knowledge of nineteenth-century conceptions of heredity as well as providing a wealth of detail on the state of sheep husbandry, breeding and the textile industry in various European countries and Australia. Historians of technology will find here a rich case study of the diffusion of a technology, and the book’s implications for the relations of science and technology are significant. For Wood and Orel do not merely treat breeding techniques as substantially independent of the science of the time; they also demonstrate that breeding experience and problems prompted the very questions which Mendel later posed. As their opening sentence declares, ‘The origin of genetics... is not only to be found in the clever writings of philosophers’ (p. vi).

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The Henry Charlton Bastian revival is in full swing. Not since the 1870s, when the London-based Bastian enjoyed reputations as a clinical neurologist, medical teacher, worm taxonomist, disease theorist and defender of the doctrine of spontaneous generation, has he been the object of so much attention. His observations of ‘low organisms’ (to use a common phrase of the day) in silica solutions are being taken up anew in the microbiological literature. Under the editorship of James Strick, Bastian’s major and minor writings on spontaneous generation are back in print, along with a generous selection of reviews and other commentary, in a six-volume set from Thoemmes Press, Evolution and the Spontaneous Generation Debate. And in Strick’s lucid and sympathetic historical monograph Sparks of Life, the story of Bastian’s rise and fall has been updated as a case study in how high-level Darwinians ended experiments. Bastian’s ‘sprawling The Beginnings of Life (1872) gives the flavour, so to speak, of those experiments (i, p. 359):

A flask containing a very strong [and very boiled] infusion of turnip was opened fifteen days after it had been hermetically sealed. ... On microscopical examination the fluid was found to contain a multitude of plastide-particles and very active Bacteria. The Bacteria were almost more active than any I had before seen, and there were many different kinds.

Contemplated from a distance, the times could hardly have been less propitious for this sort of thing. Consider that three of the most famous and enduring theories of the era – Darwin’s theory of evolution, Pasteur’s germ theory of disease and the ‘new’ cell theory of Virchow – united in rejecting de novo origins for biological items (as was pointed out as early as the 1890s). For Darwin, of course, the items were species. They do not arise whole and in isolation, he argued, but only in ‘descent with modification’ from other, pre-existing species. For Pasteur, the items were disease-causing micro-organisms. These descend from other, pre-existing ones, captured from the air in open wounds and open mouths. For Virchow, the cells making up living bodies descend from other, pre-existing cells. Across the life sciences, and internationally, the idea of parentlessness was in decline. Spontaneous generation suffered still more direct snubs. Darwin gave it no causal role whatsoever. In his view, simple species exist alongside complicated ones not, as Lamarck had argued, because monads constantly bubble into life, with more recent arrivals being responsible for the simpler species, but because the different branches on the tree of life have progressed upward according to different, contingently determined circumstances. Around the time of Darwin’s Origin of Species (1859) Pasteur too worked to undermine the doctrine. His famous swan-necked flasks were part of a campaign to show that the micro-organisms inhabiting
the boiled infusions of his opponent Pouchet had not self-organized, but instead derived from dust-borne ancestors.

Yet Bastian had supporters in British scientific and, especially, medical circles. The bare fact points to the existence of a number of interesting and easily overlooked counter-currents within the mainstream of Victorian biological theory, several of them identified in John Farley’s *The Spontaneous Generation Controversy from Descartes to Oparin* (Baltimore and London, 1977). Among the Darwinians, for instance, it was noted that natural, law-governed causes must have brought forth the first organisms as well as transformed them thereafter. Among the doctors, there was much scepticism about interpreting associations between micro-organisms and disease as showing that micro-organisms caused disease. Chemical traditions of explanation, long popular in Britain, could be called upon to supply the view put by Bastian – that micro-organisms were by-products of the chemical changes constituting disease. Among the physiologists, new emphases on the ‘protoplasmic’ content of cells as the basis of life, and on protoplasm as a simple form of matter, blurred the divide between the living and the non-living.

Strick goes further still in reconstructing the framework of ideas that had rendered spontaneous generation plausible. He argues that Bastian’s programme suffered as much from the dismantling of that framework, piece by piece, as from the numerous digs at his competence or the growing consensus that his results were due to heat-resistant bacterial spores. There was, most obviously, the displacement of chemical theories of disease by germ theories. But there were other casualties too – little-remembered doctrines asserting, for instance, the colloidal nature of living substance, the permeable boundaries of bacterial species and ‘vital force’ as interconvertible with other forms of energy. The dwindling of a native ‘histological molecules’ tradition, central since the days of Robert Brown, proved especially damaging. As late as the 1860s its stress on the activity of tiny particles in living tissues played a role in winning converts to spontaneous generation, most famously Richard Owen. But it did not long survive in the age of Virchow.

Farley is still the best guide to the experiments with which Pasteur and his London ally John Tyndall attacked Bastian. Strick largely ignores the experimental details, invoking a familiar philosophical pair – the underdetermination of theory by data and the experimenter’s regress – to suggest that experiments on their own were powerless to falsify claims for spontaneous generation. He locates that power instead in the social and institutional settings of the debate, in particular with two distinct but overlapping agendas for professional reform of the life sciences in Britain. One is Tyndall’s attempt to bring clinical medicine more firmly within the sphere of the laboratory sciences. As Strick shows, before Tyndall took up the cause of his fellow physical scientist Pasteur in the late 1860s, discussions in Britain about the nature of disease were notable mainly for their pluralism. There was no single ‘germ theory of disease’, nor were medical men uniformly opposed to germ theories, nor was hostility to such theories always linked to belief in spontaneous generation. (Michael Worboys’ *Spreading Germs* (New York, 2000) paints a similarly complex picture.) Then came Tyndall, who in 1870, from his platform as Professor of Natural Philosophy at the Royal Institution, presented an especially crude germ theory as the gift of the exact sciences to benighted medicine. Bastian and Tyndall exchanged fire in *The Times* letters pages. As Tyndall grew ever more antagonistic towards the medical profession, and towards Bastian in particular, Bastian – effectively shut out from publishing in the natural-scientific periodicals – found a hearing in the medical press and medical societies.

The result, Strick argues, was polarization and homogenization. On the medical side, there was belief in spontaneous generation, indifference to micro-organisms in disease and deference to the traditional authority of the clinician. On the non-medical side, there was scepticism about spontaneous generation, indifference to the constitution of the diseased
person and deference to the rising authority of the physiologist. The impasse was broken, Strick argues, because the furiously networking Tyndall outmanoeuvred Bastian. Tyndall's success came, moreover, at medicine's expense, since the defeated doctors then worked with a germ theory far more estranged from their clinical experience of disease than would otherwise have been the case. It is a little hard to tell how far Strick wants to defend this line of thought. His basic contention, it seems, is that the germ theorists and their opponents each had parts of the true theory of disease and, if things had worked out differently, they would have cooperated to create a medicine sensitive to germs and patients. That is an appealing prospect. But what about spontaneous generation? In that better possible world where Bastian won, would the biology textbooks proclaim that bacteria could emerge out of dissolving turnips? Why, exactly, would doctors educated to think that way have been better in dealing with AIDS (Strick's example) than our doctors? Still, the thesis flags up intriguing alliances. One of the most fascinating documents in the Thoemmes set is the record of the 'Discussion on the Germ Theory of Disease', held at the Pathological Society of London in the spring of 1875 and organized around Bastian's carefully argued case for doubt. Those who want to know what it was like for thoughtful practitioners to confront the germ theory in its early days need look no further.

The classiest prose in the set belongs to Bastian's other opponent among the Victorian elite, T. H. Huxley. Both Tyndall and Huxley were members of the X-Club, devoted to the good health of Darwinism and other forms of naturalistic scientific enquiry. Strick depicts Huxley as prosecuting an X-Club agenda complementary to Tyndall's: to ensure that the permanently disreputable idea of spontaneous generation did not compromise the fortunes of Darwinism. This interpretation, while in tune with the recent historiography of Darwinism, does not seem especially well grounded in anything Huxley actually did or wrote.

Undoubtedly, as Strick shows, Huxley's admiration for Bastian cooled as he persisted, despite criticism, with his defence of spontaneous generation, and Bastian found Huxley an annoyingly meddling editor when Bastian tried to publish his work in the Royal Society Proceedings. But the theory of evolution in 1870 was a fixture of scientific orthodoxy, in no apparent danger of being dislodged. Far from discouraging debate about evolutionary matters, Huxley was one of the chief stirrers, openly questioning natural selection and even the gradual nature of evolutionary change. Furthermore, as early as his 1862 lectures to working men on organic evolution (not discussed by Strick), Huxley insisted on a distinction that Strick rather elides, between the emergence of micro-organisms from infusions and the synthesis of such organisms from artificial protein matter. Huxley was sceptical only about the former. Indeed, his stated reason for scepticism was the same for infusion-based generation as for natural selection: no one, to his satisfaction, had demonstrated it experimentally. To Huxley, Pasteur's elegant work had put the infusion-makers out of the running. It was to the organic chemists that Huxley looked instead for the experiments that would bring living organisms into being from scratch. He regarded the infusionist Bastian not so much as a threat as a time-waster—though, to Huxley's credit, he remained open-minded enough to go along and witness some of Bastian's experiments anyway.

In an 1870 letter to Nature, reprinted in the Thoemmes set (as is his celebrated BAAS presidential address on spontaneous generation, 'Biogenesis and Abiogenesis', from that same year), Huxley referred to 'Dr. Bastian's really wonderful effluence of words'. Alfred Russel Wallace, a fan of The Beginnings of Life, complained in his (reprinted) review that Bastian was an appalling writer. Readers willing nevertheless to brave Bastian's texts really require more guidance, summary and analysis than Sparks of Life and Strick's otherwise fine Introduction to the source collection offer. Those who persevere will find some odd discrepancies between what is
in those texts and what is said about them. Compare, for instance, Bastian’s definitions of his key terms ‘archebiosis’ and ‘heterogenesis’ (Beginnings, i, pp. 251–2; ii, pp. 171–2) with the definitions Strick assigns these terms (Sparks, pp. 19, 91). According to Strick, ‘archebiosis’ was Bastianese for ‘abiogenesis’, meaning the production of living organisms from inorganic materials, while ‘heterogenesis’ applied, following standard usage, to the production of life from dead organisms or their degraded remains. It comes as a surprise, therefore – and even allowing for terminological slipperiness – to find that Bastian grouped his turnip infusion experiments under the heading of ‘archebiosis’. As near as I can make out, it is not the organic and the inorganic, but the living and the non-living, that most concerned Bastian. ‘Archebiosis’ could involve either organic or inorganic ingredients, so long as those ingredients were truly non-living. ‘Heterogenesis’ included lots of quite different processes, up and down the scale of life, where one sort of living thing gave rise to a different sort of living thing.

There is, I suspect, an altogether stranger, more unlikely project or set of projects still to be recuperated from Bastian’s writings. The Thoemmes set makes it possible for a much wider circle of historians to take up the challenge. Whether there is a great deal more to learn about the scientific reception of Bastian’s experiments on the origins of life is, on the other hand, much more dubious. In Sparks of Life Strick has amassed, and energetically contextualized, virtually everything anyone ever wrote in English about Bastian and spontaneous generation, publicly and privately. The sum does not quite amount to the book we need on the merging of the natural-historical and the laboratory-medical sciences in the mid-nineteenth century. But on its own specialist terms, Sparks of Life is a major contribution. Nor does one need always to agree with Strick’s micropolitical glosses to find, almost on every page, one’s knowledge of the period broadened, deepened and complicated. To my mind, the most remarkable discovery of all is an 1873 essay by the American college president Frederick Barnard. He made exactly the connections between evolution, spontaneous generation, the germ theory of disease and the correlation of forces that Strick prepares his readers to appreciate. Barnard went on to argue that an honest person who accepted this cluster of ideas could not believe in an immortal soul – and that was a terrible, desolating price. If, he wrote (p. 114),

the final outcome of all the boasted discoveries of modern science is to disclose to men that they are more evanescent than the shadow of the swallow’s wing upon the lake, ... give me then, I pray, no more science. Let me live on, in my simple ignorance, as my fathers lived before me, and when I shall at length be summoned to my final repose, let me still be able to fold the drapery of my couch about me, and lie down to pleasant, even though they be deceitful dreams.

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From the middle of the nineteenth century, most psychiatrists, statisticians and sociologists have agreed that the massive increase in the registered insane witnessed during that century could be attributed to an assortment of social or nosocomial factors. Victorian alienists and modern social historians have enumerated various constitutive causes, including the better registration of lunatics, the longer life expectancy of in-patients, changing diagnostic criteria and the loss of family support networks in the wide-scale disruption and emigration of the Industrial Revolution. For the last twenty years this model has been under attack. Beginning with Edward Hare’s Maudsley Lecture of 1982 some psychiatrists have insisted that the apparent increase in Victorian lunacy was a genuine one and have