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Paper £2.50 approx Publication May

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Advisors (Freeman: San Francisco, 1976). Further on, however, she has included a selective quote from him but not his conclusion that the advice of the Committee was "sound and right". She also writes: "One may ask how the GAC members, strongly opinionated scientists, could have so changed their vote on the hydrogen bomb in a couple of weeks". Reference is then made to a meeting on 29 October and then to a "second meeting". Actually, there was no change of vote, and the written views, unanimously against an all-out effort, are dated 30 October. If entertainment is the objective of Libby's book, perhaps these details are unimportant, but I view such distortions as poisonous. The student and the serious reader must take something like York's book as an antidote; then Libby's continued loyalty to "one of my heroes", Edward Teller, can be set in contrast to York's development from his early career as the first director (1952-57) of Teller's

Lawrence Livermore Laboratory to his subsequent activity in arms control and disarmament.

The Uranium People is interesting in the same sense that movie magazine stories about the stars are interesting. However, events and people, now for the most part passed into history but leaving a legacy which still affects our world, have a different kind of interest and deserve a different kind of treatment. □

J.H. Manley became active in nuclear physics at Columbia as a faculty member with I.I. Rabi, John Dunning and others in 1934, and joined the Metallurgical Laboratory at Chicago under A.H. Compton in January 1942 while on leave from Illinois. He assisted J.R. Oppenheimer in planning and establishing the Los Alamos Scientific Laboratory, to which he moved for what turned out to be a total of 23 years before becoming a consultant — his present status. He was Executive Secretary of the General Advisory Committee (USAE) during the first four years of Oppenheimer's chairmanship.

Descent is what matters

David L. Hull

The Darwinian Revolution: Science Red in Tooth and Claw. By Michael Ruse. Pp.320. (University of Chicago Press: Chicago and London, UK, 1979.) £12.

MOVIE reviewers like so few movies because they see too many of them. For the same reason, academics write very few favourable reviews of books, especially if the book is in their own area and is directed at a general audience. It is difficult to believe that yet another book on Darwin and the Darwinian revolution could add anything new or contain any surprises. Ruse's book is an exception on all counts. Darwin scholars and the general reader alike can learn from it.

In the first half Ruse describes the state of science before the *Origin*. Historians argue that scientific views must be set out in their own terms, not biased towards the use which later scientists will make of them. That is easier said than done. Ruse cannot discuss every topic of interest to scientists in the first half of the nineteenth century. Understandably he limits himself to those areas of science which eventually contributed to Darwin's theory of evolution — Charles Lyell's uniformitarian geology, Richard Owen's archetype theory, Karl Ernst von Baer's embryology, and so on. In certain cases, Darwin incorporated these views into his own. Just as often his ideas were shaped by

reacting against the views of his predecessors. But, in any case, once Ruse has narrowed his focus, he discusses the science in its own right, not biased towards the use which Darwin eventually made of it. Although Ruse teases us a bit along the way, he does not get around to Darwin and his theory until the middle of the book. All the attention which Ruse pays to the scientific work which preceded Darwin, instead of detracting from the magnitude of Darwin's own contributions, only enhances it.

Another strength of Ruse's exposition is his concern with the impact that social factors had on both the formulation and the reception of Darwin's theory. However, the 'society' of greatest interest to Ruse is not Victorian society at large but the society of scientists. Darwin was caught up in the groundswell as science in Great Britain became self-consciously a profession. Young scientists today can only envy the ease with which budding young scientists in Darwin's day were able to step into the innermost circles of science. To begin with, relatively prosperous Victorians were a tightly-knit group. For example, the first three directors of Kew Gardens were William Hooker, his son J.D. Hooker, and William Thistlethorn-Dyer, the younger Hooker's son-in-law. J.S. Henslow was the first scientist to have significant influence on Darwin, and J.D. Hooker was his first convert to evolutionism. Henslow married the sister of Leonard Jenyns, the man who had been offered the position of *de facto* naturalist on the *Beagle* before Darwin. Their daughter in turn married J.D. Hooker. Upon her death, Hooker married the widow of William Jardine, a famous ornithologist. Darwin married a Wedgwood,

and so it goes. Of equal importance, so few young men were inclined or financially able to contemplate a life of science that the ones who did were welcomed enthusiastically.

Darwin began his career as a Lyellian geologist. In retrospect we tend to think that Lyellian uniformitarian geology was more successful than it actually was because the first histories of geology were written by uniformitarians. To the victors goes the history. But in fact Darwin was one of the few young geologists at the time who accepted Lyell's views. Darwin's early work on the formation of coral reefs (which turned out to be basically correct) and the parallel roads of Glen Roy (an embarrassing blunder) was in the Lyellian mould. Gradually, however, Darwin became his own scientist, departing in several important respects from Lyell. By 1859 not only was Darwin a professional scientist with a well established reputation, but also he had "built around himself a new scientific community from which his evolutionism could be launched" (page 185).

One of the great puzzles in science is why certain scientists succeed in converting their fellow scientists when all their precursors failed. For example, Lamarck's theory was greeted with derision, Robert Chambers's *Vestiges of the Natural History of Creation* was condemned by everyone who was anyone, including T.H. Huxley, and Herbert Spencer's musings about the transformation of homogeneity into heterogeneity produced little more than embarrassed silence. Ruse gives many reasons for Darwin's success — such general considerations as the changes wrought by the industrial revolution and the rise of naturalistic metaphysics to such specific considerations as the way in which Lyell posed the species problem in his *Principles of Geology* — but of prime importance was the professional manner in which Darwin conducted himself and presented his case. Lamarck had destroyed his credibility by tossing out theory after theory in areas in which he knew nothing; Chambers was the Velikovsky of his day; and Spencer was a "philosopher". But Darwin was a scientist, writing as a scientist, for scientists. Nineteenth-century views on the nature of science had as much to do with the reception of Darwin's theory as religion and the fossil record.

The most entertaining part of Ruse's exposition is the attention he pays to the interpersonal relations between the scientists of the period. Although Owen initially helped the young Huxley in his career, he soon came to be Huxley's chief enemy. Because the Darwinians won, everyone knows what an unpleasant man Owen was, but Huxley too had a mean streak. After Owen tried to block the publication of one of Huxley's papers, Huxley used his review of the *Vestiges* to attack him. As Ruse sees it, the animosity between Huxley and Owen was as much

responsible for Owen coming out against Darwin as the content of Darwin's theory. "At the scientific level Owen was determined to oppose Huxley, and if Huxley was to support Darwinism then so be it" (page 144). Ruse also mentions, possibly to neutralize anti-Owen propaganda, that "at least until 1853 — when Huxley started attacking Owen and when Darwin and Huxley became close — there was warm feeling between Darwin and Owen" (page 144). The source of this claim is the following remark which Darwin made in a letter to Huxley in 1854 about Huxley's review of the *Vestiges*:

By Heavens, how the blood must have gushed into the capillaries when a certain great man (whom with all his faults I cannot help liking) read it!

This is warmth of sorts.

One of the chief messages of evolutionary theory as a biological theory is that descent is what matters, not abstract similarity. Ruse applies this same principle to the history of science. No scientist has suffered more from the effects of the antiquarian disease known as "precursoritis" than has Darwin. Endless energy has been expended to uncover precursor after precursor — authors who happened to mention in passing one or more views similar to those later enunciated by Darwin. More recent historians have complained that often the putative similarity is spurious. But the important point is that ineffectual precursors do not count. As Ruse argues, "it would be a mistake to think that the most significant intellectual continuity in the Darwinian Revolution existed among those one would label 'evolutionist'" (page 199). The real precursors to Darwin were not Lamarck, Erasmus Darwin, William Wells, Patrick Matthew and Edward Blyth. Darwin was totally unaware of the work of several of these men, and the others had little effect on him or anyone else. Darwin's real precursors were Lyell, Henslow, Owen, Herschel, Whewell, von Baer and Sedgwick. These were the men who contributed to the Darwinian revolution. What counts in the history of science is continuity of effect, not similarity of ideas. Likewise, as Ruse remarks, a "Darwinian" is "someone who identified with Darwin but not necessarily someone who accepted all of Darwin's ideas" (page 203).

During the past decade, Darwin scholars have increased our understanding of Darwin and the Darwinian revolution immensely. Ruse gives his readers the benefit of that scholarship without making them suffer under the burden of the tedious pedantry which usually accompanies it. □

David L. Hull is Professor of Philosophy at the University of Wisconsin-Milwaukee, Milwaukee, Wisconsin.

Full of hope and promise

R. V. Short

The Politics of Contraception. By Carl Djerassi. Pp.274. (W.W. Norton: London, 1980.) £6.50.

CARL DJERASSI, one of the world's outstanding organic chemists, has written a most important book about contraception that makes compelling reading. It deserves the widest possible audience, not only throughout the scientific community, but far beyond, in the realms of policy-makers, politicians, journalists, administrators, funding agencies, and indeed amongst all who are concerned about world futures. There are few indeed who have mastered the art of popularizing important issues without seeming to trivialize them; this book stands out as a rare example of scientific reporting at its very best. Djerassi's writing reflects the man himself — forthright, scholarly, direct, uncompromising, and with an outstanding ability to single out important issues for critical comment. He shares with Samuel Pepys "a fine conceit" that adds to, rather than detracts from the narrative, because one respects the integrity and intellect of the man behind the pen.

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Carl Djerassi

Djerassi tells us that he has aimed to produce "not a scholarly treatise, but the distillate of the conclusions and opinions of a person who for over 20 years has lived a bigamous professional life in serving simultaneously as a professor carrying out basic research and as an industrialist who has had to concern himself with finding worldwide applications for laboratory discoveries". Not only has he been able to combine the viewpoints of a professor of organic chemistry with those of a director of a pharmaceutical company, but he has added profound biological and social insights; it is to this amalgam of talents that we must attribute his success as an