regimes do not select for neoteny, but they permit the prolonged period of growth that neoteny requires. Neotenic pedomorphosis is selected for only if juvenilized morphology is adaptively superior; progenetic pedomorphosis is selected for just because it allows faster reproduction, even when juvenilized adult morphology is something of a handicap. This sketch of Gould's ideas does not do justice to their subtlety, or to the care and honesty with which he documents them and assesses possible counterexamples.

The conclusion of the book, in which Gould tries to revive Bolk's theory that people are neotenic apes, is less convincing. Somatic growth in *Homo*, Gould notes, is both absolutely and relatively retarded compared to that of apes, and we retain into adulthood the short faces, bulging braincases, hairless skins, and slender erect necks of fetal apes. Gould accounts for all this by showing that fetal rates of brain growth, facial elongation, and so on continue far longer after birth in *Homo* than in other anthropoids. (In documenting evolutionary changes in hominid face-braincase proportions, Gould relabels Hemmer's cranial-length measurements as measurements of facial length, analyzes them on that basis—and goes on to criticize Hemmer's statistics!) I'm not convinced that all this adds anything to our understanding; how, after all, *could* the adult brain be enlarged, or the face shortened, except by prolongation of the rapid fetal growth of the brain or the slow juvenile rate of facial elongation? It is not clear that postulating "pedomorphosis" or "fetalization" results in a more economical description of human peculiarities, especially since so many of them cannot be described in those terms.

Gould acknowledges that some of man's distinctive traits cannot be explained by invoking neoteny, but argues that most of the standard counterexamples to Bolk's theory can be analyzed as effects of retarded somatic development. Some of his arguments to this effect smack of special pleading, especially when he tries to link upright posture to neoteny. It seems to me that if all the supposedly neotenic features Gould mentions were in fact produced by alterations in the basic mechanisms regulating growth they ought to covary throughout human evolution. But they don't; australopithecines with apparently perfected upright posture retain endocranial volumes in the chimpanzee range, and the australopithecines that have the flattest faces do not have relatively the largest braincases. Such facts suggest that some of the various "neotenic" features of *Homo* are under independent genetic control and have been differentially acted on by natural selection.

Ontogeny and Phylogeny is an important and thoughtful book which will be a valuable source of ideas and controversies for anyone interested in evolutionary or developmental biology. It is bound to promote fertile interactions between the two fields. But there are some deep flaws in it, mostly reflecting the incoherence of Gould's philosophy of science. If, as Gould claims, it is a historical fact that "natural history does not refute its theories by cataloguing empirical exceptions to them," then to insist that a theory must yield falsifiable expectations is a mere formal requirement, as pointless as demanding that a hypothesis be written in couplets. Yet Gould criticizes Lombroso and others for making their doctrines "invincible to disproof" and takes great pains to evaluate (and in most cases reject) possible counterexamples to his own theories. But in his view, counterexamples ought to be simply irrelevant. Since neither Gould's ideas nor Lombroso's are untenable in theory, they should be rejected only by changes in higher-level theory or the caprices of intellectual fashion.

Gould, I think, has failed to notice that logical implication is a two-way street. If he is correct in asserting that Haeckel's biogenetic law is "inconsistent with the precepts of Mendelian genetics," then he must err in asserting that the law "could engender no refutation because it included all phenomena." If Mendel implies not-Haeckel, then Haeckel implies not-Mendel, and Haeckel's doctrines must rule out the range of phenomena that invalidate certain non-Mendelian theories of inheritance. The theory of recapitulation did not lack testable implications; these were simply not noted by the participants in the dispute (though Weismann apparently came close). On the other hand, if the biogenetic law had embraced all possible phenomena, we would not need Gould to document the fact that it did not fall beneath the weight of counterexamples. Irrefutable generalizations are (irrefutably) irrefutable. This tautology tells us nothing about how science operates.

Gould may be right in asserting that many theories in "natural history" are immune to empirical refutation. A devout Popperian would conclude from this that such theories have no scientific status. A more moderate conclusion would be that disputes over such theories are unlikely to lead anywhere. At least two of the theories Gould espouses in this book—the theory of hominid neoteny and the theory of evolution by "punctuated equilibrium"—seem to have this character. Proponents and opponents of each theory agree on the range of phenomena, but regard different parts of the range as ideal or typical. History suggests that, as Gould insists, induction by enumeration will not settle such questions. The profitable course in cases like these has often involved abandoning the original question and concentrating instead on the mechanisms that underlie the varying occurrence of the alternative phenomena. We might, for instance, ask, "Which (if any) of the distinctive morphological features of *Homo* represent pleiotropic effects of genes retarding somatic development?" or "What sorts of populations, under what conditions, exhibit gradualistic evolutionary change?" rather than "Is man *by and large* neotenic?" or "Is evolution *by and large* gradualistic?" Gould provides an admirable example when he turns from the Haeckelian controversy ("Does ontogeny by and large recapitulate phylogeny?") to propose mechanisms relating different selective regimes to different sorts of pedomorphosis and recapitulation. His ideas about ontogeny and phylogeny are of greater scientific interest (if not historical importance) than Haeckel's, partly because they are more vulnerable to disproof. In holding that Haeckel's virtually invulnerable theorizing provides a model of scientific procedure in his own discipline, Gould as a historian denigrates his own best accomplishments as a scientist.

MATT CARTMILL
Departments of Anatomy and *Anthropology, Duke University, Durham, North Carolina 27710*

A View of Edison

Edison. The Man Who Made the Future. RONALD W. CLARK. Putnam, New York, 1977. 256 pp., illus. $12.95.

Ronald W. Clark, an English ex-journalist, has for a number of years given himself over to writing scientific biographies for the general public, most notably a 1971 biography of Einstein. The book considered here is not comparable to that earlier one in either bulk or quality. Yet for the time being it may claim a useful though modest place in the Edison literature.

Its shortcomings are several and substantial. For a reviewer, one of them is the lack, at least in this first American

edition, of an introduction explaining the book's occasion and purpose. Obviously, in quotability, abundance of dramatic experiences, picturesqueness of character, and recognition by the general public Thomas Alva Edison approaches Abraham Lincoln and Mark Twain as a surefire subject for popular biography. That fact and the 1977 centennial of the phonograph may explain Clark's choice and timing. But Clark's book does not come near superseding Matthew Josephson's 1959 *Edison* (still available in paperback) as the fullest, soundest, and most absorbing modern biography for the layman; and given Clark's proven expertise as a biographer we may suppose that it was not meant to.

Clark's practiced hand does not entirely conceal evidence of haste. Though as a former journalist Clark doubtless writes with speed as well as fluency, this book contains a few jarring usages, some of them Anglicisms perhaps, but others obviously solecisms. More fundamental evidence is the fact that, in sharp contrast to his *Einstein*, Clark's *Edison* rests entirely on a dozen or so newspapers and journals and fewer than 80 books, many of them peripheral, superficial, or unreliable. He does not use (though Josephson did) the rich manuscript sources available for more than 20 years at the Edison Laboratory National Monument in West Orange, New Jersey, an omission especially lamentable in the case of a subject so thickly festooned with apocryphal anecdotes, often blithely embroidered by Edison himself. Presumably Clark made do with such material as came readily to hand in England. Moreover Clark is occasionally imprecise or downright sloppy in his use of his limited sources.

What, then, is left to recommend this book? As the work of an Englishman, leaning heavily on English sources, it has a point of view subtly different from that of American treatments and so to that degree augments one's perceptions of Edison. The reader catches glimpses of Edison through the eyes of English rivals such as Joseph Swan in electric light and William Friese-Greene in motion pictures (though Clark freely concedes the weakness of their claims). Edison's English involvements, English reactions to and use of his inventions, and English parallels and contrasts all are more strongly emphasized than in Josephson's account. More generally, some readers may find it advantageous that Clark's version is only half as long as Josephson's. It achieves this brevity not only by skillful compression but also by pruning away much of the human interest, historical background, and technological fullness that abound in Josephson, as well as by scanting Edison's later work, such as the storage-battery quest. But it does give the reader a succinct, lucid, readable, and generally judicious though not especially original account of Edison's best-known work, along with enough of the man to suggest his character and personality. In this stripped-down model, moreover, the shape of Edison's career—his role as an "improver" of others' ideas (pp. 73–74), for example, or the flagging of his inventive genius after the early 1880's (pp. 149–50)—stands out more clearly.

In short, the scholarly reader must wait (presumably for Thomas Hughes's work now in progress) and the general reader who has the time should still turn to Josephson, but the casual or hurried reader might reasonably try Clark. Very likely this is all Clark intended.

ROBERT V. BRUCE
Department of History, Boston University, Boston, Massachusetts 02215

Research Specialties

Perspectives on the Emergence of Scientific Disciplines. Papers from two meetings, Paris, Dec. 1973, and York, June 1974. GÉRARD LEMAINE, ROY MACLEOD, MICHAEL MULKAY, and PETER WEINGART, Eds. Mouton, The Hague, and Aldine, Chicago, 1977. xii, 282 pp. $18.50. Maison des Sciences de l'Homme, Paris, Publications, 4.

The study of scientific research specialties is one of the newest and most interesting varieties of the sociology of science, and one of the most likely to catch the interest of scientists. Traditionally, sociology of science has been concerned with small groups (for example, an industrial research laboratory) as material for case studies in the sociology of occupations, or with science as a whole and its professional norms and values. Scientific specialties are a middle level of social organization between the laboratory and the profession. Because they are defined by shared technical knowledge, problems, and skills, specialties promised to be the key to understanding scientific productivity and progress. The central problem of the sociology of specialties is the symbiosis of ideas and social organization: how the life cycle of specialties is shaped by intellectual concerns and how institutions and communication networks shape the development of new ideas. The challenge for sociologists was to understand and integrate the technical content of science with their conceptions of how communities work.

The earliest studies of specialties tended, not surprisingly, to be top-heavy on sociologizing and weakest on the technical side. Specialties were treated as mechanical communication systems, without much attention to what was communicated or why. Intellectual content was dealt with by simple-minded application of historian Thomas Kuhn's scheme of preparadigm and paradigm stages of scientific development. The effects of institutional contexts and national science policies were ignored, as if scientific communities could somehow be isolated in a sociological test tube. The hope was to reduce particular cases to a paradigmatic process of specialty formation, independent of place, time, and circumstance. More recently, however, sociologists have retreated from this cramped reductionist program, and a broader program has begun to emerge that puts more weight on scientific content and on the social and political context of specialties.

The book under review captures the study of specialties in the early stage of this transition. It is a collection of essays from two Anglo-European conferences held in 1973 and 1974 to bring together people working on specialties from different points of view. Owing to the four years' delay in publication, it is by now a retrospective rather than a current assessment of the state of the art. The absence of North American and Israeli sociologists, who were instrumental in opening up the field, makes it still less a representative selection.

The most striking features of this collection of essays are its diversity of approach and its unevenness of quality. Five sociologists, three historians, two scientists, and one political scientist write on agricultural chemistry, thermodynamics, physical chemistry, tropical medicine, biophysics, x-ray crystallography, and radio astronomy. Too many of the essays are simply potboilers; others are self-indulgent exercises in methodologizing or ideologizing. A few make real contributions to our understanding of modern science. Eclecticism, uncertain standards of achievement, and self-consciousness are characteristic of specialties in their early stages, sociologists tell us. So too with the sociology of specialties.

In a sensible and useful introduction (one of the best things in the book), the editors systematically lay out a broad program for the study of specialties. They identify five or six aspects of social reality that may be relevant to any particular case, including institutional con-