



## EDITORIAL

## THE NATURE OF DISCOVERY

Carin Berkowitz's account, in this issue of *Notes and Records*, of the work of Charles Bell and François Magendie on the roots of motor and sensory spinal nerves raises fundamental questions about the nature of discovery and the priority disputes that discoveries so often engender. As Berkowitz shows, it is a far from easy matter to determine whether Bell (who first wrote on the nerves in 1811) or Magendie (who began publishing on the subject in 1822) should be regarded as the true 'discoverer'. An important conclusion of her article is that a comparison between Bell's and Magendie's contributions throws less light on who was the true victor in a dispute that rumbled on for some 30 years than on the changes in anatomical science that came about during the first half of the nineteenth century. Bell's work was rooted in a tradition that emphasized the surgeon-anatomist's role as a teacher and had little time for experimental physiology and vivisection, whereas Magendie's was the product of laboratory science of a kind that is familiar to us today. That familiarity has helped Magendie to emerge in many accounts as the 'winner' of the dispute. But it has done so, as Berkowitz argues, at the expense of what might have been a more profitable focus on the evolving nature of the contexts in which Bell and Magendie worked.

The complex nature of discovery emerges no less strongly from Mark Grossman's review of John Dalton's possible debt to the atomists Bryan and William Higgins and William Austin in the fashioning of his chemical atomic theory. Grossman's approach is properly tentative. But, in pointing to similarities between Dalton's rules of chemical combination and those advanced by Bryan Higgins in particular, he raises at least the possibility that Higgins and Austin should be counted among the significant influences on Dalton's thinking. As Grossman insists, these cannot be regarded as the sole influences; in a Daltonian historiography marked by uncertainty and a paucity of manuscript resources that might have illuminated Dalton's route to his theory, we can only continue to speculate on the significance of these and other possible sources of Dalton's ideas, including for example his work in meteorology and the solubility of gases in water. However, in the light of Grossman's discussion, Bryan Higgins's role clearly deserves more serious attention than it has tended to receive in the recent literature.

Gildo Santos's article is another that bears directly on discovery, in this case of element 75 (rhenium) and possibly of element 43 (masurium, formally renamed technetium in the late 1950s) by the German husband-and-wife team of Ida and Walther Noddack in 1925. Whereas the Noddacks overcame scepticism about their claim to the discovery of rhenium rather quickly, their work on masurium was contested for many years to come. Fritz Paneth, a distinguished chemist with interests extending far into the history and philosophy of science, saw the claims of Carlo Perrier and Emilio Segrè, working at the University of Palermo a decade later, as much stronger. As Santos shows, however, the

judgement about priority with regard to masurium is far from straightforward. His discussion of why the Noddacks never received the recognition they arguably deserved for their work on masurium touches interestingly on the common (though unjustified) perception of them as Nazi sympathizers, a charge voiced in particular by Paneth and Segrè, both of them Jewish refugees. But other considerations unquestionably included Ida's sharp tongue and outspoken heterodox opinions. Her criticism of Enrico Fermi's claims to have produced element 93 in a process of nuclear fusion (induced by the bombardment of uranium with neutrons), rather than by nuclear fission, was injudicious, though as it happens well founded. Also undermining of her position in the scientific community was her advocacy (with Walther) of a new natural system of classification based on a table of isotopes, and not just of elements. Her conclusion that all the elements are present in all earthly substances, mineral and organic, was another belief that set her firmly outside the main stream. Such unorthodoxy and the difficulties she encountered as a woman in the male world of German science make it all too easy to understand why Ida remains a rather shadowy figure in the history of science. But they also invite the further examination that Santos advocates in his article.

Discovery, of course, normally entails the abandonment, or at least the qualification, of earlier theories or observations. That has often been a straightforward process leading to the marginalization of previously respected authorities. But when such an authority has the status of a national icon, marginalization becomes anything but straightforward. Imogen Clarke's discussion of the handling of Isaac Newton's reputation in the context of the emergence of quantum physics and relativity theory in the first half of the twentieth century points to a particularly delicate adjustment that went hand in hand, especially for British physicists, with talk of a revolution in their discipline. Newton was too respected, with an image of genius deeply rooted in the public as well as the scientific mind, for his work to be laid aside as 'wrong'. However, as Clarke shows, the cultural climate of the day was one that privileged the new and the modern, exemplified in modernist revolutions in art and literature and Virginia Woolf's retrospective identification of the years just before World War I as the time when 'human character changed'. Against that background, how far was physics to take the modernist turn and reject the past? The dilemma ran deep, and Clarke's examination of the writings of the popular writer J. W. N. Sullivan, as well as those of such leaders of British physics and astronomy as Lodge, Eddington, Rice, Crommelin and Jeans, leads us through the intricacy of responses that sought broadly, though with very different emphases, to maintain a measure of continuity with the past in the face of scientific change that the Australian-educated mathematician Samuel Bruce Maclaren identified with a 'spirit of revolution'. Newton, at all events, survived.

Another iconic figure whose reputation has survived, despite subsequent discoveries and reinterpretations of his contributions, is Leonardo da Vinci. Denis Noble, Dario DiFrancesco and Diego Zancani demonstrate how Leonardo's understanding of the origin of male semen changed in the light of his own observations. His early drawings, from before 1500, show him following established teaching, according to which there were two separate ducts in the penis, one of them connected to the spinal cord. But a drawing dating from after 1508 corrects the error. Its representation of male genital anatomy squares remarkably with modern understanding, a point that has passed largely unnoticed in the abundant secondary literature devoted to Leonardo. As Noble, DiFrancesco and Zancani observe, the documentary evidence that might elucidate possible influences on Leonardo's change

of heart is just not there. But the drawings display convincingly his readiness to correct his own, and contemporaries', mistakes.

The case that Noble, DiFrancesco and Zancani make rests on items, in the Royal Collection, of extreme rarity. As historians we depend on such resources in all their diversity, as we depend also on the librarians, archivists, and picture curators who care for them. Daniele Cicuzza's account of Alfred Russel Wallace's fern specimens in the Cambridge University Herbarium points tellingly to the riches that wait to be exploited, even in well-known academic collections. The survivors of the ferns that Wallace collected in Borneo, embracing 33 species, had been largely unstudied or barely studied until their recent rediscovery, and their coming to light must be accounted a major event for all those interested in Wallace's work and the history of the broader discipline of biogeography, of which he was essentially the founder. No less importantly, as Cicuzza observes, the Cambridge collection stands as a reminder of the value of historic herbaria for our understanding of evolving patterns in the distribution of species and hence for informing current studies of climate change.

I cannot end these comments, my last after seven years as editor, without thanking all those who have contributed so much to the well-being of the journal. It has been a pleasure to work with the authors who have chosen *Notes and Records* as the destination for their research and with a distinguished editorial board whose generosity and wise advice have been indispensable. Within the Royal Society, the journal's editorial administrator, Jennifer Kren, has brought to bear a friendliness and exemplary efficiency essential to the smooth running of the journal and the all-important good relations with contributors. As editor, I have also drawn heavily on the hard work and goodwill of the staff of the Royal Society Library. Under Keith Moore, the Library stands as a major institutional setting for research in the history of science; it is a jewel in the Royal Society's crown.

Finally, those whose names never appear in print. Phil Hurst has been a constantly supportive publisher whose imaginativeness and ambition for *Notes and Records*, backed by encouragement from the Society's Publications Board, have been crucial. Also crucial have been the attentions of the journal's eagle-eyed copy-editor, Bruce Goatly; in work that has gone way beyond the purely typographical, he has been a champion of accuracy and clarity of expression for whose interventions I and many authors have had reason to be grateful. And then there are the unsung heroes of all academic publishing, the referees. We have often wondered how we might properly acknowledge these essential guarantors of our scholarly standards. But, in a system that quite properly rests on double-blind refereeing, it is hard to offer them more than the satisfaction of seeing articles that they have read, at least the successful ones, finally appear in print. Small thanks for a selfless investment of time and expertise.

My successor, Ben Marsden, will have access to these many sources of support. He will make the most of them, I know, and I wish him every success in taking the journal forward.

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