomena could, at best, bring into being a new area of research, called a middle science *(scientia media)* between pure mathematics and natural science. It could not give adequate explanations of natural phenomena.

Here I have sketched only the scientific method discussed by Aristotle in the *Posterior Analytics. I* have said nothing about the principles of the dialectical method so necessary in solving scientific problems; these principles are discussed in the *Topics.* Nor have I said anything about the historical method used by Aristotle to introduce his scientific problems. Nor have I discussed the proper method *(modus proprius)* of Aristotle's natural sciences. Such an undertaking would be

27 Critics have often complained that there are few syllogisms in Aristotle's scientific works. However, the formal expression of the syllogism is not necessary for true demonstration; the main thing is to discover the middle term, the reason for asserting the conclusion. In *propter quid* demonstrations this middle term is nothing more than the causal definition of the property in question. "Et sic ex ipso *quod quid est* noto per sensum vel per suppositionem, demonstrant scientiae proprias passiones, quae secundum se insunt generi subiecto circa quod sunt. Nam definitio est medium in demonstratione propter quid" (St. Thomas, *In VI Meta.,* l. 1, n. 1149). Critics have also complained that there are relatively few real demonstrations in Aristotle's works. But every scientist realizes that perfect and complete answers are hard to come by; often, despite much research and speculation, one must fall back on the opinions of others, on probabilities, possibilities, and hypotheses. These are steps in the direction of demonstrations not yet reached.

too vast and not really indispensable for an understanding of the scholastic method.

III. *The Scholastic Method*

The scholastic method is one of the great glories of the Middle Ages; yet its history has not yet been written, and its nature has not yet been adequately explained.[26] What is commonly known as "Scholasticism" must not be imagined as a "body of doctrine." Neither was it essentially "a point of view regarding faith and reason," as it is sometimes claimed. Nor was it "the systematic use of reason in theology." This last expression, however, comes closer to the truth than any of the earlier, but it is not sufficiently accurate to satisfy a medievalist. Scholasticism is essentially a method of inquiry *(modus inveniendi)* which arose in the schools of the Middle Ages and was universally accepted as the best method of teaching *(modus docendi).* For the schoolmen the best method of teaching was a reasonable re-creation of the original discovery. Thus the order of teaching *(ordo doctrinae)* was said to follow the order of discovery *(ordo inventionis)*.[29] It is truly amazing how Aristotelian in spirit this method was even before the introduction of the "new Aristotle" into the Latin West. To appreciate the historical development of the scholastic method, we should distinguish the original elements of the scholastic method from the later influence of the "new Aristotle," particularly the influence of the *Posterior Analytics.*

From its earliest, obscure beginnings there were two essential parts to the scholastic method: the *lectio* and the *disputatio.*

A most valuable beginning is the incomplete work of M. Grabmann, *Die Geschichte der scholastischen Methode,* 2vols. (Freiburg i. Br., 1909-1911). See also "Die scholastische Methode und Literaturformen," in Ueberweg-Geyer, *Grundriss der Geschichte der Philosophie,* 12 ed., (Basel, 1951), pp. 152-7; G. Paré, A. Brunet, P. Tremblay, *La Renaissance du XIle siècle, Les écoles et l'enseignement* (Paris-Ottawa, 1933); H.-D. Simonin, "Qu'est que la scholastique," *Vie intell., X* (1931), 234-242; M.-D. Chenu, *Introd. à l'étude des. Thomas d'Aquin* (Paris, 1955), pp. 51-60.

29 See M. A. Glutz, C.P., "Order in the Philosophy of Nature," *The Dignity of Science,* ed. by J. A. Weisheipl, O.P. (Washington, 1961), pp. 268-271.

The disputation was certainly the more original and the more characteristic, but the *lectio* was its foundation.[30]

The basis of all medieval teaching was the Master's lecture, or commentary on the official text accepted as the *auctoritas.*[31] From the very beginning the Bible was the only official text in theology. In arts the *auctoritas* was Cicero for rhetoric, Priscian and Donatus for grammar, Aristotle for logic. In the thirteenth century the rest of the Aristotelian books were incorporated in the faculty of arts. The *Decretum* and other collections became the official text in Canon Law, and Avicenna's *Canons of Medicine* became the main text in medicine. The schoolmen were convinced that students should learn from the best masters available. The study of these "great books" of human knowledge, as we have said, constituted the basis of medieval teaching.

While commenting on the text, however, certain obscurities of the author would present problems. Even early twelfth-century Masters would digress to state the *pro* and *contra* of the case before attempting a solution. By the middle of the twelfth century the occasional digressions became more numerous and elaborate, and collections of *sic* and *non* authorities and arguments were made. Such was Abelard's famous *Sic et non* in philosophy (assuming that Abelard was the author of this well-known collection). Doctors of Canon and Civil Law likewise collected conflicting legislation and interpretations of law. Thus the problem gave birth to the question, the scientific question.

With the evolution of the *quaestio* came the disputation as a distinct part of the scholastic method, conducted at a distinct time of the academic day. Generally, the lecture on the text

30 "In tribus igitur consistit exercitium sacrae scripturae: circa lectionem, disputationem et praedicationem. . . . *Lectio* autem est quasi fundamentum et substratorium sequentium............*Disputatio* quasi paries est in hoc exercitio et aedificio, quia nihil plene intelligitur fideliterve praedicatur, nisi prius dente disputationis frangatur. *Praedicatio* vero, cui subserviunt priora, quasi tectum est tegens fideles ab aestu et a turbine vitiorum" (Peter Cantor, *Verbum abbreviatum,* c. 1. PL 205, 25).

31 On the technical meaning of "auctoritas," see M.-D. Chenu, *La Théologie au XII* siècle* (Paris, 1957), pp. 353-357.

was given in the morning, and the disputation on some point in the text was held in the afternoon. Originally the order of questions to be disputed followed the order of the text. A text like the Bible, however, hardly offered an order which could be called systematic, but by the middle of the twelfth century Masters had achieved a certain systematic ordering of the fundamental questions following the articles of the Creed. Similarly in the arts, problems were discussed with a certain semblance of order among the various questions, but here there was much to be desired, since the Masters in arts were young men and the disputants were teen-agers. However, the protocol of the disputation was firmly fixed and there was little opportunity to stray from the point under discussion.

The introduction of Aristotle's *Posterior Analytics* exercised an important and invaluable influence on the scholastic method once it was understood. This understanding, however, took about one hundred years to achieve. Around the middle of the twelfth century there existed three new translations of the *Posterior Analytics,* two from the Greek and one from the Arabic. By 1159 the text was known to the Masters of Paris, but John of Salisbury tells us that there was scarcely a Master willing to expound it because of its extreme subtlety and obscurity. John himself gives us what is probably the first Latin paraphrase in his *Metalogicon.* The first full-length commentary known is that of Robert Grosseteste, written between 1200-09. But it is one thing to explain the scientific method found in the *Posterior Analytics,* and quite another thing to see it actually employed in the sciences. Consequently, it was not until the schoolmen saw this method applied in the other Aristotelian books that they could appreciate the nuances of Aristotle's scientific method. The Latin translation of Averroes' great commentaries (c. 1220-30) aided the schoolmen considerably in this appreciation. The first to appreciate fully the scientific method of Aristotle was, without doubt, Albertus Magnus, who utilized it not only in his paraphrases, but also in his own original and extensive investigations of nature.

The sublimest product of the Aristotelian method in the Middle Ages, however, was none other than the *Summa Theologiae* of St. Thomas Aquinas. He wrote this handbook for beginners, because they were in need of a brief, systematic presentation of sacred doctrine. His purpose is stated clearly in the prologue:

> Students in this science have not seldom been hampered by what they found written by other authors, partly on account of the multiplicity of useless questions, articles and arguments; partly also because the things they need to know are not taught according to the order of learning *(secundum ordinem disciplinae),* but according as the plan of the book might require, or the occasion of disputing *(disputandi)* might offer; partly, too, because frequent repetition brought weariness and confusion to the minds of the students.

The order and plan of the *Summa* cannot be appreciated without a good understanding of the *Posterior Analytics,* for in the *Summa* St. Thomas consciously applied the scientific method to an entirely new field. The general plan, of course, is the ancient Creed seen as a sublime *exitus et reditus* of creatures destined for salvation.[32] But it is the order of questions and the order of articles within each question that reveals St. Thomas' profound appreciation of the Aristotelian method of scientific knowledge.

Many modern misunderstandings have arisen in the reading of St. Thomas because the reader or commentator failed to appreciate the kind of question being asked. Historically and scientifically, we can say that every article in the *Summa* has its proper place and purpose. A clear example of St. Thomas' use of the scientific method can be seen in the very first question of the *Summa.* The failure to understand this method has occasioned an infinite variety of confusing commentaries on this so-called introduction to theology.[33] The

32 Cf. M.-D. Chenu, *Introd, à l'étude de s.Thomas, ed. cit.,* pp. 258-273.
33 *See* G. F. Van Ackeren, S.J., *Sacra Doctrina* (Rome, 1952).

entire question deals with one subject, sacred doctrine,[34] and each article within the question is designed to lead the student to a better idea of the subject he is about to study. The question is divided into three essential parts: its existence (an *sit),* its nature *(quid sit),* and its method *(de modo eius).* Thus:

- a. 1: Whether sacred doctrine exists
- aa. 2-7: search for definition
  - generic
    - it is knowledge
    - it is intrinsically one
    - it is both speculative and practical
    - it is the highest kind of knowledge
    - it is, in fact, the highest wisdom
  - specific determinant: its subject is God himself
- aa. 8-10: its method
  - is demonstrative
  - is symbolic and poetic
  - is pluralistic in meaning

The method employed within each article is the scholastic *videtur quod non* and *sed contra* familiar to every school boy of the Middle Ages. The problem embodied in the arguments "for" and "against" clarifies the precise question under discussion. Obviously, not every solution offered in the body of the article should be taken as a scientific demonstration. When it is a question of *quid sit,* St. Thomas uses the proper method for finding definitions. When it is a question of probability, he uses the best and simplest dialectical argument intelligible to beginners.

In his requirements for a true scientific demonstration, St. Thomas was perhaps more rigorous than Aristotle. A number of arguments presented by Aristotle as demonstrative are re-

34 The extent to which commentators have gone in refusing to accept the unity of this question is summarized by Van Ackeren, *ibid.,* pp. 19-52. Fr. Chenu even maintains that articles 9 and 10 appear in the first question only out of St. Thomas' deference to usage — the internal logic of his theory will eliminate them in the course of time! M.-D. Chenu, "La théologie comme science au XIIIe siècle," AHDLMA, II (1927), 69.

jected by St. Thomas as inconclusive, for example, the eternity of the world and animation of the celestial bodies. True demonstrations are not too difficult to discover in sacred doctrine, since all the important ones are revealed or rest on divine revelation. In natural science, however, true demonstrations are much more difficult because of the complexity of the physical world. But even here, Aquinas insisted that true, proper and adequate explanations of natural phenomena must be sought within the proper subject matter of natural science. For this reason he denied that a mathematical middle term could give a *propter quid* explanation of a natural fact.[35]

St. Thomas realized that quantity is an attribute of the physical world, and that, consequently, mathematical principles are indeed applicable to physical phenomena.[36] But for Aquinas, as for Albert before him, mathematical principles are applicable only to the quantitative aspects of the physical world.[37] There are other aspects of reality, which for Aquinas are not quantitative, and consequently not explainable in terms of mathematics. Such aspects would include actuality, potentiality, finality, form, existence, and causality. To the extent that mathematical principles are applicable to the physical world, mathematical explanations of natural phenomena are considered to be demonstrations *"quia" per causam remotam* by Aquinas. Where mathematical principles are inapplicable, then true mathematical demonstration is impossible, even though considerable information and insight may be obtained about secondary aspects of the problem.

In the fourteenth century the area of phenomena considered capable of mathematization was considerably extended. Schoolmen such as Thomas Bradwardine, John Dumbleton, Richard Swyneshed and Nicole Oresme considered such quali-

35 St. Thomas, *In I Post. Anal.,* 1. 25, n. 6. *See* Cajetan's explanation of this passage of St. Thomas, *In I Post. Anal.,* cap. 13, § *Quomodo in diversis,* ed. Venice, 1599, fol. 131 a-b.

30 *In Boeth. De Trin.,* V, 3 ad 5 and 6; *In II Phys.,* 1. 3, nn. 6-9; *Sum. Theol.,* I-II, 35, 8; II-II, 9, 2 ad 3.

37 See my article, "The Celestial Movers in Medieval Physics," *The Dignity of Science, ed. cit.,* pp. 153-161.

ties as heat, color, sound, density, and velocity to be forms capable of *a* certain latitude of intensity which could be determined mathematically. For Bradwardine the degree of motion (velocity) is intensified (accelerated) according to determined laws of geometrical proportion.[38] Theoretically Bradwardine's mathematical law of proportionality was perfect, and it inaugurated a new move to find this kind of kinematic proportionality in all types of "qualitative" changes. Dumbleton tried to work this out for degrees of certitude, doubt, condensation, rarefaction, and light; his attempts, however, produced little of merit.[39] Nevertheless, the "new physics" inaugurated by Bradwardine was immediately received in the schools and widely disseminated. There is abundant evidence to show that the "new physics" of the fourteenth century influenced the scientific revolution of the seventeenth century.[40] Even Leibniz was so impressed with the work of Richard Swyneshed that he seriously considered putting out a new edition of the famous *Calculationes.* Leibniz was under the impression that Swyneshed was "the first to introduce mathematics into scholastic philosophy."[41]

The new physics founded by Bradwardine was not a rejection of the scholastic method. On the contrary, he uses it throughout his works. The new physics was simply an attempt to apply mathematical principles to more and more phenomena in nature. In the classical sense of *modus,* Bradwardine did in

38 Bradwardine, *Tract, de proportione velocitatum in motibus,* c. 3, ed. H. L. Crosby, Jr., as *Tract, de proportionibus* (Madison, 1955), pp. 110-116. On the meaning of Bradwardine's law, *see* A. Maier, *Die Vorlaufer Galileis im 14. Jahrhundert* (Rome, 1949), pp. 81-110; J. A. Weisheipl, *The Development of Physical Theory in the Middle Ages* (New York, 1959), pp. 73-81.

39 See my article, "The Place of John Dumbleton in the Merton School," *Isis,* L (1959), 439-454.

40 Much evidence is presented by Marshall Clagett, *The Science of Mechanics in the Middle Ages* (Madison, 1959), pp. 629-671, and by Anneliese Maier throughout her *Studien zur Naturphilosophie der Spdtscholastik* (Rome, 1949-58).

41 "Vellem etiam edi scripta Suisseti vulgo dicti Calculatoris, qui mathesin in philosophiam scholasticam introduxit." Letter to Thomas Smith (1696), quoted by L. Thorndike, *History of Magic and Exper. Sc.,* (1923-1958) III, 370.

fact use a new method for the study of natural phenomena. That method was the mathematical method previously employed in astronomy and in the other "middle sciences" between mathematics and physics. This method was more thoroughly exploited in the seventeenth century.

IV. *The Method of Galileo Galilei*

The scientific revolution of the seventeenth century is commonly summed up under the caption "The Downfall of Aristotle." Not infrequently this "downfall" is credited to a new scientific method based on observation and experimentation. However, some years ago Ernst Cassirer suggested that Galileo's method was really the Aristotelian *compositio-resolutio* employed by Jacopo Zabarella.[42] This view has recently been defended by J. H. Randall, A. C. Crombie, and N. W. Gilbert.[43] For the present I would like to pass over both of these views and direct attention to two methodological innovations of Galileo which can more suitably culminate our discussion of scientific method.

The first innovation to be considered is the very point discussed by Aristotle in the *Posterior Analytics* and in the *Physics,* namely, the value of mathematical middle terms in the explanation of physical phenomena. It was Galileo more than anyone else who was chiefly responsible for introducing the mathematical middle term as the *only* true, certain, and *propter quid* demonstration in natural science. This is implied in his famous panegyric on mathematics :

> Philosophy is written in that vast book . . . the universe. ... It is written in mathematical language, and the letters are triangles, circles and other geometrical

42 E. Cassirer, *Das Erkenntnisproblem in der Philosophie und Wissenschaft derneuren Zeit,* 2 vols. (Berlin, 1906-7).

i-CJlT· H. Randall, Jr., "The development of scientific method in the school of Padua," *J. Hist. Ideas.* J. (1940), 177ff.j)A. C. Crombie, *Robert Grosseteste and the Origins of Experimental Science* (Oxford, 1953), pp. 303-319; N? W. *Giibat^e&^cit.*

> figures, without which means it is humanly impossible to comprehend a single word.[44]

The origin of this innovation can be seen vaguely in the mathematical ideal of Robert Grosseteste and Roger Bacon, as A. C. Crombie has pointed out.[45] It can be seen more clearly in the kinematics and dynamics of Thomas Bradwardine, as we have already noted.[46] It is even more conspicuous in the cryptic notebooks and drawings of Leonardo da Vinci, for whom "no human inquiry can be called true science, unless it proceeds through mathematical demonstrations."[47] But none of these current claimants as "precursors of Galileo" can adequately account for Galileo's unshakeable conviction in the power of mathematics. The origin of this innovation must be sought elsewhere.

Historically and doctrinally, Galileo's basic conviction that he had discovered "an entirely new science in which no one else, ancient or modern, has discovered any of the most remarkable laws which I demonstrate to exist in both natural and violent movement"[48] must be traced to Copernicus. It must be traced to Copernicus' own conviction that he had found, not merely another way in which "to save" the phenomena of the

44 Galileo, *Il Saggiatore,* q. 6: "La filosofia è scritta in questo grandissimo libro ... (io dico l'Universo). . . . Egli è scritto in lingua matematica, e i caratteri son triangoli, cerchi, ed altre figure geometriche, senza i quali mezi è impossibile a intendeme umanamente parola" *(Opere,* ed.naz. IV, p. 232).

45 Crombie, *op. cit.,* pp. 91-127; 139-145.

40 In an ecomium which reflects Roger Bacon and reminds us of Galileo, Thomas Bradwardine says: "It is [mathematics] which reveals every genuine truth, for it knows every hidden secret and bears the key to every subtlety of letters; whoever then has the effrontery to study physics while neglecting mathematics, should know from the start that he will never make his entry through the portals of wisdom" *(Tract, de continuo,* Erfurt, MS Ampion. Q. 385, fol. 31v and Torun MS 4°. 2, p. 171). An edition of the Latin text prepared by John E. Murdoch is expected shortly.

47 Leonardo da Vinci, *Frammenti letterari e filosofici,* ed. Edmondo Solmi (Florence, 1925), p. 83. See the pioneer work on Leonardo by Pierre Duhem, *Études sur Léonard de Vinci* 3 vols. (Paris, 1906-13).

40 Letter of Galileo to Belisario Vinta (1610),'trans, by S. Drake, *Discoveries and Opinions of Galileo* (New York, 1957), p. 63.

heavens, but the *only* way. Osiander's Preface not withstanding, Copernicus himself and many of his supporters were not content to consider the new system as a mere theory, a mere "saving of the appearances."[49] The real point was that Copernicus and many Copernicans, including Galileo, insisted that it was the *only true* system of the system of the heavens. This was the understanding of the Holy Office in 1616 when the *De revolutionibus orbium* was placed on the *Index* "until corrected." In order to prove the absolute truth of the Copernican system, Galileo frequently resorted to sensible proofs, such as the motion of the tides and telescopic evidence of corruptibility in the heavenly bodies.[50] Nevertheless he was convinced that mathematics alone sufficiently demonstrated the necessary truth of the Copernican system.

But if mathematics could demonstrate so perfectly the true world system in astronomy, why not in terrestrial physics as well? Many factors led Galileo and his contemporary, John Kepler, to believe that terrestrial and celestial phenomena must be governed by the same mathematical laws of nature. Those factors need not concern us here. The important point is that for Galileo only mathematics could give true and certain *propter quid* demonstrations in natural science. The basis for this conviction was his conception of quantity, which was thoroughly Platonic. Instead of considering mathematical entities as abstractions from nature, as Aristotle and St. Thomas had done, Galileo conceived the ideal geometrical bodies as the true substrate of all reality. During the second day of the *Dialogue Concerning the Two Chief World Systems* Galileo explains that there is no real difference between abstract and concrete geometric figures:

> Just as the computer who wants his calculations to deal with sugar, silk, and wool must discount the boxes, bales and other packings, so the mathematical scientist

49 On this point *see* Edward Rosen, *Three Copernican Treatises* (New York, 1959), pp. 22-33.

50 Cf. A. C. Crombie, "Galileo's Dialogues Concerning the Two Principal Systems of the World," *Dominican Studies,* III (1950), 105-138.

> *(jilosofo geometro),when* he wants to recognize in the concrete the effects which he has proved in the abstract, must deduct the material hindrances, and if he is able to do so, I assure you that things are in no less agreement than arithmetical computations.[51]

Consequently Galileo did not consider quantity and quantified aspects to be a "remote cause" of natural phenomena, but the immediate, proper cause of everything that counts in objective nature: "size, shape, quantity and motion, swift or slow."[52] For this reason the so-called secondary sense qualities — tastes, smells, sounds, colors, heat, etc. — were eliminated from Galileo's objective world and reduced to individual sensations ; they "are nothing more than mere names, and exist only in the sensitive body."[53]

However, if we overlook Galileo's Platonic view of quantity, and if we discount his optimism in the matter of demonstration, we must admit that he did discover a new method, a new *modus,* namely the mathematical way to nature. Because this method is determined by the objectively measurable aspects of physical phenomena, he did indeed discover "an entirely new science." This "new science" was, in fact, an extension of celestial mechanics, the ancient science of astronomy, to the world of terrestrial phenomena.

The method of this new science is still the analytical or "resolutive method" of Aristotle, as Galileo himself states on the first day of the *Dialogue.*[54] We should not have expected

5' Galileo, *Dialogue Concerning the Two Chief World Systems,* trans, by S. Drake (Berkeley, 1953), p. 207. On Galileo's Platonism *see* A. Koyré "Galileo and Plato," *J. Hist. Ideas,* IV (1943), 400ff., reprinted in *Roots of Scientific Thought,* ed. by P. P. Wiener and A. Noland (New York, 1957), pp. 147-175.

52 Galileo, *Il Saggiatore,* q. 48. Opere, IV, 333. The full text is translated and discussed by E. A. Burtt, *The Metaphysical Foundations of Modern Physical Science,* rev. ed. (London, 1932), pp. 73-80.

53 On the subjectivity of secondary qualities in Descartes and Hobbes, see Burtt, *ibid.,* pp. 111-113, 122-127.

54 Galileo, *Dialogue Concerning the Two Chief World Systems, ed. cit.* p. 51.

anything different, since mathematical physics is a speculative science, requiring the general method of all speculative knowledge. The special characteristics of the *modus proprius* are determined by the objective measure. Among the more important characteristics are:

1. the indispensable role of mathematics in all demonstrations of measurable quantities;
2. the impossibility of dealing with anything but measurable quantities;
3. the need to search for more and more suitable hypotheses to account for the facts, as did the astronomers of old; and
4. the necessity of experimentation (a) to obtain the necessary measures, and (b) to verify or falsify the hypotheses proposed.

Historians of seventeenth-century science, I think, would admit that these characteristics were universally recognized and enthusiastically praised by the founders of classical physics.

The second innovation in seventeenth-century science need only be considered briefly to establish a very important point concerning the "new science." It is generally recognized that the seventeenth century gave birth not only to a new mathematical physics, but also to a new *mechanical philosophy*.[55] What is not so clearly recognized is that there is no necessary connection between these two. There was no necessary connection between these two even in the seventeenth century. The foremost proponents of the mechanical philosophy, namely, Descartes, Gassendi, Francis Bacon, and Robert Boyle, can hardly be listed as mathematical physicists. However, like Galileo, these philosophers recognized only two first principles in natural science — matter and motion. Like Galileo, they recognized no motion in nature other than mechanical. The truth

55 On this, see the many writings of Marie Boas Hall, especially her "Establishment of the Mechanical Philosophy," *Osiris,* X (1952), 412-541, and her forthcoming "Matter in Seventeenth Century Science," in *Concepts of Matter,* ed. E. McMullin, to be published by Notre Dame University Press.

of the matter is that in the seventeenth, eighteenth, and nineteenth centuries there was a comfortable compatibility between a mechanical philosophy and mathematical physics. To use Whitehead's felicitous phrase, we might say that they were oblivious to "the fallacy of misplaced concreteness."

The essential feature of this mechanical philosophy was the rejection of φυσις, or nature, as an explanatory principle in natural science. With this rejection also went potency and act, substance, formal and final causality, and even the ontological reality of true causality. In their place, as is well known, the seventeenth-century philosophers substituted quantified matter (corpuscular, atomic, or continuous), mechanical agencies (like impulse, attractions, repellents, adhesive forces and various energies), and local motion. But the important point is that these substitutes for the concept of nature were, in fact, principles proposed for a new *natural philosophy.* They were not the principles of the new mathematical physics actually discovered by Galileo.'The principles of the new physics were and still are mathematical. In other words, the "new science" discovered by Galileo, and developed by Newton, and perfected in our own day by the theories of relativity and quantum, can be recognized as a legitimate science in the Aristotelian sense of the term. At the same time we can reject the mechanical philosophy which happened to predominate in the seventeenth, eighteenth, and nineteenth centuries.

If we have presented a fair estimate of the evolution of scientific method, then we must say that this evolution did not consist in rejecting the old for the new. Rather, it consisted in the addition of new methods and discoveries to the still valid ancient methods and discoveries.

Index