Giant steps towards reality

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I. BERNARD COHEN
Revolution in Science
711pp. Harvard University Press. £22.75.
0674767772

The history of science is a subject with special responsibilities. The history of art, say, is under no special compulsion to represent past artistic endeavours as successful by contemporary standards. Nor do historians of music necessarily have to admire past music. Even historians of moral thought are free to reject the codes of behaviour that they discern in past societies. For in none of these areas of human activity is it clear that success answers to timeless standards. Science, however, cannot happily be viewed just as some kind of expression of transitory human concerns. For the aim of science is to describe an independently existing world, and success in describing reality cannot coherently be thought of as relative to time and place. If we hold that past scientists succeeded in delineating certain features of reality, then we ourselves must endorse the beliefs involved. And, conversely, if history shows us that most past science consists of beliefs that are false by present lights, then by implication history casts discredit on the whole enterprise of science.

Unfortunately, many contemporary historians of science fail to take their responsibilities seriously. This is especially true of those whose intellectual careers have begun in the past two decades. Far from portraying scientific beliefs as reflections of an independent natural world, this new generation of historians of science tends to explain scientific developments in terms of ideological resonances, or in terms of the institutional and rhetorical power wielded by successful scientists. So, for example, Pasteur has been argued to have been against spontaneous generation, not because of any experimental evidence, but because of the materialist and anti-creationist implications of the doctrine. And his success in winning others to his side has been attributed, not to any intrinsic merit in his views, but to his single-minded manipulation of the means of persuasion.

Indeed it is an article of faith among many young historians of science that the putative reality that scientific beliefs purport to be about should never be invoked in explaining those beliefs. And the apparent implication, that science fails in its professed aim of objective description, does not seem to worry them. Most would probably say that such objectivist pretensions are themselves one of the rhetorical devices by which scientists claim authority for their views. (Lest it be thought I exaggerate, I refer readers to the symposium of twelve historians of science in the April and May 1985 issues of *History Today*. Not all the contributors fit my characterization. But most of the younger ones do.)

To a large extent the new history of science is riding on the back of philosophical fashion. Sir Karl Popper has been preaching for some years that scientific views are not imposed on scientists by reality, but are free creations of inventive minds. According to Popper, scientific claims are never anything more than brave conjectures, put forward in a tentative spirit to await the inevitable fate of future falsification. And in more recent years T. S. Kuhn and others have added a further twist. At least Popper allowed that falsifications were objective responses to independent facts. But Kuhn denies even this. According to Kuhn, theoretical presuppositions so affect the way that scientists perceive the world that facts don't even have the power to force falsifications. Significant scientific developments consist, not of falsifications, but of "revolutions", new systems of concepts which give rise to radically new ways of interpreting the world.

Neither Popper nor Kuhn would themselves have much truck with the new history of science. Explaining science in social terms is anathema to Popperians. And even Kuhn is in practice far more interested in the internal imperatives of theoretical developments than in "external" influences on science. But it is a natural step from their anti-cumulativism to sociological extremism. For by denying that answers are forced on scientists by reality, Popper and Kuhn cut us off from the natural explanation of why scientists say what they do. And neither of them really offers any alternative account of where new ideas come from. Appeals to "intellectual creativity" or "scientific genius" are in the end unsatisfying. So where else are we to look for real explanations, apart from in the social realm?

The post-Popperian rejection of the traditional view that science accumulates lasting truths may be fashionable. But taken at face value it is deeply implausible. As David Stove points out in his *Popper and After* (reviewed in the *TLS*, July 1, 1983), one of the most striking features of the modern world is the growth of our knowledge of nature. We now know that water is made of hydrogen and oxygen, that equal volumes of any gas at the same temperature and pressure contain an equal number of molecules, that parents each contribute one half of their children's complement of chromosomes, and many, many other things that were unknown 300 years ago. Yet Popper and those who follow him in effect deny that we know these things. For if the characteristic fate of all scientific claims were really rejection, as they maintain, then even such claims as these would be overturned in time, and our current faith in them would be misplaced.

I don't believe that any serious-minded and sufficiently informed person can really doubt that water is made of hydrogen and oxygen. And so no serious and informed person can really rest content with Popper's and Kuhn's vision of science. I don't want to deny, however, that Popper and Kuhn raise interesting questions. One interesting question, discussed at length by Stove, is how they manage to obscure the absurd consequences of their views from their readers. And, more substantially, there are real questions about the way science develops through history.

For even if science does accumulate lasting truths, this isn't by any means a straightforward matter. Popper is quite right to point out that new scientific ideas often correct old ones by indicating various errors and imprecisions (as, say, Newton's ideas did to Galileo's and Kepler's). And Kuhn is quite right to hold that new ideas sometimes (if rather less frequently) involve wholesale conceptual reorganizations, in which all previous assumptions come to be seen in a new light (as, say, with the Darwinian and Einsteinian revolutions). But the right response to all this isn't to conclude that scientific consensus is essentially transient, always mistaking the way things really are. Rather we need to find some way of thinking of scientific developments as improving and deepening our representations of reality. We need to understand how science manages to preserve the core of previous discoveries when revising our views of nature. For if we can't do this then there is no avoiding the disastrous conclusion that *sub specie aeternitatis* (which, after all, is the only perspective that matters when our aim is successful description) all faith in scientific theorizing is misplaced human folly.

Nor do I want to deny that there are interesting questions to be answered about the social processes in which scientific activity is embodied. We certainly need to get beyond the naive realist view that the truth will automatically reveal itself to any healthy-minded scientist who looks hard enough in the right direction. Reality does not always wear its character on its sleeve, and in most cases a whole tradition of prior discoveries is needed to tease its secrets from experiment and observation. And, apart from this, new ideas will not even get a hearing if the general intellectual climate is wrong. Important here can be a sympathetic ideological background, or the availability of suitable models from other areas of thought, as well as the right kind of theoretical context within science itself.

It should not be thought that social influences will cancel out once new ideas are subject to the rigours of experimental test. For the conduct of experiments, and especially the persuasive processes by which particular scientific groups establish their experimental results as authoritative, are themselves social activities, and can be rewardingly studied as such. Indeed there is no question but that much of the new work in history of science has been extremely revealing about the institutional interactions and rhetorical devices which determine whose results get written into the scientific archives.

But one can accept all this without accepting the extremist thesis that natural reality never plays any part at all in determining what scientists believe. Of course there are some cases where scientists manage to put across stories whose creation has been quite unconstrained by the relevant features of reality. Thus the Lysenko episode, or the "discovery" of polywater, or, indeed, Pasteur's "disproof" of spontaneous generation (for wasn't the conclusion that life *never* arises *ex nihilo* a quite unreasonable extrapolation from the data?) Moreover, since there is no reason to think that scientists are any better than the rest of us, there is always a real danger that particular groups of scientists will claim the authority of "scientific fact" for purely conjectural or self-serving doctrines. But why suppose that this is the norm? Why suppose that they will always get away with it? Since the official aim of science is to tell it like it is, and since there are usually plenty of competing scientists straining to discredit any given proposal, one would expect doctrines that don't have the backing of reality to fall by the wayside. What the new history of science really ought to be showing us is how the institutions of science and the devices of scientists combine to ensure that only those doctrines that do fit reality manage to surmount the strenuous social obstacles in the way of general scientific acceptance.

I. Bernard Cohen is a historian of science from a rather older generation. For many years Professor of History of Science at Harvard, he has a long-established reputation for his work on Franklin, Newton and other aspects of early modern physics. His new book, *Revolution in Science*, is an ambitious work. As he explains, it aims to deal "with the chronological history and successive transformations of the concept of revolution in science in the seventeenth, eighteenth, nineteenth, and twentieth centuries – with illustrations taken from some major revolutions of each of these periods". A book of this description certainly promises to do much to remedy the current ills of history of science. But in the end the project turns out to be much easier said than done.

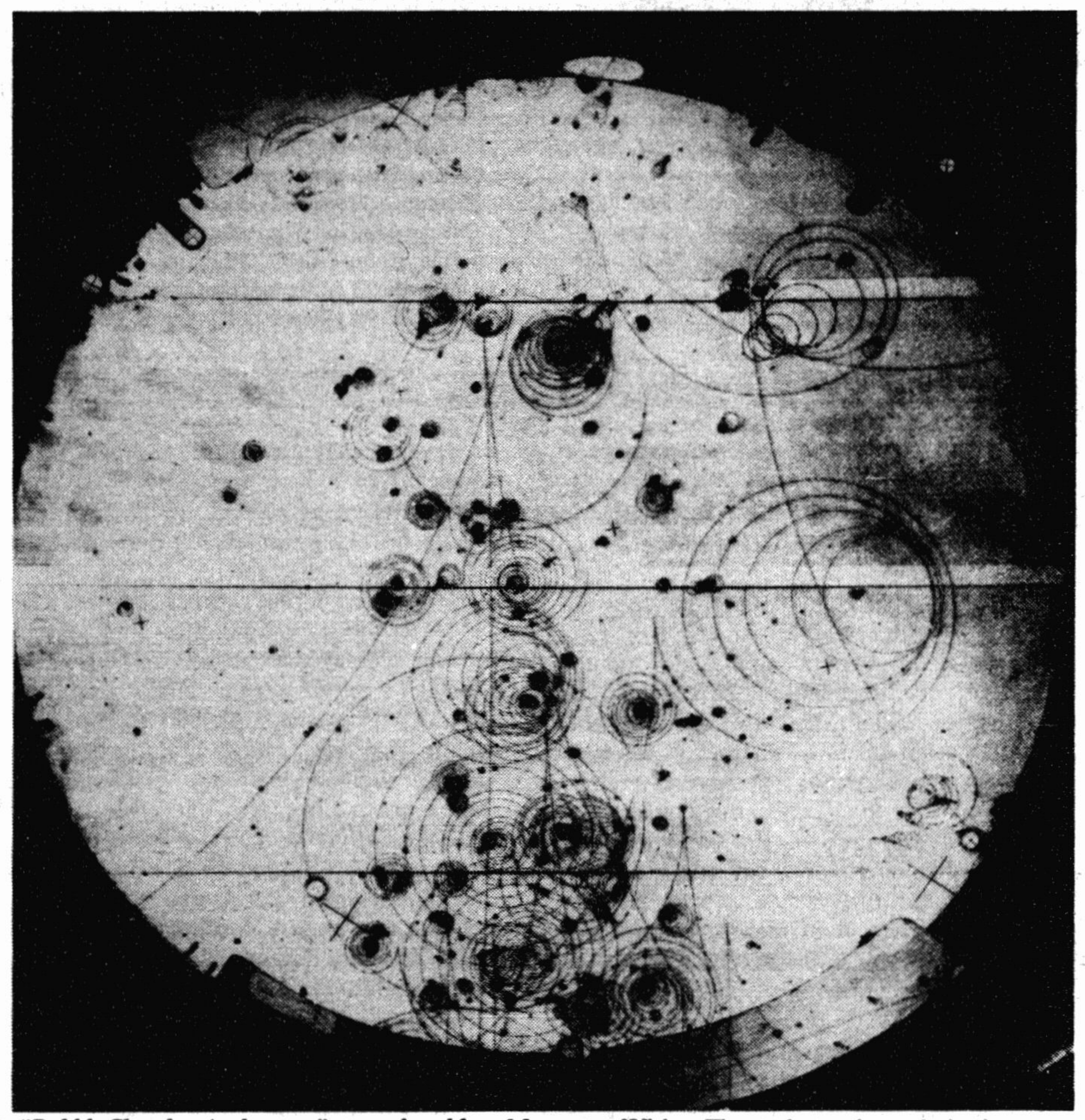
An underlying difficulty is Professor Cohen's vagueness about the concept of revolution itself. In his first sentence he characterizes revolutions as "giant steps forward that give us an altogether new perspective on the natural world". But we never get much beyond this. Though he mentions Kuhn, there is no suggestion that the "new perspectives" that come with revolutions are generally evidentially incommensurable with the old. Nor does Cohen even seem committed to the Popperian view that revolutions generally involve a rejection of what went before. Thus, for instance, he distinguishes "the Cartesian revolution . . . from many revolutions in science" on the grounds that "it did not last".

It would be unfair to call Cohen a naive realist. In some of his earlier works he has developed the important idea that key scientific developments generally proceed by "transforming" existing intellectual resources into new sets of theoretical assumptions. But he explicitly excludes such reflections from this book. And so in the end all we get here is the optimistic impression that scientific practice as such, aided by a smattering of "genius", is destined to keep theoretical progress on preordained rails.

How does Cohen tell whether an episode counts as revolutionary without any concept of revolution? His basic strategy is to see whether people *say* it was a revolution. More specifically, he has four tests. Did scientists and other intellectuals at the time deem it revolutionary? Did later workers in that field? Do historians of science, past and present? And, finally, do scientists today? No doubt these tests are evidence for something. But even if we leave the meaning of "revolution" to one side, I can think of many good reasons for not taking the judgment of any of these categories of people at face value.

Not that we really can leave the meaning of "revolution" to one side. For the meaning of this word has itself changed radically since the beginning of the seventeenth century. Originally a "revolution" stood for some kind of rotation of the wheel of time, and often signified a cyclical return to an earlier state of affairs. It was only when the Enlightenment idea of progress had gained general currency that "revolution" came to have its modern meaning of a radical alteration in some linear sequence. Cohen is of course aware of this, and indeed discusses these shifts in meaning at length. But it does make his tests for the occurrence of a scientific revolution extremely problematic when applied to the seventeenth and early eighteenth centuries. He can't go strictly on whether seventeenth and eighteenth-century figures actually applied the word "revolution", since they almost invariably didn't mean what we do. But since he is never entirely specific about what we ourselves are supposed to mean by a "revolution in science", it isn't ever clear what other seventeenth and eighteenth-century statements might be relevant instead.

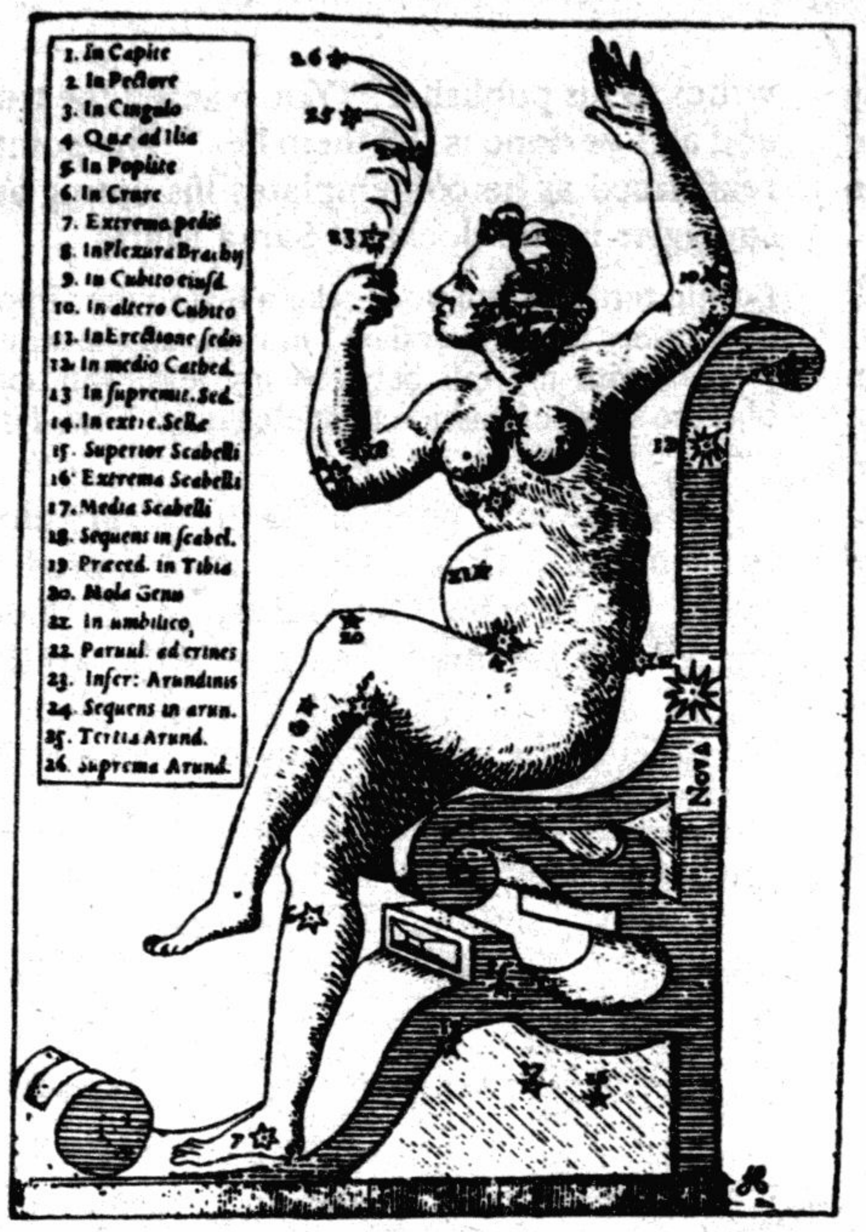
As it happens, his first-order descriptions of seventeenth-century science are the most suc-



"Bubble Chamber Arabesques"; reproduced from Moments of Vision: The stroboscopic revolution in photography *by Harold E. Edgerton and James R. Killian Jr. (176pp. MIT Press. £14.25. 0262550105).*

cessful part of the book. Here the reader gets the benefit of Cohen's close acquaintance both with the primary materials and with many years of historiography. But as we move away from the seventeenth century we move away from descriptions of revolutionary science to descriptions of what various figures have said about revolutions in science. This takes us down some curious pathways. There are chapters on Marx and Engels, and on Freud, which consist largely of reports and quotations of what they and others said about the revolutionary status of their own and other theoretical innovations. And there is a chapter on "Kant's Alleged Copernican Revolution" devoted entirely to the point that Kant never explicitly called Copernicus's hypothesis "revolutionary" (though Cohen does allow that Kant said, first, that his own work was revolutionary, second, that revolutions occur in science and, third, that his own work was analogous to Copernicus's).

Despite its title, *Revolution in Science* never really strays from the traditional picture of science as a steady accumulation of knowledge about the natural world. In the present climate of extremist anti-cumulativism this is only to be applauded. But although he diverges from present fashion, Professor Cohen never really explains what he thinks is wrong with it, and never gets properly to grips with the ideas that have seduced the new generation of historians of science. And because of this I fear he will do little to persuade his younger colleagues back to more responsible ways.



The constellation of Cassiopeia as depicted in Tycho Brahe's Astronomiae instauratae *showing the new star, marked "Nova", which the twenty-five-year-old Brahe had observed suddenly appearing (a supernova) on the night of November 11, 1572: reproduced from Norman Davidson's* Astronomy and the Imagination: A new approach to man's experience of the stars *(237pp. Routledge and Kegan Paul. £12.95. 0 7102 0371 3.*

The facts of the matter

John Polkinghorne

W. H. BROCK
From Protyle to Proton: William Prout and the nature of matter, 1785–1985
252pp. Bristol: Hilger. £25.
0 85274 801 9

William Prout was a successful physician, working in London in the first half of the nineteenth century and specializing in urinary medicine. With the energy and enthusiasm so characteristic of the period he also devoted himself to scientific research. As befitted a doctor, his principal interest was in "animal chemistry", the proto-biochemistry then in its infancy. However he is chiefly remembered today as the originator of Prout's hypothesis, the suggestion that the chemical elements are compounded of a single substance, which Prout named protyle and which, with some hesitations and reservations, he identified as hydrogen.

It is a curious fact that W. H. Brock's book is the first full-scale attempt to describe Prout's life and achievements. The material available is somewhat patchy, particularly for the last years of Prout's life, but it is presented with scholarly care and with attention to the cognate work of contemporaries. The meticulous character of Brock's discussion makes it the more surprising that there is persistent misspelling of quotations from the Greek. I wished from time to time, particularly in the sections on organic chemistry, that he had permitted the light of hindsight to play more often on the scene and that he had stated more clearly the relation of Prout's ideas to modern understanding.

In his early scientific life Prout attached great importance to exact measurement and was, indeed, awarded the Copley medal by the Royal Society for his development of an improved apparatus for organic analysis. This did not prevent him from exercising his speculative faculties and in later life they came to predominate. This, together with his distaste for the use of chemical notation, is in Brock's opinion why Prout made much less of a lasting impression upon biochemistry than did his great German contemporary, Liebig.

It is notoriously difficult to know how to assess speculative notions which subsequently prove to have more than a grain of truth in them but which are insecurely anchored in contemporary knowledge. Are they deep intuitions or just lucky guesses? Prout scored one success with his famous hypothesis about matter but he was well wide of the mark with the supposition that it was possible to construct a unitary theory of sensation. He was impelled to this search for simplicity by the belief that God would have made the world so, and perhaps natural theology provides the only sufficient reason for the elegant and economical intelligibility of the world. Prout was certainly a natural theologian, for he was the author of one of the Bridgewater Treatises, with the somewhat catch-all title of *The Chemistry, Meteorology and the Function of Digestion considered with Reference to Natural Theology*. He was also a vitalist, attributing to "organic agents" roles that we would now assign to enzymes.

However it is doubtful if anyone would devote a book to Prout today if he had not put forward his hypothesis of the compound nature of the chemical elements. He did so in two anonymous papers, published in 1815 and 1816, only taking public credit later when interest was aroused. All advances in our understanding of the nature of matter involve a two-fold process. The first step is the discernment of a pattern; the second is the interpretation of that pattern in terms of an underlying structure. Brock rightly insists that there were really two Proutian hypotheses. They correspond to these two steps. The first is what Brock calls the "multiples hypothesis", the suggestion that the atomic weights of all elements are integral multiples of the atomic weight of hydrogen. This is the pattern that Prout claimed to discern. He could not do so without some illegitimate manipulation of the data. In particular the stubbornly non-integral atomic weight of chlorine presented a problem which could only find its true solution much later with the recognition of the existence of isotopes. The second step was the "protyle hypothesis", the interpretation of that pattern in terms of hydrogen as the building-block for all the elements. Like so many new ideas this one was already in the air. Brock is illuminating in his description of the influence on Prout of Humphry Davy and others in the formation of his hypothesis.

By Chapter Six of this nine-chapter book Brock has finished with Prout, but the story continues, for the hypothesis lived on as a stimulus to further experiment and thought. The whole history of atomic theory is a refutation of the positivist account of the scientific endeavour, for it was only by going beyond mere observation that a point of view could be formed which enabled the asking of fruitful questions. Eventually, the interpretation of atoms as composed of electrons and nuclei and of nuclei as composed of protons and neutrons provided the accurate understanding of the structure of matter after which Prout had been groping with his famous hypothesis.

The story did not end there, however, for we now believe protons and neutrons themselves to be composites of the yet more basic quarks and gluons. A short final chapter gives a desultory and inaccurate account of that development.

The revised and updated edition of I. Bernard Cohen's *The Birth of a New Physics* (258pp. New York: Norton. $17.95. 0 393 01994 2) takes into account Thomas B. Settle's reproduction of one of Galileo's most famous experiments.

A mirror to a diamond

Brian Pippard

JAMES E. FORCE
William Whiston: Honest Newtonian.
208pp. Cambridge University Press. £25.
0 521 26590 8
GALE E. CHRISTIANSON
In the Presence of the Creator: Isaac Newton and his times
623pp. New York: Macmillan. £18.95.
0 02 905190 8

The historiography of science is dominated by Isaac Newton. Dozens of books, hundreds of learned articles, have tried to reveal every aspect of his life and work – and why not? He was, after all, among the most complete geniuses the world has seen, and I use the term deliberately. A man deserves to be called a genius when his achievements and mental processes put him beyond the scope of our envy. With other great ones we may feel that they possess in fuller measure qualities we recognize in ourselves, and we may wish we too had been a little more gifted, a little luckier perhaps. But in the man of genius we see something quite different, an instinctive penetration to the heart of problems we should not know how to start thinking about, and it does not cross our minds that we too might in happier circumstances have developed that way. I have known two or three in the world of physics who have seemed like that to me, and have no doubt that each, in his own way, would have said the same of Einstein, as he in his turn would have spoken of Newton. But when Newton wrote "If I have seen further it is by standing on the shoulders of giants", he intended no more than a formal compliment; nothing in his writing or his controversies suggests he could have tolerated the idea of a superior mind. No other in his own age even approached his intellectual distinction.

The fertility and penetration of his thought were matched by a heroic capacity for work, as can be judged by the size of his library (which he read thoroughly and extracted in tabular form) and the huge hoard of manuscripts which has survived; it is this, of course, apart from his scientific importance, that has led to the growth of a Newton industry. We have the means to know him as well as any thinker, but knowing him better only increases our amazement. To everything he touched he brought precision, whether it was mathematics, mechanics, cosmology, alchemy, theology or chronology. When he devised and made a reflecting telescope, grinding and polishing the mirror himself, it was the wonder of Europe. And when in his fifties the solitary passion of scholarship burned low, and he took himself off to the Mint, he devoted to its work an intensity and technological understanding quite new to it, and invaluable for the great re-coinage which had been undertaken.

After a few years in London Newton abandoned Cambridge, making sure that his own candidate, William Whiston, succeeded him to the Lucasian chair of mathematics. It was Whiston's theology, however, not his mathematics (which was undistinguished) that won Newton's approval. Both abominated the fashionable heresy of deism which conceded God's general providence in establishing the Universe, while denying the special providence whereby he interfered from time to time with the mechanism. God has, they agreed, certainly been very sparing in his special providences, foreknowing that natural processes, obedient to his laws, will usually bring his purpose to pass. To Whiston, anticipating Velikovsky, the comets were a potent, though entirely automatic, instrument – at the Fall the Earth was struck by one so that the smooth surface and equable climate of Eden were transformed to a rugged landscape beset by storms; and in Noah's time a near miss by a comet cracked the crust and opened the fountains of the great deep. But the deists go so far as to question whether God involved himself even in that central act, the creation of Man, and his endowment with a soul; such doubts were abhorrent to Newton and Whiston, who in this matter were devoutly orthodox.

Whiston's importance lies in his acting as a megaphone for Newton's theological views. Both studied the scriptures assiduously, with total faith in the prophecies of Daniel and St John the Divine. But while Newton was, as Whiston said after his hero's death, of a "prodigiously fearful, cautious and suspicious temper", Whiston could not compromise the safety of his eternal soul by silence. This was all very well in a defender of true religion against the heretic, but soon brought about his downfall. For their painstaking search for the earliest sources led them to the firm conviction that Athanasius succeeded in carrying his trinitarian doctrine at the council of Nicaea by deliberately corrupting scripture. They did not question the duty of worshipping Christ as perfect man and saviour, but to worship him as the equal of, and coexistent with, God the Father was idolatry, almost the worst of sins. Newton could easily avoid talking about it, but Whiston's unquenchable garrulity extended to his Arianism, with the result that in a short time the staunch defender of the faith was himself convicted of heresy and expelled from the University.

J. E. Force has treated Whiston sympathetically, allowing us to see him not just as in the eyes of contemporary satirists, a figure of fun, but as, at the same time, a scrupulously honourable scholar with an irrepressible urge to proclaim important truths, and a readiness to suffer hardship uncomplainingly. His portrait on the dust-cover shows nothing beyond an urbane eighteenth-century gentleman, but there is another in Clare College whose hatchet face and jutting chin suggest a potential for fanaticism more in keeping with the story of his life. Apart from a certain repetitiousness, Force tells this story very readably, and with well-documented scholarship that manages to conceal what must at times have proved a rather tedious examination of Whiston's voluminous writings. In himself Whiston has little to offer the modern student; as a mirror of Newton's thought he makes a significant contribution to Newtonian studies.

I wish I could recommend Gale Christianson's book as warmly, for there is much excellent material in it, especially his treatment of Newton's career at the Mint, which he brings to life admirably. There are minor weaknesses – a profusion of misprints in the main text that make one doubt whether all the curiosities of spelling in the manuscript excerpts are genuine; an indifferent index; and an occasional descent into the purple of historical novelese. The disappointment I feel lies in missing any solid sense of the intellectual stature of the man. Newton's cosmology is to Descartes's (or Whiston's for that matter) as a diamond is to cottonwool. He lives for his introduction into science of an exactness of analysis and a technical power that were dreamt of, but not realized, by Galileo. Everything else he achieved, however revealing of his great gifts and influence, was ephemeral. When, therefore, a book as long as this, and devoted to Newton and his circle, fails to convey any feeling for what was involved in the invention of integral and differential calculus, or any understanding of the principles of dynamics which he discovered and developed so powerfully, there is a void at the heart of the argument. For those who lack mathematics, yet still want to know about Newton, there is almost twice as much here as in Frank Manuel's already considerable *Portrait of Isaac Newton*, covering a wider canvas and fully documented. But for the robust inquirer with a taste for mathematics, Richard Westfall's *Never at Rest* still leads the field.