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SCIENCE AND HISTORY

Contents

Section: Essay review

A review of Philosophical Concepts in Physics: The Historical Relation between Philosophy and Scientific Theories. By J. T. CUSHING. (Cambridge University Press, 1998.) Pp. xix + 424. £55.00 (hbk), £19.95 (pbk). ISBN 0 521 57071 9 (hbk), ISBN 0 521 57823 X (pbk). Scope: text. Level: undergraduate and general.

When I put down Cushing's book, I was rather envious of his skill. He has packed many topics of considerable interest into Philosophical Concepts in Physics and has done it all with a light touch. The book is intended for students of both the sciences and humanities with some first knowledge of university-level physics, but I am sure many people with advanced knowledge of physics and current debates about the history and philosophy of science will enjoy it.

The book is ideally suited for its primary purpose as a text in undergraduate courses in the philosophy of science. Cushing has covered almost all the topics one would wish to find in such a book, and his basic format is well chosen. The individual topics are self-contained, intrinsically interesting, and well suited to discussions held once or twice a week throughout a semester. At the same time, the individual topics hold together well, since they are linked by the main issues raised in the book. These are basically three: How is science possible? How reliable are its conclusions? What does it tell us about what the universe really is?

The format works because the topics used to illuminate these three main issues are well chosen and splendidly highlighted by generous quotations from major scientists and commentary by Cushing. It is the skill in the selection of the quotations and the succinctness of the commentaries that make the book. Somewhat less satisfactory is the device of putting most technical material into appendices to the various chapters. This was done to make the core material accessible to students of the humanities with only limited knowledge of the science. My guess is that they may still remain somewhat out of their depth, while the scientists might have preferred a more integrated text.

The book consists of 25 chapters, which are grouped into nine parts. Three parts cover general questions or are reflections on the nature of science. The opening part, The Scientific Enterprise, is especially well done. The reader is introduced painlessly to the methods by which science is to be advanced that were advocated by Aristotle, Descartes, and Francis Bacon and to the fundamental problems raised by Hume, Mill, and Popper: What warrant do we have for believing the findings and pronouncements of scientists and what criterion do we have for telling good science from bad? The impossibility of certain induction from empirical data and Popper's alternative, more modest insistence that our theories must be falsifiable and can never be more than tentative are clearly presented.

In his preface, Cushing explains that although he wants to show that philosophical considerations have played an essential and ineliminable role in the actual practice of science he has devoted more space to the history and content of science than to philosophy of science per se. This is because, in his view, 'it takes a lot of history of science to anchor even a little bit of philosophy of science'. The history of science is covered in the remaining six parts of the book, which treat ancient models of the universe, the Newtonian universe, mechanical versus electrodynamic world views, relativity, quantum mechanics, and the acute philosophical problems raised by quantum mechanics, especially the Einstein-Rosen-Podolsky paradox.

This final part is a tour de force. Cushing argues that it could well be a mere historical accident that the Copenhagen interpretation of quantum mechanics, and not the rival de Broglie-Bohm pilot-wave interpretation, is the current orthodoxy. I was initially rather sceptical about this claim and still do not fully swallow it. Nevertheless, Cushing is able to marshal some surprisingly effective arguments. For me, he presses his case a bit too far by claiming that the two interpretations are completely equivalent. Provided one does not insist strongly on the physical reality of wave-function collapse, I would argue that the Copenhagen interpretation is simply a statement of objective facts that relate the theoretical wave function to observations that can be made in different experimental set-ups, while the de Broglie-Bohm interpretation is a putative explanation of these facts by means of conjectured hidden variables.

In my view, the quality of the history of science increases markedly on the transition to the more modern parts--relativity and quantum mechanics. I suspect that this is because Cushing writes with first-hand knowledge of these subjects, whereas for the earlier part he seems to have relied rather heavily on secondary sources, above all Thomas Kuhn's The Copernican Revolution, which have not served him well. This is a pity, since deficiencies in the early part do detract somewhat from the book and leave the conclusions that Cushing draws less well founded and definite than they could be. I should like to go into this in some detail, partly because far too many scientists (and others) have a very imperfect awareness of what really happened in ancient astronomy, and partly because this leads, I believe, to a somewhat inaccurate picture of how science actually developed. Cushing is absolutely right to emphasize how much history of science is needed to anchor a little philosophy of science. He could have done with a bit more of the real early history.

Let me begin with some general observations. Many professional scientists are today horrified at the way most historians of science depict the subject and by the extreme claims of post-modernists that scientific theories are no more objective than myths. Science, it is said, is nothing more than one story among many. As another reviewer of Cushing's book has remarked [1], the defence of science has largely been left to the scientists themselves (of whom Cushing is one--he is a physicist, not a historian or a philosopher). However, when working scientists turn their hand to the history of science they are frequently accused by professional historians of writing 'Whig history', treating everything in the past as a progression towards our present admirable state of knowledge. Instead, it is argued, the past should be studied in its own right and in its own terms. These are held to be very different from present norms. Thus, it is argued, there is nothing special and immutable about the practice of science--it changes like all other human activities.

I think that Cushing could have done more to defend science from such charges, especially if he had had a more detailed understanding of what did actually happen in ancient astronomy. This would have highlighted a fact about science that receives too little attention in all these general discussions of how science is or should be practised and interpreted. More account should be paid to the role played by Nature itself and by fortunate circumstances of our location in the universe. Einstein often made the claim that scientific theories are free creations of the human mind. He dismissed the idea that empirical data by themselves suggest scientific theories. Einstein, in turn, had a great impact on Popper, who regarded theories as daring conjectures that had value only to the extent that they could be falsified. Since then, much discussion has revolved around the merits of the Popperian thesis that empirical data are used to reject theories rather than suggest them through a process of induction.

It seems to me that Einstein was a very special case and that if the enterprise known as science is viewed in its entirety a rather different picture emerges. Intuition and conjecture still play a vitally important role, but they do so within a framework that, at least for physics, creates its own logic and methodology. What I have in mind is the working assumption of an external world that is ordered by the laws of geometry and kinematics. Geometry itself was almost given to humanity on a plate, since evolution on the stable earth equipped our hunter-gatherer forbears with an exceptionally good sense of the spatial disposition of things. Effectively rigid bodies and the stable ground meant that many laws of Euclidean geometry were strongly suggested as empirical facts. The early emergence of geometry as an extraordinarily precise scientific discipline confirms this.

Especially since Thomas Kuhn published his immensely influential The Structure of Scientific Revolutions, many commentators have emphasized what they see as a lack of continuity in science, which is supposed to lurch unpredictably from one paradigm to another. But this completely ignores the fact that since Euclid codified the facts of geometry around 300 BC the basic presupposition of real science has not changed in the slightest. It is that the world is to be described and explained by things moving and changing in space and time, which constitute a well-defined arena. Within this basic conceptual framework, progress is to be achieved by making ever more accurate observations of an ever wider class of phenomena and attempting to describe them by laws.

Now it is certainly true that the early practitioners of this method--Archimedes and the ancient astronomers--could not have had any inkling of where it would all end. The only point I want to make is that once the programme had been set up Nature itself guaranteed its success. It is true that we now have a somewhat more sophisticated geometry, but it is still geometry. Moreover, in many ways the change from Euclidean to Riemannian geometry is as nothing compared with the difference between no geometry and Euclidean geometry. And even in the most abstract developments of quantum mechanics we are still studying changes that take place in space and time. We have come a long way from the motion of rocks, arrows, and planets, but still a wave function does evolve in accordance with definite laws, while the spatial arrangement of instruments used to make measurements in quantum mechanics is vital. Plus ca change, plus c'est le meme chose.

It is particularly instructive to see how things worked out in astronomy up to the time of Newton. This is where Cushing, relying on inadequate sources, missed a great opportunity to anchor his philosophy of science much more securely. The essential point he failed to appreciate--and it completely escaped his main source (Kuhn's The Copernican Revolution)--was the degree of sophistication already achieved by Ptolemy in his Almagest (written around 150 AD). At this point, I must go into some details. Since they are fascinating in their own right and far too little known, this will not harm. I have written about them at much greater length elsewhere [2].

Most people who have studied ancient astronomy know that Ptolemy introduced something called the equant, but very few know what it actually is. This makes it completely impossible to understand the work of Ptolemy, Copernicus, and Kepler. The fact is that Ptolemy discovered two extraordinarily good approximations to Kepler's first two laws. Let me recall them. Kepler's first law states that the planets move in ellipses that have the Sun at one focus. The second states that the area swept out by the radius vector from the sun to the planets increases uniformly with time. Now ever since Newton illustrated these laws by means of an ellipse with eccentricity of 2/3, for which the relative displacement of the Sun from the centre of the ellipse is 2/3 (figure 1 (a)), people have mistakenly believed that the planetary ellipses are sensibly oval. In fact, had Newton shown them as they actually are, his readers could not possibly have distinguished them from circles, which they are to a very good approximation. To the eye, ellipses with eccentricity 1/10 (figure l(b)) or even 1/5 are indistinguishable from a circle. Only when the eccentricity reaches 1/3 does the ellipse begin to look oval (figure 1 (c)).

To appreciate the subtleties of ancient astronomy, it is essential to understand the difference between the eccentricity (the relative amount by which the loci are offset from the centre of the ellipse) and the ellipticity, which is the relative flattening, defined as (a - b)/a, where a is the semi-major axis and b the semi-minor axis. The key point is that the ellipticity is proportional to the square of the eccentricity. At this point it will be helpful to recall the actual eccentricities of the various planetary orbits. Working outwards from the Sun, the eccentricity of Mercury's orbit is about 1/5; for Venus it is very small, about 1/150; for the Earth too it is small, about 1/60; for Mars in contrast it is relatively large, about 1/11; for both Jupiter and Saturn it is around 1/20.

Now we must consider how readily the effects of such eccentricities could be observed with the naked eye. The Sun and Moon each subtend an angle of about half a degree (30 minutes of arc) on the sky. Any observer prepared to make a little effort can measure angles on the sky to an accuracy of about one third of that. This meant that the effect of the various planetary eccentricities could be readily observed, and indeed Aristotle already knew that the Sun did not move uniformly round the ecliptic during the course of the year but had a more rapid motion in the winter and a slower one in the summer. From the Copernican point of view, the solar motion is just the reflection of the Earth's motion, and Aristotle's knowledge shows how readily the small eccentricity of the Earth's orbit was detected. In contrast, the effects of the ellipticities of the orbits, being proportional to the square of the eccentricities, were far too small to be detected by naked-eye observations except for Mercury, which however is very difficult to observe, being so close to the Sun. It therefore played virtually no role in ancient astronomy. Few writers on ancient astronomy have been aware of the vital distinction between eccentricity and ellipticity. They did not appreciate that the effects of the eccentricity were easy to observe, while the effects of the ellipticity were virtually impossible to detect.

Even more serious has been their failure to understand the nature and significance of the equant. It corresponds to a remarkable and beautiful effect. If one could stand on the Sun and watch any of the planets as it tracked around its orbit, its motion relative to the background of the fixed stars would appear distinctly non-uniform. There are two reasons for this. First, the Sun is offset by its eccentricity from the centre of the orbit, so that there is a purely geometrical effect, which makes the planet appear to be moving slower when it is further from the Sun. In addition, there is also a genuine physical effect--the planet really is moving slower when it is further from the Sun, as it must in accordance with Kepler's second law. If one could travel to the centre of the planet's orbit and hover there in a spacecraft, the geometrical effect would disappear, but the planet would still seem to move non-uniformly because of the genuine physical non-uniformity of its motion.

However, if one were then to fly on to the void focus of the planet's orbit (as far as the Sun from the centre but on the opposite side, see figure 1) and hover there, one would discover the effect that dominated ancient astronomy from Ptolemy's discovery of it until Kepler found his first two laws in 1604. From the void focus, the planet appears to move round against the background of the distant stars with almost perfect uniformity. The reason for this is that the geometrical effect of the shift from the centre to the void focus exactly offsets the genuine physical slowing down of the motion when the planet is furthest from the Sun. In fact, exactly half of the non-uniformity of the planet's motion as seen from the Sun is due to the physical effect, while the other half is due to the geometrical effect of the displacement of the Sun from the centre of the orbit. The void focus thus has the remarkable effect of 'equalizing' the observed motion of the planet, hence the name equant, which Ptolemy called it when he found it in geocentric guise sometime before 150 AD. One can see the equant phenomenon as a remarkably good approximation to Kepler's second law, while the fact that the planets move in nearly perfect circles with their centres displaced from the Sun is an equally good approximation to his first law.

These crucial facts escaped nearly all historians and philosphers of science, especially Kuhn, and this has had the consequence that even today the actual history of ancient astonomy is seen, as it were, through hopelessly blurred glasses. Very few people are aware of what the key issues were. Generalities about the methods and true nature of science are drawn on the basis of very poorly understood material. The extent to which the progress of ancient astronomy was dictated by the magnitude and kind of the various observable effects, i.e. was dictated by Nature, is not appreciated. The achievements of Ptolemy and Kepler are seriously underrated, while Copernicus is credited (by Cushing too) with the doctrine of heliocentricity (instead of terrestrial mobility, which is what he actually proposed). In fact, as far as Copernicus was concerned the Sun simply happened to be near the centre of his universe. It played so little role in his system that it does not even appear in the diagrams in which he explains the motions of the planets. The centre of the Copernican universe was the void focus of the Earth's orbit!

The way this came about is both intriguing and illuminating. It shows how the ancient astronomers invented the true scientific method but also reveals the pitfalls they faced. The first really great step was taken by Hipparchus, who lived about 300 years before Ptolemy. It was he who started to turn qualitative conjectures about the motions of the 'wanderers' (Sun, Moon, and five naked-eye planets) into quantitative theories, testing them by observations. Given the Greek enthusiasm for the idea of perfectly circular motion, it was very natural that he attempted to explain the known non-uniformity of the Sun's annual motion around the ecliptic by the assumption that the Sun does actually move in a perfect circle with perfect uniformity but that the centre of the circle is not at the centre of the Earth. Such a displaced circle became known as a deferent and its relative offset the eccentricity, (this is the astronomical origin of both modern meanings of eccentricity). With hindsight, Hipparchus made only one serious mistake. He tried to explain the entire observed non-uniformity of the solar motion by geometrical displacement, instead of explaining half by displacement and half by genuine nonuniformity. He doubled the eccentricity.

His mistake was hidden by a truly remarkable fluke. If one considers only the motion of the Sun, Hipparchus's incorrect theory explains its observed motion with extraordinary accuracy, to about I minute of arc (the anomaly that has to be explained can be as much as 300 minutes of arc at some times in the year). The error was below the level that even a phenomenally conscientious observer like Tycho Brahe could detect. For this reason the error remained undetected until Kepler, soon after he joined Brahe in 1600, found it with an absolutely inspired piece of detective work based on triangulation using the observed positions of both the Sun and Mars. Had the Earth's eccentricity been only very slightly larger, say about 1/20 as for Saturn or Jupiter, the error would have been detected far earlier.

Unless one suspected it, as Kepler did, the existence of the equant phenomenon was essentially invisible in the apparent motion of the Sun. It was quite a different matter for the planets. This is because their apparent motion against the background of the fixed stars arises (in the Copernican picture) from two causes: their actual heliocentric motions and the heliocentric motion of the earth. The two are compounded. The Greeks quite soon came up with a qualitative geocentric explanation of the resulting decidedly irregular motions of the five naked-eye planets. They supposed that an invisible 'guide point' moved uniformly around an eccentric deferent, exactly as in Hipparchus's theory of the solar motion, while the planet moved uniformly round a smaller circle that had its centre at the instantaneous position of the guide point. The smaller circle was called an epicycle. By this simple device, without being aware of the fact, the Greeks correctly separated the apparent motions of the planets into their two heliocentric components: the deferent motion represented the heliocentric motion of the planet, while the epicyclic motion reflected the annual motion of the Earth around the Sun.

However, for very understandable reasons the Greeks never succeeded in making the split perfectly cleanly, though Ptolemy, in a great achievement, came close to doing so. His failure had nothing to do with the fact that he was using the geocentric system. It was entirely due to the magnitudes of the eccentricities of the various orbits. In the apparent motions of the three outer planets Mars, Jupiter, and Saturn (all with relatively large eccentricities) the equant effect shows up quite readily in the deferent component if an observer makes a conscientious effort to test the eccentric-deferent-epicycle model. This is exactly what

Ptolemy did. He was the first scientist to actually carry out the great method of science: make a hypothesis on the basis of a first examination of observed facts, test it with more accurate observations, and change the hypothesis if the first one fails. Then repeat the testing procedure for the new hypothesis. Ptolemy did and it worked.

However, because the Earth has such a small eccentricity, the equant effect is far harder to detect in the epicyclic component of the models for the three outer planets. The situation for Venus was especially ironic. Because its orbit is inside the Earth's orbit, the role of deferent and epicycle are reversed in this case. The deferent is the geocentric reflection of the Earth's heliocentric motion, while the epicycle reflects the heliocentric motion of Venus. But Venus has an even smaller (significantly smaller) eccentricity of its orbit than the Earth. As a consequence, in this case too Ptolemy found an equant in the deferent motion but not in the epicycle.

Had Ptolemy been working to the accuracy Brahe achieved one and a half millennia later, he would have found the equant effect in all five deferents and all five epicycles of the five naked-eye planets. Instead, he found it in only the five deferents. Now when Copernicus had his brilliant insight that the epicyclic motions of the five naked-eye planets could all be explained by motion of the earth alone, he unfortunately had no further deep insights. He attempted to construct a new model of the solar system simply by taking Ptolemy's models and converting them unchanged. He got into an unholy muddle, since the models, when literally transcribed, implied that the Earth should be doing four different things simultaneously! The apparent motions of the three outer planets suggested one thing, while those of the Sun, Venus, and Mercury each suggested some different motion of the Earth. Since only one of the four possibilities could be correct, Copernicus opted for the quantitatively brilliantly successful (but actually seriously deficient) theory of the solar motion due to Hipparchus. It suggested that the void focus of the Earth's orbit was the centre of the universe, and there Copernicus put it. He made the Sun a mere lantern to illuminate the dance of the planets, which had to perform some very odd movements indeed. This was forced upon them once Copernicus had made his decisive choice--to retain Hipparchus's model and merely invert it. The error this introduced had to be corrected by bizarre motions of the other planets.

Many historians of science have completely failed to understand the nature and manner of the next great advance in theoretical astronomy, which was Kepler's. Cushing's comments on Kepler are sadly all too typical. He writes that Kepler found his laws 'largely empirically and by trial and error, with no coherent theory behind them. They were simply concise mathematical summaries of regularities that Kepler found by studying an incredible amount of data over a period of many years'. This is simply wrong.

Kepler started with one brilliant heuristic idea--that the Sun is not just accidentally near but precisely at the true centre of the planetary system because it actually controls the motions of the planets by physical forces. No one had dreamed of such a thing before. The notion at the heart of modern physics--that matter interacts with matter through forces--was born when Kepler had this idea. It was his constant guide in a series of brilliantly conceived assaults on the mountain of accurate data that Brahe had accumulated and made available to him. Kepler found his laws because he knew exactly what kind of phenomena he was looking for. His really great discoveries were all made in four years from 1600 to Easter Sunday 1604. He first proved beyond doubt the existence of an equant in the motion of all the planets and simultaneously established true heliocentricity beyond a shadow of reasonable doubt. The picture was then vastly clearer and in much sharper focus. But still a tiny defect remained in the motion of Mars. Kepler absolutely refused to accept it. His physical intuition constantly suggested new models, and relatively soon they bore fruit.

Far too many people have accepted Koestler's gross misconception that Kepler was a sleepwalker [3]. Nobody ever explored the solar system more totally wide awake than Johannes Kepler. The closest parallel that I know to his heuristically-driven work on the planets is Einstein's heuristic idea about the photon in 1905. How do you progress when something tells you the present conceptions are seriously wrong and you have a strong hunch it is necessary to go in the direction of a quite new idea? You grasp for any model that incorporates the idea and see how well it fits the data. Bit by bit you make progress. That is exactly what both Kepler and Einstein did.

I have probably already said a bit too much about ancient astronomy, which takes up only a relatively small part of Cushing's book. However, I do feel that shortcomings there distort the whole of the first third or so of the book, which is a pity, since so much else in it is excellent.

Let me conclude by summarizing the lessons that I draw from the history of ancient astronomy as it actually happened. Science does advance surely, progessively, and organically. Seen on the largest perspective, its methods have hardly changed over more than two millennia. It presupposes an external world in which laws of geometry hold and in which things move and evolve. Although Nature can play the occasional dastardly trick on scientists, as happened with Hipparchus's accurate but incorrect model of the solar motion, at other times it richly rewards those who are prepared to observe phenomena closely and attempt to understand them in a orderly geometrical space-time framework. Some contingent features of the world are so striking that, once they have been accurately delineated, they very strongly suggest their own interpretation.

This is what happened in astronomy. Solid work by Hipparchus and Ptolemy led to the quantitative elaboration of the deferent-epicycle model, which strongly suggested the heliocentric model. The residual oddities of this model coupled with the close proximity of the Sun to its centre suggested heliocentricity as a physically deeply relevant possibility. Kepler's brilliant demonstration of exact heliocentricity made the eventual emergence of Newton's theory of universal gravitation almost inevitable. In all these great advances, long chains of induction hardly played a role. Four equants in five planetary motions was suggestive and Kepler's discovery of the fifth clinched the matter. Heliocentricity was a unique fact, solidly established by geometry and kinematics. The contingent values of all five planetary eccentricities were vitally important, both helping and hindering advances.

It is said, and the history of science provides ample support for the contention, that whenever the accuracy of observations is increased by a factor of ten a major discovery can be expected. This alone demonstrates how science advances very largely in a systematic, progressive, and organic manner. Virtually all observable effects found in ancient astronomy were discovered and modelled exactly in the order that, with hindsight, one would have expected them to be made: the largest and most visible (in the Moon) first, then the regular but still anomalous behaviour of the Sun, then the qualitative behaviour of the planets, then the equant phenomenon, then Kepler's laws, then Newton's theory of universal gravitation. It seems to me that the biggest shortcoming of all too many theorists of science (Kuhn, Popper, etc) is that they often lack a feel for the really salient features and for the importance of orders of magnitude. Sheer contingency such as the uniqueness of the Sun and the rigidity and uniform rotation of the Earth also need to be taken into account.

It is a pity that the sources for the early part of Cushing's book let him down on these matters. They are not nearly so well anchored as the more modern developments, for which his book is certainly recommended.

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CHART: Figure 1 (a) Newton's diagram of the elliptic motion of the planets shown with greatly enlarged eccentricity. The sun is at one focus (S), while the void focus (Ptolemy's equant) is at H. (b) An ellipse with eccentricity 1/10 (corresponding approximately to the orbit of Mars). (c) An ellipse with eccentricity 1/3.

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2 *Barbour, J. B., Absolute or Relative Motion?, Vol. 1. The Discovery of Dynamics, Cambridge University Press (1989).*

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