Church may ultimately be enriched. But always this must be the goal. India must find the answer to her own quest for God in Christ and she must find it in her own way. It must come as the fulfilment of her own tradition, the end to which by secret ways God has been leading her from the beginning of her history.

ANCIENTS VERSUS MEDIEVALS

M. A. Hoskin

Lecturer in the History of Science in the University of Cambridge

'IN time no less than in regions there are wastes and deserts.' So wrote Francis Bacon with medieval science in mind. And when Whewell two hundred years later spoke of 'the almost complete blank which the history of physical science offers, from the decline of the Roman Empire, for a thousand years', he was only summing up a view of the Middle Ages generally accepted from the seventeenth century down to our own times.

Historians of science did not need much encouragement before passing rapidly and gratefully over the medieval period. Many of the works had never been printed and existed in manuscript only; and everywhere one was confronted with an unfamiliar terminology and barbarous style. That we now know as much as we do about medieval science is due in the first place to the French physicist Pierre Duhem. He re-examined works that had been untouched for centuries and in his monumental treatises of fifty years ago made some disconcerting claims for medieval physicists, especially for his fellow-countrymen of the fourteenth century. To take just one example, Nicolas Oresme emerged as the inventor of co-ordinate geometry, and even as a precursor of Copernicus, although he discussed the motion of the earth only to reject it. Not surprisingly, many took the view that Duhem had overstated his case, but now the medievals had their champion. Issues had been raised, and evidence must be heard.

Unfortunately, the evidence was not forthcoming, unless one accepted Duhem's quotations at their face value, and overlooked his tendency to tear them out of context and to offer only his translation even when this meant imposing his own interpretation. And there, by and large, the matter rested for thirty years.

In the last two decades, however, the history of science has emerged as a university discipline. This has provided scholarpower for medieval studies, and as a result of the work of Marshall Clagett's group in America, Anneliese Maier on the continent and A. C. Crombie at Oxford, there has been a striking increase in our knowledge of the physics and philosophy of science of the Middle Ages.

So, for example, whereas only eight years ago a medievalist could say that during the Middle Ages 'Archimedes had been almost or entirely unknown', recently Dr Clagett has announced a complete book under the title *Archimedes in the Middle Ages*. But because later writers have been anxious to avoid Duhem's faults of historical method, their own works have been presented cautiously and with careful documentation. To put it frankly, they are not light reading, and this is probably the reason why specialists in other periods have sometimes been unaware of what has been going on in medieval studies. Authoritative surveys of this new material were urgently needed.

Fortunately the situation has improved in the last few months. In this country we are still awaiting the enlarged edition of Crombie's summary of the history of science from Augustine to Galileo; but recently Father J. A. Weisheipl has published a short but important booklet on *The Development of Physical Theory in the Middle Ages*,[1] and last November saw the appearance of Clagett's 700-page volume, *The Science of Mechanics in the Middle Ages*.[2] Dr Clagett quotes passages from the most interesting medieval works on mechanics, along with extracts from classical and early modern authors for comparison. Commentary there must be, but Dr Clagett's object is to present the evidence rather than to argue a case, and the reader is encouraged to come to his own conclusions.

The new evidence is destroying some cherished illusions about the work of Galileo. I should like to illustrate this by considering one example in detail. Here are two typical passages from books published only last year:

The first runs: 'Acceleration, as we understand it, was one of Galileo's fundamental contributions. It involves the conception of the indefinite splitting up of time, and thus of the application to time of the doctrine of limits as Archimedes had applied it to space.'

And the second: 'It was Galileo in his *Discorsi* who, in describing the movement of an accelerated body, made the revolutionary step of introducing time as a coordinate analogous to spatial coordinates,

[1] *The Development of Physical Theory in the Middle Ages*. By James A. Weisheipl, O.P. (Sheed and Ward; 4s.)
[2] *The Science of Mechanics in the Middle Ages*. By Marshall Clagett. (Oxford University Press; 50s.)

and of expressing physical quantities (such as position and velocity) as variables depending on time'.

These are strong assertions, and their authors are evidently unaware of the work of the fourteenth-century school at Merton College, Oxford. Dr Clagett quotes extensively from the Merton manuscripts. In fact, he shows that Galileo's definitions of uniform velocity and uniform acceleration have almost exact Mertonian counterparts, and he devotes a whole chapter to the so-called 'Merton theorem of uniform acceleration'. This theorem tells us that, for example, a body starting at two units of speed and uniformly decelerating to rest covers as much ground as a body moving for the same time with one unit of speed. There are many Mertonian proofs of this theorem, several of them due to Richard Swineshead. It would be useful to consider some of the features of one of Swineshead's proofs.

Swineshead begins by dividing the length of time into infinitely many parts, each part half the length of the preceding part: this is a clear example of the 'indefinite splitting of time' credited to Galileo. He then compares the velocity of the body at an instant of one interval with its velocity at the corresponding instant of the following interval—using, that is, the concept of instantaneous velocity often said to be due to Galileo—and, by an argument we would now express in terms of the calculus, he concludes that in the whole of one interval the body moves four times as far as in the whole of the following interval. He then takes, first the infinitely many intervals making up the *whole* period, and secondly the infinitely many intervals making up the *second half* of the period; and because he has shown that a body travels four times as far in an interval of the first set as it does in the corresponding interval of the second set, he argues that in the whole period the body travels four times as far as in the second half of the period, from which the theorem he requires follows. This is a remarkable example of the use of infinite series and limits, and one of many to be found in the writings of the Mertonians.

Now, even without going into the evidence that Galileo knew of the Mertonians and their work, we can say that Galileo's greatness did *not* lie in introducing time as a coordinate, in making position and velocity depend on time, or in applying to time infinite series and limiting processes, for all this was done centuries before.

But this is not to say that Swineshead has dethroned Galileo. You might think that an advance that in the seventeenth century was 'fundamental' or 'revolutionary' would be nothing short of sensational in the fourteenth. But, curiously, the very opposite seems to be true; an achievement transferred from Galileo to the medievals is found to lose some of its lustre. And there is justification for this. Something quite extraordinary happened to science during Galileo's lifetime; and the sense of revolution is not dispelled, however much of evolution there is shown to be. Yet it is remarkably hard to pin down just what constituted the revolution. Sometimes, as in the passages I quoted, writers are lured into setting too high a value on some conceptual step simply by the great things Galileo did with it. It seems a magic wand, an 'open Sesame', and only when the same concept is found in use earlier is it seen to be no such thing at all. By helping in this way to eliminate unsatisfactory candidates for the role of revolutionary ideas, studies in medieval science clarify our understanding of the seventeenth century.

One of the most influential studies of Galileo was written by Alexandre Koyré twenty years ago, just at the beginning of this recent spate of work in medieval physics. A philosopher by training, Koyré has taught us to see the fundamental importance of the underlying conception of nature accepted in a given period, and the great difficulty of breaking away and forming a new conception; which explains, he says, 'why the discovery of such simple and easy things as, for instance, the fundamental laws of motion, which today are taught to, and understood by, children, has needed such a tremendous effort—an effort which has often remained unsuccessful—by some of the deepest and mightiest minds ever produced by mankind: they had not to "discover" or to "establish" these simple and evident laws, but to work out and to build up the very framework which made these discoveries possible. They had, to begin with, to reshape and re-form our intellect itself; to give to it a series of new concepts, to evolve a new approach to being, a new concept of nature, a new concept of science, in other words, a new philosophy.' To Koyré, the Galilean revolution can be boiled down to the discovery of the language in which questions must be put to nature, in Koyré's words, 'to the discovery of the fact that mathematics is the grammar of science'.

The recent work appears to contradict Koyré's thesis. We now know that Roger Bacon was not an isolated figure in the thirteenth century when he said: 'It is impossible to know the things of this world unless one knows mathematics'. Robert Grosseteste and Robert Kilwardby were other Englishmen of the period to fall into what Albert the Great called 'the error of Plato'. And the Mertonians of the fourteenth century accepted a single mathematical law describing changes produced by the action of forces of all kinds. But Koyré's argument stands. The Mertonian laws operated *within* the

Aristotelian cosmos, the cosmos in which a falling stone moved naturally to take up its place in the scheme of things, or an arrow moved violently when projected into the air away from its place at the centre. Motions such as these could be classed as either natural or violent, and were temporary.

But the mathematical and mechanical world that emerged in the seventeenth century knew nothing of natural and violent motion; there was no more difference between one place and another than between one point in geometrical space and another. Koyré, in emphasizing the mathematical character of the seventeenth-century universe, is putting his finger on a philosophy of nature not indeed entirely novel but one that in a short space of time revolutionized the thinking of poet and peasant as well as scientist. The critical steps are those taken by Galileo himself, and Koyré is surely right in making these the focal point of the scientific revolution.

On the other hand, inroads have, I think, been made into some of Koyré's subsidiary judgments, particularly when he draws a sharp contrast between what he calls 'modern, mathematical, Archimedean or Galilean physics' and the physics of the Middle Ages—largely qualitative, according to the evidence of twenty years ago. Galileo had to wean men's minds from the all-too-plausible cosmos. As Koyré puts it, 'You must begin by re-educating them. You must proceed slowly, step by step, discussing and rediscussing the old and the new arguments; you must present them in various forms; you must multiply examples, invent new and striking ones. . . .'

One of Galileo's most famous examples is that of a ball rolling on a smooth horizontal plane—motion which refuses to fall into the Aristotelian categories of natural and violent, for the ball gets neither nearer to nor further from its natural place at the centre of the earth, and motion too which Galileo (as against Aristotle) asks us to see as liable to continue indefinitely. This we accept only if we are willing to disregard friction and other impediments which in practice must always bring the ball to rest. In other words, we must make mathematical abstractions, just as Archimedes did in his statics and hydrostatics; and Koyré speaks of 'the Archimedean world of Galilean physics, this world in which all the exterior obstacles to movement are removed in advance'.

However, as Dr Clagett's book clearly shows, abstractions such as those adopted by Archimedes appear equally in the medieval treatises on statics, and, at a highly theoretical level, in the mathematical physics of the Mertonians. That Galileo personally owed a debt to Archimedes is beyond doubt, but his adoption of mathematical abstractions was not the sharp breakaway from medieval science it seemed twenty years ago. And, if we are to compare ancient and medieval features of Galileo's thought, we must remember that Galileo's science was one of motion, whereas Archimedes's was one of rest; and surely from this point of view it is the Mertonians, with their single mathematical law governing motion of all kinds, who are nearer to Galileo's thought.

Even among the Parisians we find at least one example of mathematical abstraction curiously similar to the idealized rolling ball of Galileo. This occurs in the work of John Buridan, who noted that a mill continues turning for a long time after one has stopped pushing it. 'Perhaps', he says, 'if the mill would last forever without some diminution or alteration of it, and there were no resistance corrupting the impetus, the mill would be moved perpetually by that impetus.' This is surely very close to Galileo's 'ball exactly round and a plane exquisitely polished, so that all external and accidental impediments might be taken away'. Galileo would have accepted Buridan's example, and no doubt Buridan his. Both men recognized the possibility of the persistence of motion under certain circumstances.

But here we see too the differences between Galileo and Buridan. For Buridan, the theoretical motion of the mill-wheel is a detail of his teaching which remained by and large based on Aristotle, even though just how this particular doctrine was to be fitted into an Aristotelian framework is far from clear. Galileo, on the other hand, discusses such examples expressly *because* they fail to fit into the Aristotelian scheme: 'in which the question of circular motion is considered', runs the title of a chapter in an early work, 'whether it is natural or violent'. In other words, for him they are weapons to be used against the Aristotelian position and at the same time examples which will help him to construct a new philosophy to take the place of the old. Buridan, if you like, was a sleepwalker, unaware of the vital clue on which he had stumbled. Galileo, on the other hand, saw, as few others have done, the direction in which science must move and the contributions he himself could make; at his death the new science was a going concern and no longer a matter largely for academic discussion. Scholarship in medieval science is helping us to see Galileo's true genius at the centre of what remains the scientific revolution.