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# THE ORIGINS OF MODERN SCIENCE: A NEW INTERPRETATION

Since the heroic age of Pierre Duhem, to whose amazing energy and learning we owe the revelation of medieval science, much work has been devoted to the study of that subject. The publication of the great works of Thorndyke and Sarton, and, in the last decade, of the brilliant studies of Anneliese Maier and Professor Marshall Clagett, not to mention countless other monographs and papers, has tremendously enlarged and enriched our knowledge and understanding of medieval science in its connection with medieval philosophy—toward whose understanding and knowledge even greater progress has been made—and of medieval culture in general.

And yet, the problem of the origins of modern science and its relations with the science of the Middle Ages remains a very lively *questio disputata*. The partisans of continuity as well as those of revolution hold to their own positions and each group seems to be unable to convince the other.[1] This is not so much, in my opinion, because they disagree about the facts, as because they disagree about the essence of modern science itself, and, therefore, about the relative importance of some of its characteristic fea-

1. Cf. for instance my review of Anneliese Maier's *Die Vorläufer Galileos im XIV Jahrhundert* (Rome, 1949) in *Archives Internationales d'Histoire des Sciences*, 1951, pp. 769 ff., and her answer, "Die naturphilosophische Bedeutung der scholastischen Impetustheorie" in *Scholastik*, 1955, pp. 321 ff.



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tures. Moreover, what to some appear to be differences in degree are to others oppositions in kind.[2]

The theory of continuity finds in A. C. Crombie its most eloquent and most radical supporter. Indeed, the purpose of his brilliant and scholarly book on Robert Grosseteste[3]—one of the most important contributions to our knowledge of the history of medieval thought published during the last decade: a work whose scope in the field of information is equally as remarkable as its depth and subtlety of interpretation—is precisely to demonstrate that not only do the roots of modern science plunge deeply into medieval soil, but that, fundamentally and essentially, modern science is, in its methodological and philosophical inspiration, a medieval invention. For, to quote Dr. Crombie (p. 1):

> The distinctive feature of scientific method in the seventeenth century, as compared with that in ancient Greece, was its conception of how to relate a theory to the observed facts it explained, the set of logical procedures it contained for constructing theories and for submitting them to experimental tests. Modern science owes most of its success to the use of these inductive and experimental procedures, constituting what is often called "the experimental method." The thesis of this book is that the modern, systematic understanding of at least the qualitative aspects of this method was created by the philosophers of the West in the thirteenth century. It was they who transformed the Greek geometrical method into the experimental science of the modern world.

The reason why they were able to do this consists, according to Dr. Crombie, in the fact that, in contradistinction to their Greek—and even Arabic—predecessors, they were able to unite the practical empiricism of the arts and crafts with the search for a rational explanation and thus to overcome the limitations of both, and that, once more in contradistinction to the Greeks, they were able to form a much more unified conception of knowledge. Accordingly, whereas for the Greeks the different kinds and modes of knowledge distinguished by them (physics, mathematics, metaphysics) were related to different kinds of being, the Christian philosophers of the West "saw them primarily as distinctions of method" (p. 2).

Methodological problems play an important part in the critical periods of science, as we have ourselves seen in recent times. Small wonder, then,

2. Thus the replacement of the qualitative approach by the quantitative (cf. *infra*, pp. 3, 18 ff.) appears to Dr. Crombie to represent a difference in degree, and to myself, to be a difference in kind.

3. A. C. Crombie, *Robert Grosseteste and the Origins of Experimental Science, 1100–1700* (Oxford, Clarendon Press, 1953); also, A. C. Crombie, *Augustine to Galileo* (London, Falcon Press, 1952).

that they did so in the thirteenth century, an epoch when the Western world had to deal with an almost overwhelming enrichment in scientific (and philosophical) knowledge consequent to the ever increasing stream of translations from the Arabic and the Greek. Now, the most important problems dealt with by scientific methodology are concerned with the relations of theories to facts; its aim consists in the establishment of the conditions which a theory must fulfill in order to be accepted and of the ways and means which enable us to decide whether a given theory is valid or not—or, to use the medieval terms, the ways and means of its "verification" and "falsification."

The great merit of the philosophers-scientists of the thirteenth century consists, according to Dr. Crombie, in having understood the value of the experimental procedure (as distinguished from mere observation, which forms the basis of the Aristotelian induction) for this verification and falsification; they have thus discovered and worked out the fundamental structures of the so-called "experimental method" of modern science. As a matter of fact, they discovered, even more significantly, the true meaning and status of a scientific theory, namely, that such a theory "could never be certain"; that it is sufficient for a theory "to save the appearances"; and that it is impossible for such a theory to claim to be necessary, that is to say, unique and final.

Dr. Crombie does not assert, of course, that medieval (thirteenth and fourteenth century) science used the experimental method as well and extensively as did seventeenth century science. Thus, he says (p. 9):

> The experimental method was certainly not completed in all its refinements in the thirteenth or even the fourteenth century. Nor was the method always systematically practiced. The thesis of this book is that a systematic theory of experimental science was understood and practiced by enough philosophers for their work to produce the methodological revolution to which modern science owes its origin. With this revolution appeared in the Latin West a clear understanding of the relation between theory and observation on which the modern conception and practice of scientific research and explanation are based, a clear set of procedures for dealing with physical problems.

Seventeenth century science and philosophy made, according to Crombie, no fundamental change in the existing scientific procedures. What they did do was to replace the qualitative approach by the quantitative, and adapt a new kind of mathematics to the experimental research (pp. 9–10):

The most important improvement made subsequently to this scholastic method was a change, general by the seventeenth century, from qualitative to quantitative procedures. Special apparatus and measuring instruments increased in range and precision, controls were used to isolate the essential factors in complicated phenomena, systematic measurements were made to determine the concomitant variations and render problems capable of mathematical statement. Yet all these were advances in existing practices. The outstanding original contribution of the seventeenth century was to combine experiment with the perfection of a new kind of mathematics and with a new freedom in solving physical problems by mathematical theories of which the most striking are those of modern dynamics.

Seventeenth century science has laid claim to complete originality, and, in its self-interpretation, has conceived itself as fundamentally opposed to that of the medieval scholasticism which it purported to overthrow. And yet (p. 3):

The conception of the logical structure of experimental science held by such prominent leaders as Galileo, Francis Bacon, Descartes and Newton was precisely that created in the thirteenth and fourteenth centuries. They inherited also the concrete contributions made to particular sciences during the same period.

As we see it, Dr. Crombie's historical theory, besides making the general assertion of continuity in the development of scientific thought from the thirteenth to the seventeenth century, comprises a very interesting conception of the part played by methodology in this very development. According to him, the thirteenth century thinkers first developed a conception of science and scientific method which, in its fundamental features—including the use of mathematics for the formulation of theories and of experiment for their verification and falsification—was identical with that of the seventeenth century and then, in conscious application of this method to particular scientific investigations, developed a science of the same type as that of Galileo, Descartes, and Newton. And it is in order to prove this very original thesis that Dr. Crombie presents an extremely interesting history of the medieval discussions *de methodo;* that is, of the development of *inductive* logic (a field somewhat neglected by the historians of this discipline), as well as a suggestive and very valuable survey of the development of medieval optics. It is indeed to the field of optics much more than to that of physics proper (or dynamics) that Dr. Crombie turns for the verification of his theory.

The methodological discussions of the medieval philosophers follow the pattern laid down by the Greeks and are closely linked with Aristotle's treatment of the problem of science (inductive and deductive) in his *Posterior Analytics*. More often than not they are presented to us as *Commentaries* to these *Analytics*. And yet, these medieval *Commentaries*, some, at any rate, and certainly those of Robert Grosseteste, the hero of Dr. Crombie's tale, represent a marked progress in respect to their Greek—or Arabic—models. To quote once more (pp. 10–11):

The strategic act by which Grosseteste and his thirteenth and fourteenth century successors created modern experimental science was to unite the experimental habit of the practical arts with the rationalism of the twelfth-century philosophy. Grosseteste appears to have been the first medieval writer to recognize and deal with the two fundamental methodological problems of induction and experimental verification and falsification which arose when the Greek conception of geometrical demonstration was applied to the world of experience. He appears to have been the first to set out a systematic and coherent theory of experimental investigation and rational explanation by which the Greek geometrical method was turned into modern experimental science. As far as is known, he and his successors were the first to use and exemplify such a theory in the details of original research into concrete problems. They themselves believed that they were creating a new science and in particular that they were creating a new methodology. Much of the experimental work of the thirteenth and fourteenth centuries was in fact carried out simply to illustrate this theory of experimental science, and all their writings show this methodological tinge.

Thus, for instance, one of Grosseteste's most important and fruitful methodological ideas, namely, that mathematical science can often provide the reason for factual knowledge acquired in physical science, seems to have been first developed by him as a purely epistemological conception and later put in practice in the examination of particular physical problems and exemplified by optical illustrations (cf. pp. 51–52). This, as a matter of fact, is quite natural, since optics (like astronomy and music) was determined by Aristotle as belonging to the *mathematica media*, that is, to a class of sciences which, though distinct from pure mathematics, were nevertheless *mathematical* sciences in so far as their subject-matter—in contradistinction to that of his *physics*—could be dealt with mathematically (as our applied mathematics). But, in the case of Grosseteste, this recourse to optics has another and much deeper meaning. Indeed, as Dr. Crombie repeatedly, and quite rightly, points out, "Platonic metaphysics . . . has always contained the possibility of mathematical explanation." And for the Neoplatonist, Grosseteste, for whom light (*lux*) was the "form" of the created world which informed formless matter and in its expansion gave birth to the very extension of space, "optics was the key to the understanding of the physical world" (pp. 104–5) because, as Ibn Gabirol held before him, and as Roger Bacon was to hold after him, Grosseteste believed "that all causal action followed the pattern of light." Light metaphysics, thus, transforms optics into the basis of physics which, in its turn, becomes—or, at least, could become—a *mathematical* physics.

Yet, in spite of this potential trend towards mathematization of physics, Grosseteste does not proceed very far on the way toward geometrization of nature. Quite the contrary: he makes a careful and sharp distinction between mathematics and natural science, as when he tells us, for instance, that the cause of the equality of the angles of incidence and reflexion is to be sought, not in geometry, but in the nature of radiant energy. He always insists on the uncertainty of physical theories as opposed to the certainty of mathematics (Dr. Crombie even attributes to him the assertion that all physical knowledge is only *probable*[4]), which uncertainty is precisely the reason for the need of experimental verification of their validity.

"The conception of science," says Dr. Crombie (p. 52), "which Grosseteste, like his twelfth century philosophical predecessors, learnt from Aristotle, was one in which there was a double movement, from theory to experience and from experience to theory." Thus, in his *Commentary to the Posterior Analytics*,

> Grosseteste said "There is a double way with already existing knowledge and (new) knowledge, namely from the more simple to the composite, and the reverse," from principles and from the effect. Scientific knowledge of a fact was had, he held, when it was possible to deduce the fact from prior and better known principles which were its causes. This meant, in fact, relating the fact to other facts in a deductive system; such knowledge he found exemplified in Euclid's *Elements*.

In mathematics, the way from the more simple and better known to the composite was called by the Greeks "synthesis," and the way from the more complex to the more simple, "analysis." But, in some sense, there is no fundamental difference between these ways, or procedures, as both the premises and the conclusions are indisputable, necessary, and even self-evident.

4. This seems to me to be an overstatement. Indeed in the passage quoted by Dr. Crombie (p. 59, n. 2) Grosseteste says only that in natural science there is *minor certitudo propter multabilitatem rerum naturalium*, pointing out that *according to Aristotle* science and demonstration *maxime dicta* occurs only in mathematics, whereas in other sciences, though there is also science and demonstration, they are, however, *non maxime dicta*. Grosseteste is perfectly right as Aristotle distinguishes quite sharply between things that are necessarily so, and things that are so only in most cases, or habitually. Still, it is far from this statement to the probability thesis.

The situation is quite different in natural science. The simple principles are by no means evident, nor are they even better known than the complex, given facts. Simple empirical induction does not lead us to the wanted goal: there is a leap from it to causal, explanatory, statement. In order to prepare this leap we must use a procedure analogous to analysis and synthesis: that of "resolution and composition." But that is not sufficient; in order to ascertain the validity of the principles (causes) we arrive at by this procedure, we must test them by experiment. Indeed, the "resolution" can be made in more than one way and the effects to be explained can be deduced from more than one cause or set of causes (p. 82). Thus:

In natural science, Grosseteste held, in order to distinguish the true cause from other possible causes, at the end of "composition" must come a process of verification and falsification. A theory reached by resolution and intuition was, as he pointed out, capable of yielding by deduction consequences beyond the original facts on which the induction was based. "For when the argument proceeds by composition from principles to conclusions . . . it may proceed in infinity by the addition of the minor extreme under the middle term." On the basis of these consequences controlled experiments were arranged by which false causes could be eliminated.

All scientific procedures imply some metaphysical basis, or, at least, some axiomatic assumptions about the nature of the reality. Those of Grosseteste, inherited of course from the Greeks, and, as a matter of fact, stated by all, or nearly all, representatives of natural science, before as well as after him, were the following:

The first was the principle of the uniformity of nature, meaning that the forms are always uniform in their operations. As he put it in *De Generatione Stellarum*: "*Res eiusdem naturae eiusdem operationis secundum naturam suam effectivae sunt. Ergo si secundum naturam suam non sunt eiusdem operationis effectivae, non sunt eiusdem naturae.*" In support of this principle he quoted "Aristoteles II *de Generat.*: '*idem similiter se habens non est natum facere nisi idem*' "; "the same cause, provided it remains in the same condition, cannot produce anything but the same effect" (p. 85). The second . . . was that of the principle of economy, or *lex parsimoniae* . . . derived also from Aristotle who stated it as a pragmatic principle . . .

and which Grosseteste, as well as his medieval precursors and modern successors, used as a principle governing not only science but nature itself (p. 87):

Beginning with these assumptions about reality, Grosseteste's method was to distinguish between possible causes "by experience and reason." He made deductions from rival theories which contradicted either the facts of experience or what he regarded as an established theory verified by experience, and used those theories which were verified by experience to explain further phenomena. This method he explicitly put into practice in his *opuscula* on various scientific questions, where the theories with which he began were sometimes original though usually taken from such previous authors as Aristotle, Ptolemy or various Arabic naturalists. Good examples are his discussions of the nature of stars and of comets . . .

as well as those about the nature and cause of the rainbow and of some animals having horns.

It is Roger Bacon, probable as it is that he never actually attended the lectures of Robert Grosseteste, whom Dr. Crombie sees as the latter's best pupil. Thus he says (p. 139):

The writer who most thoroughly grasped, and who most elaborately developed Grosseteste's attitude to nature and theory of science was Roger Bacon. . . . Recent research has shown that in many of the aspects of his science Bacon was simply taking over the Oxford and Grossetestian tradition though he was able to make use of new sources unknown to Grosseteste; as, for example, the *Optics* of Alhazen.

and therefore not only to repeat, but also to improve, at least in some cases, Grosseteste's optical theories. At other times, alas, he replaced them by much worse ones.

Thus, whereas in his theory of the propagation of light (multiplication of *species*) he accepted Grosseteste's explanation of this process by the autogeneration and regeneration of *lux*, as well as the suggested analogy between light and sound, he brings a notable clarification to this conception by stating that light is not a flow of body, but a pulsation; he accepts also Alhazen's denial of the instantaneous propagation of light; yet, whereas Grosseteste explained the appearance of the rainbow by a series of refractions of light "in the midst of a convex cloud," Bacon, though rightly pointing out the role played by individual raindrops and the fact that each observer sees a different rainbow,[5] substituted, rather unfortunately, reflection for refraction. As for his general, logico-methodological position, Roger Bacon, indeed, stresses *both* the mathematical and the experimental aspects of science (p. 143):

Mathematics, Roger Bacon said, was the "door and keys of the science . . . and things of this world" and gave certain knowledge of them. In the first place, all

5. He learned this from Alexander of Aphrodisias, or Avicenna (cf. p. 158, n. 3) or even from Seneca.

categories depend on a knowledge of quantity, concerning which mathematics treats, and therefore the whole excellence of logic depends on mathematics.

But not only logic—natural science itself—was dependent, according to him, at least to a large extent, on mathematics, as Bacon goes on to say (*ibid.*):

In mathematics only, as Averroës says in the first book of the *Physics* . . . "things known to us and in nature or absolutely are the same, [wherefore] the greatest certainty is possible . . . in mathematics only are there the most convincing demonstrations through a necessary cause . . . : Wherefore it is evident that if, in the other sciences, we want to come to certitude without doubt and to truth without error, we must place the foundations of knowledge in mathematics. . . . Robert, Bishop of Lincoln, and Brother Adam of Marsh" had followed this method and "if anyone should descend to the particular by applying the power of mathematics to the separate sciences, he would see that nothing magnificent in them can be known without mathematics . . ."

as we can clearly see from the fact that astronomy is entirely based upon mathematics, and that it is by mathematical reasoning and calculations that we arrive—in the *compotus*—at the determination of facts.

On the other hand, nobody praised experimental science as highly as Roger Bacon, who attributed to it not only the "prerogative" of confirming—and disconfirming—the conclusions of deductive reasoning (*verificatio* and *falsificatio*) but also, and much more importantly, of being the source of new and significant truths that cannot be arrived at by any other means. Indeed, who would be able, without experience, to know anything about the magnet? How would it be possible, without experience, to discover the secrets of nature and, for example, to promote medicine? It is experimental science, that unites reasoning and manual work, which will enable us to construct instruments and machines that will give to mankind—or to Christendom—both knowledge and power. But I need not insist: everybody recognizes the amazing anticipations—and just as amazing credulity—of Roger Bacon.

I cannot, alas, analyze here the history of medieval optics and medieval explanations of the rainbow presented to us by Dr. Crombie, with whose expert guidance we encounter Albert Magnus (pp. 197–200), Vitello (pp. 213–32), who certainly knew both Grosseteste and Roger Bacon though he does not quote them, and who, moreover, was a decided partisan of the Neoplatonic light-metaphysics of the great Oxford thinker, and, finally, Dietrich von Freiberg (pp. 232–59), the greatest of the



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medieval opticians, to whom we owe the decisive step of admitting a double refraction of rays of light in raindrops. I must return to the history of methodology, where Dr. Crombie presents to us as Grosseteste's successors, who developed and elaborated inductive logic along his lines, Duns Scotus (which is rather natural) and William Ockham, which is rather surprising, since Ockham—as Dr. Crombie himself insists (p. 171)—"violently reacted against contemporary Augustinism-Platonism" of which Grosseteste was so keen a supporter.

Dr. Crombie believes, indeed, that Ockham's positivistic epistemology which, according to him, favored the development of empirical science, is the normal outcome of the methodological movement inaugurated by Robert Grosseteste, and even its *akme*. Thus, in summing up Grosseteste's own views, he tells us that (p. 13) "he held that the function of mathematics was only to describe and correlate occurrences. It could give no knowledge of the efficient or other causes producing facts because it was explicitly an abstraction from such causes" the investigation of which was the proper function of natural science in which however, "knowledge of causes was always incomplete and only probable." Moreover, already in his general presentation of the intellectual (epistemological) development of the medieval scientific philosophy, which I have quoted above, Dr. Crombie has told us (p. 11):

> The eventual result of this attempt to understand how to correlate facts by theories in an accurate practical discipline was to show that in science the only "criteria of truth" were logical coherence and experimental verification. The metaphysical question about *why* things happen, which was answered in terms of substance and causes, in terms of *quod quid est*, gradually gave place to the scientific question about *how* things happen, which was answered simply by the correlation of the facts by any means, logical or mathematical, that was convenient.

As for Ockham, who was himself not at all an experimentalist, he nevertheless "predisposed natural philosophers to seek knowledge of nature by experiment"; this because he violently criticized the traditional conceptions of causality—not only that of final causes which, according to him, were only "metaphorical," but also that of efficient ones—and reduced knowledge to mere observation of sequences of facts. Accordingly, the practical program for natural science was simply to correlate observed facts, "or 'save the appearances' by means of logic and mathematics" (p. 175). Moreover, ruthlessly applying the principle of parsimony —the famous "Ockham's razor"—"he formed a conception [of motion] that was to be used in the seventeenth century theory of inertia" (pp. 175-76):



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He did it by rejecting both the Aristotelian conception and that of the theory of the *impetus*, defining "motion as a concept having no reality apart from moving bodies" and answering the famous question *a quo moventur projecta?* by stating "that the moving thing in such a motion (*i.e.* projectile motion), after the separation of the moving body from the first protector, is the moved thing itself, not by reason of any power in it: for this moving thing and the moved thing cannot be distinguished. If you say that a new effect has some cause, and local motion is a new effect, I say that local motion is not a new effect . . . because it is nothing else but the fact that the moving body is in different parts of space in such a manner that it is not in any one part, since two contradictories are not both true."

Let us now stop for a while and, before proceeding to the analysis of the relations of medieval and modern science as they are presented by Dr. Crombie, let us ask ourselves whether we can consider his thesis as proven. I must confess that I doubt it very much. Personally, I would even go farther than that: it seems to me that the very contents of Dr. Crombie's investigations lead to quite a different, and, in some respects, opposite, conception concerning the development of medieval science and its *anima motrix*. Dr. Crombie explains the rise of the *experimental* science of the Medievals, which he opposes to the purely theoretical science of the Greeks, by the conjunction of theory with *praxis*, the outcome of the activist inspiration of the Christian civilization in contradistinction to the attitude of passivity which characterizes that of Antiquity.[6]

I will not discuss here Dr. Crombie's conception of the Christian roots of the *scientia activa et operativa:* it is, indeed, quite certain that in the Christian—even in the medieval—tradition we can find enough elements leading to the high appreciation of labor (manual work), and that the biblical conception of a Creator-God can serve as a pattern for human activity and inspire the development of industry and even trade—as was the case with the Puritans. Yet it is rather amusing to note that these very features of activism and practicality have usually been used for the characterization of the modern mind as opposed in its this-worldliness to the otherworldliness of the medieval mind, for which this "vale of tears" was only a place of trial where the *homo viator* was to prepare himself for his eternal destiny. Accordingly, historians of science and philosophy opposed the industrial science of Francis Bacon and that of Descartes, which had to make man "master and owner of nature," to the contemplative ideal of both the Medievals and the Greeks. . . . Now, be this as it may, I am sure

6. Dr. Crombie insists upon the practical tendency of the teaching of the School of Chartres, of Kilwardby, etc. Yet Plato deplored the practical attitude and inspiration of the geometers of his time. And Archimedes was of course not only the greatest mathematician, but also the greatest technologist of the Ancient World.

that even Dr. Crombie will agree, in spite of the examples quoted by him, that medieval Christianity was much more preoccupied with the other world than with this one, and that the rise of interest in technology—as seems to be shown convincingly enough by the whole of modern history —is rather closely connected with the secularization of western civilization and the shift of interest from the future life to the present one.

As for myself, I don't believe in the explanation of the birth and development of modern science by the human mind turning away from theory to *praxis*. I have always felt that it did not fit the real development of scientific thought, even in the seventeenth century; it seems to me to fit even less that of the thirteenth and fourteenth. I don't deny, of course, that in spite of their alleged—and often real—"otherworldliness," the Middle Ages, or to be more exact, a certain, and even a rather large number of people during the Middle Ages, *were* interested in techniques; nor that they gave to mankind a certain number of highly important inventions. Some of them, had they been made by the Ancients, would probably have saved the Ancient World from collapse and destruction by the predatory Barbarians.[7] Yet, as a matter of fact, the invention of the plough, of the horse harness, of the crank, and of the stern rudder had nothing to do with scientific development; even such technical marvels as the Gothic arch, stained glass, the foliot or the fusee of late medieval clocks and watches did not depend on, nor result in, any progress in corresponding scientific theories. Strange as it may seem, even such a revolutionary discovery as that of firearms has had no more scientific effect than it had scientific bases. Bullets and cannon balls brought down feudalism and medieval castles, but medieval dynamics resisted the impact. Indeed, if practical interest were the necessary and sufficient precondition of experimental science—in our sense of the word—this science would have been created a thousand years, at least, before Robert Grosseteste, by the engineers of the Roman Empire, if not by those of the Roman Republic.

Dr. Crombie's own history of medieval optics seems to me to confirm my contention of the far-reaching independence, at least up to the development of scientific technology—a quite recent phenomenon of practical and theoretical achievements. It is possible, of course, although extremely improbable that the unknown genius who invented eyeglasses was led by theoretical considerations; it is certain, on the other hand, that this discovery did not have any effect upon the development of optical

7. It was, indeed, the inability of the Ancient World to solve the problem of transportation that was the basic cause of its ruin.

theory; whereas the latter, despite the boasts of Roger Bacon, did not give birth to optical technology and the construction of optical instruments.[8] In the seventeenth century, on the other hand, the invention of the telescope was followed by a new optics and a new *praxis*.

As for Dr. Crombie's assertion that it was the "methodological revolution of the thirteenth century" that gave birth to the new science, and that, generally speaking, methodology was the driving power and the determining factor of scientific progress, I don't believe he has proved this, either. Once more it seems to me that the very results of his investigations undermine his thesis.

He has, indeed, shown us that the famous "method of resolution and composition" which, time and again, has been presented to us as the *proprium* of the Galilean epistemology (and which Professor Randall discovered in the works of the Paduan Aristotelians[9]) was by no means a "modern" invention. It had, indeed, been well understood, described, and taught by medieval logicians since the thirteenth—and even the twelfth—centuries, and, moreover, went back to the method of *analysis* and *synthesis* (the terms *resolutio* and *compositio* are merely translations of the Greek) as practiced by the Greeks and as described by Aristotle in his *Posterior Analytics*. Nevertheless, if this be so—and after Dr. Crombie's demonstration it can hardly be doubted—the only conclusion we can draw from this very important fact seems to be that of the relative unimportance of abstract methodology for the concrete development of scientific thought. Everybody, it seems, has always known that we must try to reduce complex combinations to simple elements, and that assumptions (hypotheses) have to be verified—or shown to be false—by deduction and confrontation with facts. Thus, one is tempted to apply to methodology Napoleon's famous comment on strategy: its principles are very simple; it is the application that counts.

The history of scientific development seems to confirm this view: Dr. Crombie himself admits that the "methodological revolution" accomplished by Grosseteste did not lead him or his successors to any significant discovery, even in optics. And as for natural science in general, Grosse-

8. Optics made no progress between Dietrich of Freiburg and Maurolico, or practically (as Maurolico's writings remained unpublished till the XVI century) between Dietrich of Freiburg and Kepler. But Kepler's optics, as Vasco Ronchi has shown, is not based on medieval conceptions, but signifies the "catastrophy of medieval optics"; cf. Vasco Ronchi, *Storia della luce*, 2d ed. (Bologna, Zanichelli, 1952).

9. J. H. Randall, Jr., "The Development of Scientific Method in the School of Padua," *ournal of the History of Ideas*, 1940; cf. my "Galileo and Plato," *ibid.*, 1944.

teste's determination of the "cause" of some animals' having horns,[10] a determination based entirely upon the Aristotelian conception of the four "causes," bears very little resemblance to what *we* are accustomed to call science, be it experimental or not.

The case is not very different with Roger Bacon: his experiments, even those that are not fantastic or of a purely literary nature, are hardly superior to Grosseteste's and, in any case, do not represent progress of a revolutionary sort—if they represent any progress at all—compared with those of Greek science. On the other hand, the real progress in the development of scientific thought seems to have been to a large extent independent of that of methodology: there is method, but not methodology, in the work of Jordanus Nemorarius, and as for the thirteenth century, there is no reason to believe that Petrus Peregrinus—the only real experimenter of the time—was in any way dependent on Grosseteste.[11] Even in the field of optics, its progress—a real one—in the works of Bacon, Vitello, and Dietrich of Freiberg is determined, not by methodological considerations, but by the availability of new sources, first of all by the *Optics* of Alhazen, who, for obvious reasons, could not be influenced by the "methodological revolution" of the West.

As a matter of fact, Dr. Crombie knows very well—better, assuredly, than anyone else—that his "methodological revolution" has had a rather limited range of influence, and that the continuous development of methodological discussions during the late Middle Ages was not accompanied by a parallel development of science. He even goes as far as to explain this lack of scientific progress by the unilateral concentration of the intellectual interest of the philosophers of that period on purely methodological problems, which resulted in a divorce of methodology from science (neither Duns Scotus nor William Ockham, thus, have any real concern for science), a divorce deeply detrimental to Ockham, though not, as it seems, to Duns Scotus.

Dr. Crombie is certainly right: *too much* methodology is dangerous, and as often as not, or more often than not—we have examples enough in

10. Cf. p. 69: "The cause of having horns is not having teeth in both jaws, and not having teeth in both jaws is the cause of having several stomachs."

11. Petrus Peregrinus—and after him Roger Bacon—insist upon the necessity of manual work for the experimenter. Such is indeed the case at a time when the "artisans" are not skilled enough to prepare the instruments needed by the scientist. Thus Galileo, Newton and Huygens had to grind their lenses or *specula* themselves, etc. Yet, this is only a temporary situation and under the influence of science and its requirements an industry of instrument-makers develops and takes over the "manual work": astronomers—with very few exceptions—did not themselves prepare their astrolabes.

our own times—results in sterility. I would go even farther: in my opinion, the place of methodology is not at the beginning of scientific development, but, we might say, in the middle of it. No science has ever started with a *tractatus de methodo* and progressed by the application of such an abstractly devised method, Descartes's *Discourse on Method* notwithstanding, which, as we well know, was written not *before*, but *after* the scientific "essays" to which it forms a preface. Indeed, it codifies the rules of Cartesian algebraic geometry. Thus, even Cartesian science was no more an outcome of a methodological revolution than that of Galileo was the result of the "methodological revolution" of Robert Grosseteste. Moreover, even if we admitted the prevailing influence of methodology upon scientific development, we would be faced with the paradox of seeing an essentially Aristotelian methodology producing, after a hundred years of sterility, a fundamentally anti-Aristotelian science.

Besides, I am by no means convinced that we are entitled to apply to Grosseteste's logical teaching the term "revolution."[12] Indeed, as I have already mentioned, Dr. Crombie seems to me to have demonstrated the perfect and amazing continuity in the development of logical thinking: from Aristotle and his Greek (and Arabic) commentators to Robert Grosseteste, Duns Scotus and Ockham, the great Italian and Spanish logicians, and up to John Stuart Mill there is an unbroken chain of which the Bishop of Lincoln represents, indeed, one of the most important links: he revived this tradition and gave root to it in the West. Yet it was Aristotelian logic and methodology that he transplanted—and as this logic and methodology are part and parcel of Aristotelian physics and metaphysics, they found themselves in perfect accordance with the Aristotelian science of the Middle Ages. They did not, and could not, revolutionize it. On the other hand, Robert Grosseteste's metaphysics was by no means Aristotelian; indeed, if it embodied a good deal of Aristotelianism it was, in its most essential features, a Neoplatonic system, a rather significant fact which leads us to the problem of the influence of philosophy—or metaphysics—in general, and not only of logic or methodology, upon scientific thought.

Dr. Crombie points out, and I am glad to state that I feel myself in complete agreement with him, that Platonism and Neoplatonism were always, at least in principle, inclined towards a mathematical treatment of natural phenomena and thus, towards the attribution to mathematics of a much more important role in the system of sciences than that which

12. As a matter of fact, Dr. Crombie, though stressing its revolutionary character, admits himself that Grosseteste's methodology is essentially Aristotelian.

Aristotelianism assigned to it. He insists, too, and he is perfectly right, that the light-metaphysics of Robert Grosseteste which, moreover, was made by him the foundation of optics, was the first step towards the development of a mathematical science of nature. Here, too, I agree entirely. I believe, indeed, that it is here that Grosseteste displays his greatest originality (we must not forget that in spite of the natural harmony between Platonism and mathematization of nature, Neoplatonism finally developed a dialectical and magical and not a mathematical—arithmology is not mathematics—world-view), and a depth of intuition which only contemporary scientific development enables us to appreciate fully. It is true, of course, that his attempt at a reduction of physics to optics was utterly premature and that nobody but Roger Bacon accepted his point of view. It is also true that the evolution of optics did not play a determining part in the formation of seventeenth century physics and that Galileo did not receive his inspiration from optics. Still, and I am rather surprised that Dr. Crombie does not mention this fact, the great work of Descartes was to be called *Le Monde ou traité de lumière*, though, as a matter of fact, his physics was by no means inspired by optics, and, moreover, was not at all mathematical; in any case, it was Platonism (and, of course, Pythagorism) which inspired the mathematical science of nature of the seventeenth century (and its methods) and opposed it to the empiricism of the Aristotelians (and their methodology). Yet, as we have seen, it is not only for the Platonizing mathematicism but also, and much more so, in fact, for the empiricism of the nominalist and positivist tradition, that Dr. Crombie vindicates the merit of having inspired "modern" science.

Once more, alas, I have to disagree with him. I do not doubt, of course, that the criticism of the traditional Aristotelian conception which culminates in Ockham's attack upon the validity of final, and the knowability of all other, causes, has played an important part in the cleaning up of the ground upon which the building of modern science has to be erected and in removing some of the obstacles that hindered its construction. On the other hand, I doubt very much that it ever has had a positive influence on scientific development.

Indeed, neither the brilliant mathematical and kinematical work of Oresme—which is directly inspired by that of the Oxford school headed by Bradwardine—nor the elaboration by himself and John Buridan of the *impetus*-theory, nor even their acceptance of the possibility of the earth's diurnal motion have anything to do with nominalism or positivism.

Dr. Crombie, as a matter of fact, does not deny it. He praises, therefore, as the greatest achievement of nominalism, not the development of the *impetus*-theory, but its rejection by William Ockham in favor of a conception which, like many other historians,[13] he assimilates to the seventeenth century conception of *inertia*. I don't believe that this interpretation is quite correct, and that the text quoted by Dr. Crombie supports, or even admits it, though as a matter of fact it is, *for us*, a rather natural one. *For us* who remember the seemingly analogous pronouncement of Descartes who states that he makes no difference between motion and body in motion; *for us* who forget that for Descartes—as for ourselves—motion is first and foremost a *status* opposed to the *status* of rest, which it is not for Ockham, and which therefore—in opposition to Ockham's assertion—is a *new effect*, and an effect which requires for its production not only a cause, but even a perfectly determined one. It seems to me that, if we bear that in mind and do not read into Ockham's text something which is not there, we will recognize the impossibility of deducing from it such things as, for instance, conservation of direction and speed which are implied in the modern conception of motion, and will not burden him with the discovery of the principle of inertia.

I do not deny that, as Anneliese Maier has put it, Ockham's conception could have been developed into that of motion as *status*. It is sufficient for me to state that it wasn't, and that no one among the numerous pupils of the *Venerabilis Inceptor* has ever tried to do so: proof, at least for me, of its utter sterility. Indeed, the nominalistic approach leads to skepticism, not to the renewal of science.

Positivism is a child of failure and renunciation. Its birthplace is Greek astronomy and its best expression is in the Ptolemaic system. Positivism was conceived and developed not by the philosophers of the thirteenth century but by the Greek astronomers who, having devised and perfected the method and pattern of scientific thought (observation), found that they were unable to penetrate the mystery of the true movements of the heavenly bodies and who, therefore, restricted their aim to "saving the phenomena," that is, to the purely formal handling of observational data, a procedure that enabled them to make valid predictions, but which was paid for by the acceptance of a final divorce between mathematical theory and underlying reality.[14]

13. Thus, quite recently, H. Lange, *Geschichte der Grundlagen der Physik*, Bd. I (Munich-Freiburg, 1952), p. 159; cf. Duhem, *Etudes sur Leonard de Vinci*, II, p. 193; cf. *contra* Anneliese Maier, *op. cit.*, n. 1.

14. This view is expressed by Proclus and Simplicius and rigidly adhered to by Averroës.

It was this conception—by no means progressive, as Dr. Crombie seems to believe, but utterly reactionary—that the positivists of the thirteenth century, pretty much like those of the nineteenth and twentieth centuries, who only replaced resignation by boastfulness, tried to impose upon natural science. And it was in a revolt against this traditional defeatism that modern science, from Copernicus (whom Dr. Crombie, rather surprisingly, lists among the positivists[15]) and up to Galileo and Newton, accomplished its revolution against the shallow empiricism of the Aristotelians, a revolution based upon a deep conviction that mathematics was much more than a mere formal device for ordering data; in fact, the very key for the understanding of Nature.

As a matter of fact, Dr. Crombie does not disagree with my interpretation of the motives that inspired modern mathematical science. Thus in his excellent characterization of Galileo's epistemological attitude he writes (p. 309):

> Whereas in practice Galileo decided upon the truth of a "hypothetical assumption" by the familiar criteria of experimental verification and simplicity, it is plain that he was aiming at something more than merely to construct a convenient means of "saving the appearances." In fact he was trying to discover the real structure of nature, to read the real book of the universe. It was quite true that "the principal score of astronomers is only to render reason for the appearances of the celestial bodies" but a criticism he made of the Ptolemaic system was just that "although it satisfied an Astronomer merely Arithmetical, yet it did not afford satisfaction or content to the Astronomer Philosophical." But he said, Copernicus "very well understood that if one might save the celestial appearances with false assumptions in Nature, it might with much more ease be done with true suppositions." So it was not merely because of a pragmatic use of the principle of economy that the simpler hypothesis must be chosen. It was Nature herself that "doth not by many things what may be done by few," Nature herself that commanded assent to the Copernicus system.

At least it did so for Galileo who was deeply convinced that nature

15. This strange misinterpretation of Copernicus, whom Dr. Crombie moreover opposes to Galileo, saying (p. 309), "He [Galileo] refused to accept Copernicus' own statement that this was simply a mathematical device, a statement in keeping with Western astronomical opinion since the thirteenth century; the heliostatic theory was a literally true account of nature," is the only really important error that mars Dr. Crombie's excellent book. As a matter of fact, Copernicus never thought about his theory as being only a mathematical device and never made any statement that could be interpreted in that sense. It was Osiander and not Copernicus himself who expressed this view in the preface that he affixed to the first edition of the *De Revolutionibus orbium coelestium* in 1543—as in fact Crombie himself points out in his *Augustine to Galileo: The History of Science A.D. 400–1650* (London, 1953, 1956), p. 326.

was mathematical in its innermost structure. Indeed, as Dr. Crombie has put it, pp. 305–6:

> The special contribution that Galileo's conception of science as a mathematical description of relations enabled him to make to methodology, was to free it from the tendency to excessive empiricism which was the main defect of the Aristotelian tradition, and to give it a power of generality which was yet strictly related to experimental facts to a degree which previous Neoplatonists have seldom achieved. This Galileo did in the first place by not hesitating to use, in his mathematical theories, concepts of which no examples had been or could be observed. He required only that from such concepts it should be possible to deduce the observed facts. For example, there is no such thing as a perfectly fictionless plane or an isolated body moving in empty, infinite, Euclidean space, yet from these concepts Galileo first constructed the seventeenth century theory of inertia. "And, he said, I cannot find any bounds for my admiration, how that reason was able, in Aristarchus and Copernicus, to commit such a rape upon their senses, as in despight thereof to make herself mistress of their credulity."

Thus the Galilean conception of a correct scientific procedure implies a predominance of reason over mere experience, the substitution of ideal models (mathematical) for the empirically given reality, the prevalence of theory over facts. It is only thus that the limitations of the Aristotelian empiricism could be overcome and a true *experimental* method developed; a method in which mathematical theory determines the very structure of the experimental research, or, to use Galileo's own words, a method which uses the mathematical (geometrical) language in order to formulate its question to nature, and to interpret its answers; which, substituting the rational universe of precision for the empirically given world of the more-or-less, adopts measurement as its fundamental, and most important, experimental concept. It is this method which, based upon the mathematization of nature that has been devised and developed, if not by Galileo himself—Galileo the experimenter owes his glory much less to his own work than to the efforts of positivist historians—then, at least, by his pupils and followers. Accordingly, Dr. Crombie seems to me to exaggerate somewhat the "experimental" aspect of Galilean science and the closeness of its relation to experimental data.[16] As a matter of fact, when Galileo stayed close to experience, as often as not, he made a blunder. Yet Dr. Crombie seems, nevertheless, to recognize the radical transformation which the new ontology brought to physical science, and even the very

16. Cf. my paper "An Experiment in Measurement in the XVIIth Century" in the *Proceedings of the American Philosophical Society*, 1952.

special meaning of the famous, seemingly positivistic, assertions of the great Florentine. Thus he writes (p. 310):

> The momentous change that Galileo, along with other Platonizing mathematicians like Kepler, introduced into scientific ontology was to identify the substance of the real world with the mathematical entities contained in the theories used to describe the appearances.

This was a truly momentous change which led to just as momentous changes in *methods*, as distinct from pure *methodology*. Dr. Crombie, however, prefers to use the latter term and writes accordingly (pp. 305–6):

> The important practical result was to open the physical world to the unrestricted use of mathematics. Galileo had removed the worst inconveniences of Aristotle's notion that there was a science of "physics" outside of the range of mathematics, by declaring substances and causes postulated by that physics to be mere names.

One is therefore rather surprised when, having learned from Dr. Crombie that modern science—that of Galileo and Descartes—not only uses new and unheard of modes of reasoning (from the impossible to the real) but is also based on a quite different ontology than the traditional science that it opposes, and that its struggle against tradition had a deep philosophical meaning, we read that, as the result of his investigations (p. 318):

> We reach the conclusion that despite the enormous increase in power that the new mathematics brought in the seventeenth century, the logical structure and problems of experimental science had remained basically the same since the beginning of its modern history some four centuries earlier. The history of the theory of experimental science from Grosseteste to Newton is in fact a set of variations on Aristotle's theme that the purpose of scientific inquiry was to discover true premises for demonstrated knowledge of observations, bringing in the new instrument of experiment and transposing it into the key of mathematics. The investigator tried to construct a verified system of propositions within which the more particular bore to the more general the relation of necessary consequence.

The key to Dr. Crombie's assertion lies, apparently, in the name of Newton. He believes, indeed, in the positivistic interpretation of Newton, about whom he writes (p. 317):

> His "mathematical way" was in fact related to the observations in the same manner as the mathematical "superior science" of Aristotle's Latin commentators, which "provides the reason for that thing of which the inferior science provides the fact" but which "does not speak of the causes of the thing."

Newton's object in distinguishing his "mathematical way" from the investigation of causes, for example, from the "nature and quality" of gravity or of the light, was, Crombie points out, "to dissociate his own works from the two most popular scientific ontologies of his time, those deriving from Aristotle and from Descartes, because they were not deduced from the phenomena." It was not that Newton doubted that it could be within the competence of science to discover the real causes of "phenomena" (p. 316), only that he hesitated "to assert that he had made such a discovery in any particular case."

This is true; yet I do not think that Dr. Crombie does justice to the brutal realism which Newton combines with the belief that the *real* causes of observable phenomena are either unknown or belong to a realm of being that is transphysical, such as, for example, the *spirit* or *spirits* that cause attraction and repulsion and are the *real* forces that hold the world together and also enable matter to be composed of atoms held together by *forces*. We have to deal with these mathematically, Newton says, and not bother when doing so about their real nature; yet we have to consider them, since they exist, as an eventual goal of scientific enquiry.

Because Dr. Crombie does not believe in this goal he considers that the science of Galileo and Descartes, based on a mathematical, Plato-inspired ontology, a science which aimed at a real, though of course partial and provisional, knowledge of the real world, pursued an impossible and even a false aim. Accordingly, he believes that Newton, who renounced investigation of causes, or at least put it off indefinitely, and divorced "experimental philosophy" from metaphysics (and even from physics), turned back to Aristotelian methodology and the nominalist epistemology of the Middle Ages.

Dr. Crombie interprets modern science as a decidedly positivistic one. It is therefore in the history—or prehistory—of positivism that he sees the progress of "experimental science." This history implies a philosophical lesson (p. 319):

The philosophical truth that the whole history of experimental science since the thirteenth century has brought to light is that the experimental method, originally designed as a method of discovering the true causes of observed occurrences, or facts, turns out to be a method of constructing true descriptions of them. . . . A scientific theory has provided the whole of the explanation that can be asked from it when it has correlated the facts of experience as accurately, completely and conveniently as possible. Any further questions that may be asked cannot be asked in the language of science. Of its nature such a description is provisional, and the practical programme of research is to replace limited theories by others ever more comprehensive.

Shall we accept Dr. Crombie's historico-philosophical lesson? I don't think we are called on to do so. For one who, as myself, does not believe in the positivistic interpretation of science—nor even in that of Newton—the story so brilliantly told by Dr. Crombie contains quite a different lesson: that empiry—and even experimental philosophy—lead nowhere; and that it is not by renouncing the apparently impossible and unnecessary goal of knowing the real, but, on the contrary, by boldly pursuing it, that science progresses on its endless path towards truth. Accordingly, the history of this progress of modern science should be devoted to its *theoretical* aspect at least as much as to the *experimental* one. Indeed, as I have already said, and as Dr. Crombie's own discussion of the logic of science makes abundantly clear, the former is not only closely linked with the latter, but even dominates and determines its structure. The great revolutions of twentieth century science, as well as those of the nineteenth and the seventeenth—though based, of course, on the discovery of new facts (or on the failure to ascertain them) are, fundamentally, *theoretical* revolutions of which the result was not a more perfect correlation of "facts of experience," but, in my view, a new concept of the reality underlying these "facts."

Yet there are many mansions in the Kingdom of God, and many ways of dealing with history. Let us say, therefore, that in the Kingdom of History, Dr. Crombie has built a very beautiful mansion.