# When seeing is believing

*Albert Van Helden*

**Planets and Perception: Telescopic Views and Interpretations, 1609–1909.** By William Sheehan. *University of Arizona Press: 1988. Pp. 324. $35, £23.95.*

*Planets and Perception* is a somewhat misleading title. Rather, Sheehan's book is specifically about the Martian canal episode, from its start with Schiaparelli in 1877 to its demise at the hands of Antoniadi in 1909.

By way of background, the first 50 or so pages take the reader from Galileo's first telescopic observations to the middle of the nineteenth century, with brief excursions into the debate about extraterrestrial life. The action proper starts with the favourable opposition of Mars of 1877, when Asaph Hall discovered the two Martian satellites and Giovanni Virginio Schiaparelli announced that the planet's surface was laced with *canali* (which in English became *canals* with all the implications of intelligent design). In this story, which has been told before, the main protagonists were Schiaparelli, the hydraulic engineer turned astronomer with his penchant for sharp geometric delineation; the French astronomer and popularizer Camille Flammarion (who was fired by LeVerrier, the director of the Paris Observatory, for his speculations about extraterrestrial life); the Boston Brahmin Percival Lowell, whose mind was made up before he ever made a serious study of Mars; and the remarkable Eugene Marie Antoniadi, a native of Constantinople, who made an astronomical career in France and for many years held the position of director of the Mars section at the British Astronomical Association.

Sheehan is an amateur astronomer who makes his living as a psychiatrist. The combination of interests makes him ideally suited to deal with this complicated episode in the history of astronomy. After all, this is not merely a story about the march of telescopic progress or the history of an egregious error made by inferior scientists. It is as much about apertures, diffraction and atmospheric seeing as it is about rods and cones, hard-wired biases in our perceptual apparatus, and the astronomer's professional and psychological background. Sheehan does a fine job of weaving all these strands together in a coherent and engaging account: his book is a good read.

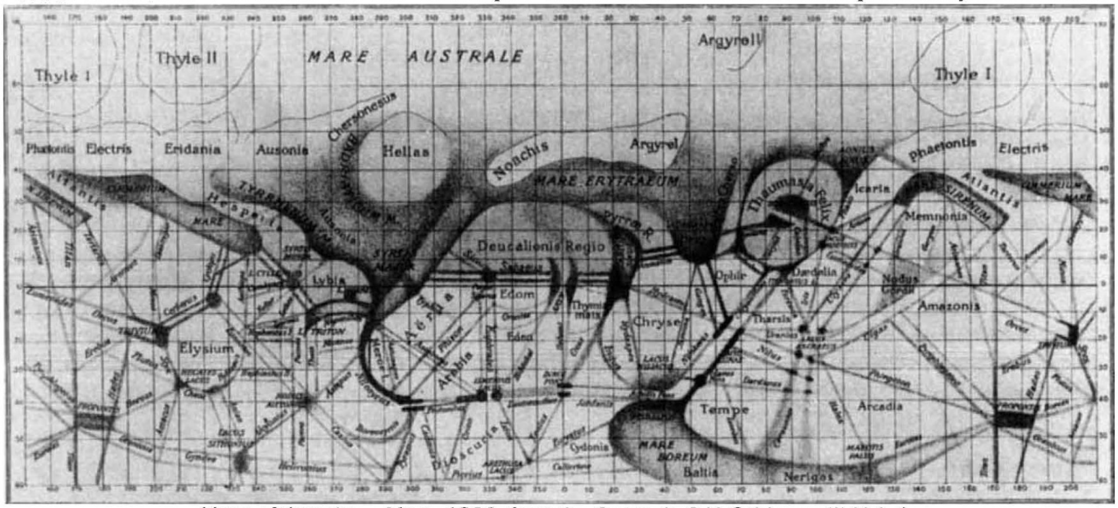
The problem of the Martian canals is not so much that the observations were wrong. A number of talented observers with impeccable credentials saw these canals, and there is little point arguing that they could not have seen them because they do not exist. *They* knew that they saw them and they agreed with each other that they really did exist. Somehow the chronicler of this episode must explain how it is that virtually an entire generation of planetary astronomers could see vague and uneven shadings on Mars's surface as canals — not as something else, but as *canals*.

The explanation must begin with an account of the telescopes used, their resolution and the quality of the atmosphere in which they were used. In the late nineteenth century there was a great debate about the efficacy of very large telescopes in planetary observations that focused the attention of astronomers on the workings of the Earth's atmosphere. Although the issue was finally settled in favour of the larger instruments, for several decades there was widespread doubt that the largest apertures (up to 40 inches) were as effective as more modest apertures, and Percival Lowell himself almost always stopped down the aperture of his 24-inch refractor to 12 to 18 inches. Obviously this debate had crucial implications for the question of the canals. Sheehan explains this aspect very well indeed.

How a series of faint and irregular markings, perhaps too small to be seen individually, can be perceived as straight lines is a more complicated exercise that takes Sheehan into the field of experimental psychology. He is able to put what actually happens at the telescope in this psychological context and constructs a good account of how canals were produced. All this is incorporated in an engaging narrative.

Finally, however, we must come to terms also with the personality of the observer. This is most obvious in the case of Percival Lowell, who founded an observatory just to prove his hunch that there was intelligent life on Mars. Lowell was an autocrat of enormous energy and great charisma. When he turned his attention to astronomy it took him less than a year to certify the canals and to launch a veritable campaign — among scientists and the public — for the acceptance of life on Mars. Here Sheehan is cautious, as befits a professional who is aware of the strengths as well as the limitations of his field. But we are given tantalizing glimpses of Lowell's relationship with his father, his bouts of neurasthenia and his relationship with his secretary. Surely in this area lies a potentially fruitful field of future research for one with Sheehan's expertise. Can we delve further into the personality of Schiaparelli, who used his revelations about Mars in a quite machiavellian way to push the Italian authorities into funding a larger telescope while often in private expressing doubt about his discoveries? Is there more to be said concerning the personality of Edward Emerson Barnard who, using the 36-inch Lick refractor, had by 1894 shown to his own satisfaction that the canals did not exist but did not press the issue? What can be found out about the flamboyant Camille Flammarion?

In the end we are left with the question of how astronomers ever got things right before the space missions to the planets. Those working at the research front, from Galileo to Antoniadi and Dollfus, always operated at the very limits of the discriminating powers of their telescopes. Sometimes, as in Galileo's case, they were right even when their colleagues could not immediately see what they were able to discern. At other times, as in the case of Schiaparelli, they were wrong even




Lines of thought — Mars, 1890, from the *Opera de G.V. Schiaparelli*, Vol. 1.

British Museum

though their colleagues verified their observations. Here is the great enigma of the history of telescopic astronomy. We need to learn what it was about Galileo, Cassini or Herschel that made them right. But before we can find the answer to that question, we need to know how Schiaparelli and many of his contemporaries could be so wrong. Sheehan's account goes a long way towards answering that question. □

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# On the move

*Martin Rudwick*

**Drifting Continents and Shifting Theories: The Modern Revolution in Geology and Scientific Change.** By H. E. LeGrand. *Cambridge University Press: 1988. Pp.313. Hbk £30, $49.50; pbk £10.95, $16.95.*

THE image of 'revolution' has been an integral part of the self-understanding of the modern Earth sciences ever since the theory that is now termed plate tectonics gained wide acceptance in the 1960s. One of the first major post-war volumes to favour crustal mobilism, the symposium on *Continental Drift* edited by Runcorn, appeared in 1962, the same year as Kuhn's *Structure of Scientific Revolutions*. The enthusiastic adoption of the kuhnian language of paradigms and revolutions by many Earth scientists in the later 1960s was no coincidence. It reflected a widespread sense of living through a period of dramatic conceptual change, which promised to transcend earlier disciplinary divisions and to make a unified 'Earth science' an attainable goal. Kuhn's model of scientific change, itself representing a dramatic break with orthodox philosophy of science, seemed tailor-made for Earth scientists. The use of such philosophical models to interpret the origin and development of plate tectonic theory is thus a tradition that stems from within the science itself.

Hallam's *Revolution in the Earth Sciences*, which was published in 1973 almost before the dust had settled, was one of the first — and one of the best — of a distinctive genre of historico-philosophical analyses by practising Earth scientists. 'Practitioner histories' have their limitations, however, because the professionalism of their treatment of the technical issues is inevitably offset by a less than professional acquaintance with the wider issues that any general model of scientific change entails. 'Participant histories', such as Menard's delightful *Ocean of Truth* (1986), are also invaluable; but like any other primary source they need to be treated as raw material for historical interpretation, rather than as unproblematical accounts of 'how it actually was'.

Homer LeGrand's new treatment of the history of 'Drift' — as he usefully terms *all* theories of crustal mobilism — is different, in that the author's professional affiliations are with the burgeoning field of science studies. Whatever his book may lack in the way of a participant's vivid recall of events, or a practitioner's tacit feeling for the science, is amply compensated by its sophisticated and up-to-date treatment of the historical, sociological and philosophical issues. As the title implies, the book is concerned with shifts in theories as well as continents.

Readers who like their history of science penny-plain are well served by a beautifully clear and concise narrative. This is grounded in a substantial bibliography; it is illustrated by reproductions of significant diagrams; and it shows an admirable mastery of the technical issues. The narrative is far less detailed than, for example, Glen's *Road to Jaramillo* (1982), but it covers a far wider field, and it is much better balanced than any comparable account. The book avoids the 'precursoritis' that turns Alfred Wegener into a neglected prophet or a retrospective hero. It also avoids the provincialism of some other histories of plate tectonics, which distort the picture by concentrating on those who constructed the theory in the form that became orthodoxy in the 1970s.

An early chapter sketches the late-nineteenth-century background of high-level theoretical debate between 'permanentists' and 'contractionists'; the contrast in scientific style between Europeans and Americans, which became so striking when Drift theories were revived in the 1960s, was clearly apparent even at this early stage. Global theorizing was an established and respectable tradition in German-language geology, and LeGrand neatly summarizes the lively debate that Wegener's *Origin of Continents and Oceans* (1915) evoked as soon as the First World War was over, not only in continental Europe but also in Britain. Even more importantly, he documents the unbroken tradition that linked those debates of the 1920s with the revival of Drift theorizing in the 1950s. The very possibility of Drift was indeed rejected vehemently by most of the leaders of the North American scientific community during this period, as it continued to be by Soviet scientists even through the 1970s. But for the Europeans, and still more for geologists in the Southern Hemisphere, various forms of Drift, progressively improved from Wegener's original formulation, remained a live option, albeit a minority position, that was continually under review.

Having established that vital continuity, LeGrand shows how the crucial new input from the physical scientists in the 1950s, namely in the technique of palaeomagnetism and the theory of polar wandering, created an increasingly favourable climate for Drift theorizing among land-based geologists and geophysicists. Contrary to subsequent myths, this dramatic revival in the fortunes of Drift theorizing took place *before* the burgeoning breed of ocean-going scientists began to apply their mass of new data to the interpretation of global tectonics. The rest, as journalists are fond of saying, is history. However, LeGrand continues his narrative with an account of the emergence of plate tectonics from pre-existing forms of Drift theory. Notwithstanding his own title, he interprets this convincingly as a story of continuous conceptual evolution rather than as a revolutionary break with the past. Finally he traces the precipitate conversion of the North American scientific community, and, to avoid too triumphalist a conclusion, notes continuing pockets of sceptical resistance elsewhere in the world.

Readers who want a tuppence-coloured version of this history get it, at no extra cost, in the form of what LeGrand terms "Voice-Overs" at the end of each chapter. These are brief commentaries on the foregoing narrative, reflecting on its compatibility — or more often, incompatibility — with the main theories of scientific change discussed among philosophers of science in the past quarter-century. The 'paradigms' of Kuhn and the 'research programmes' of Lakatos are given respectful consideration, but are rejected as incompatible with this particular example of scientific change. LeGrand has more sympathy with Laudan's model of 'research traditions', although in my opinion his narrative does not lend it much better support than the others. The 'interest model' of the Edinburgh school of science studies, with its emphasis on the constitutive role of the socio-economic context of scientific work, is effectively applied, for example to the interpretation of the rise of ocean-going Earth science in relation to the needs of the United States Navy in the era of the Cold War. But in my view, what LeGrand terms the 'internal struggle' model, now associated particularly with Latour and other French analysts of science, is easily the best supported by his narrative.

This is a book that deserves a wide readership among Earth scientists of all stripes, as well as among historians, sociologists and philosophers of science. It gives the best brief narrative there is of the origins of plate tectonic theory; and it combines that with a thought-provoking and undogmatic evaluation of the story as a notable episode of conceptual change in modern science. □

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