toward agnosticism. Yet toward the end of his life he confessed that his thoughts about religion were a muddle.

Despite (or perhaps because of) this persistent indecision, Darwin inched his way toward the triumph of his mature evolutionary insights, and Bowler's account of the process is superb. At this point, however, Bowler's synthesis begins to harden: he views the first edition of the *Origin of Species* as the pinnacle of Darwin's achievement and most of Darwin's subsequent modifications of its message as unfortunate regressions from the ideal. He does not adequately recognize the extent to which the *Origin* was a stopgap publication (Darwin thought of it as an abstract) intended to lead up to other things. Among those other things were a theory of pangenesis specifically designed to justify Darwin's belief in use-inheritance and a theory of human emotional expression based almost entirely on that belief. Bowler himself repudiates the old idea that Darwin's compromises with use-inheritance in the fifth and sixth editions of the *Origin* were designed to accommodate criticism from Kelvin and Jenkin; yet he fails to acknowledge the extent to which those compromises simply brought the *Origin* closer into line with positions Darwin was developing in other important books. The great virtue of Bowler's account is his recognition that Darwinism was not simply a matter of belief in natural selection as such, but above all a belief in the contingent, open-ended character of the evolutionary process. Darwin may have been indecisive on the relative importance of selection and use-inheritance, but he was absolutely firm in excluding all forms of directed or orthogenetic evolution. Bowler observes that field naturalists who shared Darwin's interests in environmental adaptations tended to be the most loyal Darwinists whereas investigators like Ernst Haeckel and Thomas Huxley who had a strong interest in morphology tended toward a kind of pseudo-Darwinism that neglected selection and evolutionary contingency. He somewhat exaggerates the differences between these morphologists and Darwin—both Haeckel and Huxley did take evolutionary contingency seriously—but the point is still a sound one.

Bowler is alert to the social and intellectual context of Darwin's work, and he frankly observes that Darwin was, yes, a social Darwinist. Darwin's very real aspirations for a kinder, gentler world were counterbalanced by a strong pessimism about the biological capacities of the human species. As Bowlby points out, this pessimism extended even to his children, who he feared had inherited his own delicate constitution. Unfortunately, this observation is one of the few in either book that helps tie Darwin's inner personal life to his public and scientific concerns. That biography is still unwritten, but Bowlby and Bowler offer us plenty to think about in the meantime.

WILLIAM MONTGOMERY
North Adams State College, North Adams, MA 01247

Tales Retold

Narratives of Human Evolution. MISIA LANDAU. Yale University Press, New Haven, CT, 1991. xvi, 202 pp., illus. $22.50.

In *Narratives of Human Evolution*, Misia Landau summarizes the views on human evolution of T. H. Huxley, Ernst Haeckel, and Charles Darwin in the 19th century and of Arthur Keith and Grafton Elliot Smith in the early 20th century, concluding with very brief descriptions of the more recent conclusions of Raymond Dart, J. T. Robinson, Philip Tobias, Donald Johanson, and Tim White. One of her messages is that phylogenetic reconstructions of fossil and recent hominids provided by each of these workers were strongly influenced by their beliefs about the mechanisms producing this change. None too surprisingly, Darwin thought that natural selection was the primary motor of evolutionary change in all species, including our own.

Although Huxley is thought of as the paradigm Darwinian, he disagreed with Darwin about the fundamental character of the evolutionary process and contributed very little of substance to the development of evolutionary theory itself. In his *Man's Place in Nature* (1863) he was primarily concerned to argue that the human species did evolve from ancestral primates, not to consider the mechanisms involved. Nor does Haeckel make much use of natural selection in his *History of Creation* (1868). He gives much more space to his own principle of recapitulation. The non-Darwinian tradition was continued by Keith and Elliot Smith. Although both men opposed neo-Lamarckism, their orthogenetic views departed just as markedly from Darwin. Not until the Modern Synthesis in the 1940s did a more genuinely Darwinian version of the evolutionary process actually influence paleoanthropologists.

The explicit points of contention among these paleoanthropologists concerned primarily which species were ancestral to us and the order in which key adaptations evolved. Although both Keith and Elliot Smith accepted Piltdown man as genuine, they interpreted this skull and jaw differently, and neither thought that this extinct species was among our ancestors. Keith opted for bipedalism as leading the way in human evolution, whereas Elliot Smith opted for the brain. As Landau remarks, Elliot Smith emphasized the brain so much in his work that one would hardly think that our ancestors even had bodies.

Among the more recent paleoanthropologists whom Landau discusses, debates continue over which fossils represent genuinely distinct species and, of these, which are direct ancestors to present-day humans. Although work in paleoanthropology after the Modern Synthesis is supposed to be influenced by a more Darwinian conception of the evolutionary process, the disagreements that Landau chronicles are decidedly non-Darwinian. They concern such issues as whether or not the morphological gap between *Australopithecus africanus* and *Homo erectus* is bigger than the gap between either of these two putative species and some other putative species, such as *Homo habilis*. Reconstructing phylogeny on the Darwinian paradigm involves much more than questions of morphological gaps.

Landau's discussions of controversies in paleoanthropology such as the preceding are both informed and lucid. However, she is not primarily concerned to chronicle the history of the field or even to indicate the effect that changing views about the evolutionary process had on phylogenetic reconstruction. Instead, she wants to warn those scientists who are engaged in reconstructing evolutionary history that they have been duped. They think that their historical narratives are influenced primarily by the interplay between theory and data that characterizes the rest of science. Instead, she argues, these paleoanthropological narratives "approximate the structure of a hero folk tale, along the lines proposed by Vladimir Propp in his classic *Morphology of the Folktale* (1928)" (p. x). Her working assumption is that "theories of human evolution [phylogeny] are determined by an a priori set of functions rather than an available set of fossils" (p. 14). By showing paleoanthropologists that they have been constrained by the rules of art, not science, Landau hopes to free them from this unnoticed bias. She wants to encourage her fellow workers to wrestle with the "story-telling dragon" instead of ignoring it.

The stages of the archetypical hero tale begin with the hero leading a relatively safe and untroubled life. After the hero is introduced, a change in circumstance occurs that leads the hero to depart on a journey during which he is sorely tested. Somewhere along the line, a donor appears who helps transform the hero. The hero is then tested again

and triumphs. In some hero folk tales, a final stage is added in which the hero succumbs to hubris and is destroyed. In paleoanthropology the heroes are particular primate species. The tests depend crucially on the views that the paleoanthropologist has of the mechanisms of evolution. In traditional folk tales, the donor appears in an animal or human form, but in evolutionary narratives the hidden agents are evolutionary principles or forces, such as orthogenesis and natural selection.

Landau uses this framework to lay out the views of Darwin, Keith, and Elliot Smith but inexplicably not Huxley and Haeckel. For more recent workers, she introduces a variation on the hero story—the mysterious birth. According to this literary archetype, the hero is born in obscurity or is cast out into the world soon after his birth without knowing his true identity. His primary test is to discover who he actually is. Landau sees the search for missing links as clearly exemplifying the archetype of the mysterious birth. However, she scarcely alludes to this narrative form in her ensuing discussions of recent battles over human ancestry.

For my part, I do not find Landau's use of these narrative archetypes all that illuminating. If anything, they intrude upon her own narratives. But my primary objection to Landau's main thesis is that archetypical stories are too malleable. With enough ingenuity, any sequence can be made to fit any archetype. When I was a very young man in the army, I went with some of my buddies to see *Turandot* at the Wiener Staatoper. (We were not your typical GIs.) While we waited for the performance to begin, I recounted the story of Orpheus descending depicted on the fire screen. A group of American tourists sitting in front of us overheard my story but thought I was describing the opera. One of them turned to thank me, but before I could explain the misunderstanding the opera began. For the next three hours, they followed Turandot's descent into the underworld to reclaim her beloved Eurydice. My brief description had really helped them understand the opera.

DAVID L. HULL
Department of Philosophy, Northwestern University, Evanston, IL 60208

The Events Stemming from Utah

Too Hot To Handle. The Race for Cold Fusion. FRANK CLOSE. Princeton University Press, Princeton, NJ, 1991. x, 376 pp., illus. $24.95.

Several weeks ahead of its publication the contents of *Too Hot To Handle* were spilled out on the front page of the *New York Times*. Rare as such an event may be for a book about science, it should come as no surprise given the subject of this one. Indeed, this latest media scoop gives support to Frank Close's thesis: *nothing* about cold fusion has been ordinary or typical or normal. It is weird science from start to finish.

The *Times*'s writer, William J. Broad, drew on Close's informed analysis to conclude that Stanley Pons and Martin Fleischmann's claim of sustained room-temperature, energy-producing nuclear fusion had lost its last bit of credibility in the wake of revelations about invented data and breaches of ethics. Broad focuses on one telling episode from the book: a masterful investigation by Close of the mystery of the "mobile peak." If nuclear fusion is going on inside those electrolytic cells, then according to conventional physical theory one should observe a pronounced peak at energy levels corresponding to gamma rays produced by neutron capture. In their "preliminary note," Pons and Fleischmann reported observations of a spike at 2.5 MeV as evidence for nuclear fusion. After Fleischmann was told that the peak was at the wrong energy level for nuclear fusion, the two chemists without explanation moved the peak so that it was centered at the far more interesting 2.2 MeV. The intriguing possibilities that lie behind these twin peaks—incompetence, error, bad judgment, ethical breaches, fraud—may never be untangled.

The *Times*'s story does not comment on Close's rationale for writing the book and how that shapes his interpretation of the events. This question is important in assessing a book that assumes the role of setting the record straight. *Too Hot To Handle* seeks to close the book on cold fusion not only as a claim about heat, neutrons, tritium, gamma rays, and helium but also as an object lesson in how good science really works.

Close builds not one but three frames to structure his story, which is the most richly detailed and best-documented account of cold fusion we have to date. The book is at once an accurate chronology of what happened and when, an up-close look at the individuals and research groups who did most to shape the events, and a primer on nuclear physics and electrochemistry. As theoretical physicist and science popularizer, Close shuttled between posts at Oak Ridge in Tennessee and Rutherford Appleton Laboratory in Britain, collecting a hundred or so interviews with fusioneers and their foes: this first-handedness gives his tale both color and authority. Quite possibly the most bizarre development in Close's chronology of the roller-coaster reality of cold fusion was the near simultaneity of Pons and Fleischmann's visit to Capitol Hill in search of $4.5 million and, up the road in Baltimore at a gathering of the American Physical Society, the first public denunciation of the claim in a scientific venue—one that raised the possibility not only of incompetent but of unethical behavior. Close gets us behind the scenes: Steve Koonin, Nate Lewis, and their colleagues at Caltech had found no signatures of fusion, but they still suspected that perhaps "Fleischmann and Pons were really just holding some secret back and we all were not as smart as we thought we were!" (p. 205). But with announcement of a congressional cold fusion hearing, the Caltech researchers felt that they had to go public in order to save taxpayers from more government waste. Ironically, they were able to get on the APS program at the last minute only because Fleischmann and Pons had declined the invitation to attend.

Close's reports of far-flung attempts to confirm or deny cold fusion make fascinating reading: the poignant tale of two University of Washington graduate students who used a novel hollow palladium tube—and got tritium!—only to have their claim overturned by a spectrometer with higher resolution; the disturbing tale from India, where cold fusion was looked on not only as an energy panacea but as a source of neutrons for uranium enrichment with obvious implications for weaponry. Unfortunately, when Close takes time to develop these local stories, it is easy to lose one's place in the overall sequence of events. The primer on physics and chemistry suffers even more from a bothersome redundancy. Readers with modest science backgrounds will not be confused by Close's explanations of these matters, which are clear and to the point. But they will certainly lose patience at the number of times we are told the same natural facts.

How does Close *interpret* the goings-on? He ignores the sociology of science and has no theory to order the significance of observed events. He ignores as well the history of science and grounds his assertion that cold fusion is atypical not on a review of like