on life after death, if they haven't already. You can't do better than Blum, and James' ghost—should you subsequently decide it exists—will rest easier.

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Leaps in the Dark by John Waller. Oxford University Press, 2004. 292 pp. $24.95 (hardback). ISBN 0-192804847.

Understanding the history of science may be of little if any practical use to most scientists, who are engaged in the routine puzzle-solving that Kuhn described as "normal" science; but history of science does have important lessons for those whose work is in any way unorthodox. For people who take a watching brief over science, a proper understanding of the history of science is essential. This book offers a necessary corrective for common misunderstandings about the history of science. It features detailed analysis of some fascinating cases, and should be required reading for anomalists, scientific explorers, so-called "skeptics", and other pundits of science. Anomalists in particular should note the evidence that belligerence and stridency against entrenched power are self-defeating.

The chief point expounded by Waller is that scientific reputations, both high and low, may be undeserved. Corollary points include:

- Like all history, the history of science is a complex story that becomes distorted when it is set out as a tale of heroes and villains.
- Since it is written by the victors, history may mislead in important ways.
- A proper understanding of the past requires that actions be assessed in the context of their own time.

Waller points out, correctly, that there exists no infallible "scientific method" (1). Science is always a matter of trial and error. When some counter-orthodox claim is first made, the evidence for it is rarely conclusive, and the opposition to it is not entirely misguided; "Those deemed right in the long run . . . have rarely had a monopoly over reason and good sense" (p. 3).

This book makes few demands on special background knowledge, apart from occasional unelaborated references to "the Leaning Tower of Pisa experiment" or the "tall poppy syndrome" (the latter is a common phrase in Australia). This reviewer's chief reservation is that Waller—far from alone among pundits of science—implicitly equates scientific theories with scientific knowledge, which can be subtly misleading in various ways at various points (for example, pp. 239, 273, 274). Theories are *always* applicable only temporarily, and they are never "true". Waller is also more sanguine than I am about self-correction in science: "a credible scientific theory will usually gain acceptance once a decent

empirical case has been made" (p. 6). One part of the problem is that new theories do not enter a vacuum, and even well-supported and credible theories have to overcome the inertia of prevailing views.

A smaller reservation is that Waller concentrates on correcting the roles of prominent people in science without remarking that history of science should expound primarily the scientific issues (see, for example, pp. 166–7); though he does point out that science can hardly ever be carved into neat segments attributable to particular individuals (p. 238). An unfortunate factual error is sending Waksman to Oslo rather than Stockholm for his Nobel Prize (pp. 259, 261).

Individuals whose low reputations are unwarranted include Joseph Glanvill, Lazzaro Spallanzani, and Max von Pettenkofer; "all three erred largely because of the extreme difficulties involved in studying the natural world", not because of personal failings (p. 10).

Glanvill has been painted retrospectively as a superstitious holdover in an age of dawning science, because he maintained, in the heyday of the Scientific Revolution and the blossoming of enlightened thought, that witches exist. Waller shows, to the contrary, that Glanvill was empirically oriented and that his approach to investigation was entirely consistent with that of his colleagues in the Royal Society, people like Robert Boyle and Robert Hooke. In those days it was by no means an indication of superstition to attempt to decide as to witches as Glanvill tried to do, on the basis of data, albeit the available data were anecdotes and testimonies. Waller reminds readers that Leibniz could scoff at Newton's ideas about gravity because of the requirement for some magical force acting at a distance, implying continuous divine intervention in contrast to the non-superstitious Cartesian view of a clockwork universe (p. 54).

Spallanzani believed in preformation or ovism, that eggs contain infinitesimally small but fully formed precursors of living beings. Again, by discussing in detail the evidence then available and the observations and experiments then being carried out, Waller shows that Spallanzani's views were quite logical and empirically based. The opposing view, that eggs needed to be fertilized by semen, was by no means obviously better at explaining the data; moreover it encountered problems for which no good answers were then evident. We have not necessarily progressed as to such fundamental issues. One argument for ovism was that it was inconceivable that a complex being could form itself from matter out of chaos (p. 57), not very different from Behe's "irreducible complexity" or the claims made by Intelligent Designers.

Pettenkofer had been unimpressed by Robert Koch's discovery of cholera germs. He asked for a sample, drank it, and suffered no more than a slight bout of diarrhea. Instead of being remembered as one of the heroes of medical science who used himself as guinea pig, however, Pettenkofer is typically presented as hindering the progress of medical science by opposing the germ theory of disease. Waller points out that several of Pettenkofer's students also drank the

cholera infusion without serious effect, so there was strong empirical evidence against this extract as the cause of epidemics in which many had died. In retrospect, one presumes that what Koch had sent "must have been" a weakened strain of cholera, but we presume it only because we believe the germ theory and have come to know about attenuation and weakened strains. Pettenkofer had a workable theory of moist soils as incubators of epidemics, a view by no means unique to him, and able to accommodate more empirical data than the germ theory could, in *those* days.

Waller is also able to describe how Robert Koch benefited from contemporary political and social circumstances. But as to substantive science, the convergence of bacteriology, immunology, and epidemiology in the modern science of public health "is as much the legacy of Max von Pettenkofer as it is of Robert Koch" (p. 81).

I am one of some unknown number of teachers who have told students about Newton's conclusive demonstration that white light is composed of the rainbow of colors. He split the light into a spectrum; showed that once split, it could not be further split; and that the rainbow could be recombined into white light. What could be more definitive?

It turns out that Newton had not found it easy to repeat his experiments; that prisms and lenses were often defective in a number of ways; that others had often been unable to duplicate Newton's results; that some of the prismatic deficiencies led to being able occasionally to "split further" the rays of a "single color". Newton told his opponents that they would know when their apparatus was working properly when they got the same results as he did, a circular and unjustifiable argument (p. 107). On the other hand, what Newton's opponents could not explain was why he was ever able to obtain his "decisive" results at all, if his theory was not correct. But the chief point, again, is that at the time, the plain evidence alone was not decisive. Newton's victory was owing in good part to his status and role in the Royal Society, which enabled him to impose what I have dubbed a "knowledge monopoly" (2).

The supposed demonstration by James Lind that citrus fruit is the cure for scurvy also turns out to be less simple than most stories would have it. Illnesses were generally believed to have multifactorial causes, and single remedies—"specifics"—were distrusted or even seen as quackery (pp. 124–5). Lind himself thought scurvy could have a variety of causes and cures; hardly illogical, since many people who rarely ate fresh fruit or vegetables remained free of scurvy. Neither Lind nor anyone else could possibly have got it all right a century or two before knowledge had accumulated about vitamins and the range of foods in which vitamin C occurs in useful amounts.

The martyrdom of Semmelweis, whose advocacy of hygiene among health-care workers saved many women from dying in childbirth, offers several lessons. His frustrated impatience with a stick-in-the-mud professional superior proved his downfall because Semmelweis was also on the wrong side politically and ethnically: Semmelweis was a social activist, a prominent supporter of the

1848 revolution, while his boss was a loyalist and petty bureaucrat. One should be reminded that a century later, Linus Pauling incurred professional difficulties because of his activism against nuclear testing, being denied a passport by the State Department and finding himself unwelcome at CalTech. But Pauling had never offended his detractors personally, whereas Semmelweis called his opponents murderers and assassins (pp. 151, 155). Once more, the limited state of knowledge at the time meant that Semmelweis's experience of a drastically lowered death rate *in his* own *ward* following the use of antiseptics was not decisive, because it was not replicated in every other ward that tried it, and too little was then known of the vagaries of the microbes concerned that one could explain seasonal variations, for example, or the different experiences of different hospitals.

Johann Weyer courageously confronted those who were hunting witches by inquisitional torturing, urging that the accused be treated as perhaps deluded rather than deliberately evil. But, being a man of his times, he did not deny the possibility of Satanic possession and the like. According to Waller, that history has come to see Weyer as a modern-type proponent of psychiatric treatment of people deluded into thinking they have been possessed is owing to the fact that this view served the purpose of the medical profession: the iconic Weyer illustrates that witches (and the like) are in the purview of psychiatry and not of religion, that the Church and its officers should keep their hands off this aspect of human activity. If Waller is sound on this point, then there would seem to be an analogy with modem-day "skeptics", whose crusades against superstition often spill over into activist atheism.

The case of Philippe Pinel, too, is seen by Waller as becoming iconic because it served the interests of the psychiatric profession. Pinel was indeed a humane individual who helped in abolishing the practice of chaining people in asylums, but stories of his actually ordering the first breaking of chains turns out to be an urban legend. Waller points out, too, that the sometime success of medical treatments of the time was likely coincidental—blistering, purging, etc., are not nowadays regarded as useful against mental illness. "In the absence of controlled clinical trials, it was easy to interpret these recoveries as due to the psychiatrist's efforts and failures as indicating that the case was too serious or too deeply ingrained to respond to treatment". Are things so different today? "With their professional existence at stake, . . . necessity became the mother of delusion and circularity the lifebuoy of the desperate" (p. 212). Or, as Bernard Shaw remarked, all professions are a conspiracy against the laity.

The concluding characters in this book are Robert Watson-Watt, who deliberately campaigned to make himself seen, without adequate justification, as the father of radar; and Selman Waksman, who progressively played down the contributions of Albert Schatz, the co-worker whom he had originally acknowledged as a full-fledged partner in discovering streptomycin, the cure

for tuberculosis, which remained the leading cause of death in the United States as late as 1937 (p. 243).

The last chapter of the book urges a sensibly balanced approach to history of science in which social context is given its rightful but not excessive due. Then for each chapter there are some suggestions for substantial further reading.

Difficult as it may be to remain dispassionate when treated unfairly, we serve ourselves best by sticking to the high ground of substantive discussion and leave the mudslinging and the denigration of opponents to the other side and their fellow-traveling "skeptics"—vide the sad cases of Semmelweis and Albert Schatz. That is just one of the insights offered by this interesting, instructive, thought-provoking book.

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Quantum Enigma: Physics Encounters Consciousness by Bruce Rosenblum and Fred Kuttner. Oxford, England, Oxford University Press, 2006. 211 pp. $29.95 (hardcover). ISBN 0-19-517559-X.

One of the most instructive books that I have ever read (and also simply one of the best reads, as well) is Arthur Koestler's *The Sleepwalkers*, a marvelous history of the Copernican Revolution. Koestler argues (and convincingly) that Copernicus, Brahe, Kepler, and Galileo did what they did without any real understanding of what it was that they were doing. Scientists today could easily be (and, I think, largely are) in precisely the same boat.

In that most excellent book, Koestler also makes the point that Copernicus and Galileo were extremely reluctant to have it publicly known that they held unorthodox scientific beliefs, and he emphasizes that it was fear of ridicule, not fear of the Church, that constricted them both. Copernicus only published his great book on his deathbed for that precise reason, and Galileo lied to his students for decades before "coming out" with his Copernican beliefs.