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.

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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-

text, two-way linkages with applications in technology, agriculture, and medicine, and political aims of the systems that support science, notably governments. Taking all these aspects into account is a tall order, and few of the essays in this book do so. Michael Worboys's study of British tropical medicine is exemplary, as are the studies of British radio astronomy by Nigel Gilbert and by Michael Mulkay and David Edge. (Edge and Mulkay have since published a much longer account in *Astronomy Transformed*; for a review see *Science* 13 May 1977, p. 774.) Worboys shows how the Colonial Office fostered a kind of holistic medical science that was not in favor in medical schools, with their more biological ideals. Mulkay and Edge show how and why distinctive research programs, along with distinctive administrative structures and styles, evolved at Jodrell Bank, which depended on research grants, and at Cambridge University. They show, in short, how intellectual and institutional innovation occurred in particular places at particular times. They begin to sort out the roles of individual scientists, technical imperatives, and institutional policy in shaping innovation and growth in science.

In the life cycle of research specialties periods of expansiveness are often followed by concentration on a few important and fruitful questions. In 1974 an eclectic approach was a useful corrective to sociological reductionism. The next stage, one hopes, will be a winnowing of grain from chaff. There are some indications here of what the future might bring to the study of specialties. First, it is clear that the study of science in national contexts will be increasingly fruitful as linkages to national institutions are more deeply explored. Second, the problem of the symbiosis of ideas and social structure will become more precisely defined. As the editors point out, none of these essays show that social organization directly shaped the content of science; rather, its influence was on the direction of science, the selection of certain problems over others. This distinction clarifies what the study of specialties can and cannot be expected to do. The influence of social structure on content is probably best studied in larger social aggregates, even Western culture as a whole. It is a problem for the sociology or anthropology of science. (Several studies of the place of science in the culture of professionalism have appeared recently.)

The study of specialties will probably be more and more explicitly concerned with ways in which certain scientific subjects are selectively developed by particular institutions and social and political trends. I suspect that increasing attention will be paid to the ways in which institutions mediate between professional aims and social demands. The size and complexity of modern public science and the practical problems of directing and managing the system will ensure that the study of specialties and disciplines will have meaning—perhaps even utility—to scientists and administrators of science as well as to sociologists and historians.

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Archeoastronomy

Native American Astronomy. Papers from a symposium, Hamilton, N.Y., Sept. 1975. ANTHONY F. AVENI, Ed. University of Texas Press, Austin, 1977. xviii, 286 pp., illus. $15.95.

As the urban night sky becomes increasingly invisible and both literacy and physical intuition decline from lack of exercise, most Americans cannot believe that pre-Christian societies were capable of astronomical sophistication. We are often blinded by the heritage of Rome, which propagated a vigorous anti-intellectualism for over a millennium. Astronomy was not discovered by the European renaissance but was reintroduced by the Muslim invaders who had guarded the discoveries of classical Greece while Europe slept. Neugebauer has demonstrated the marvelous planetary calculations of the Mesopotamian astronomer-priests, Stephenson has translated detailed Chinese records of the same era, and Hawkins has made us believe that even the primitive Celts could build analog astronomical computers of stone. Both primitive and sophisticated astronomy existed in pre-Columbian America as well, and this book is one of the evidences that a serious effort to advance American archeoastronomy is under way.

Unfortunately, there are severe problems. The Spanish invaders so efficiently destroyed the pagan culture that now no one can read the hieroglyphs of the advanced cultures, and no sufficient oral tradition survives from the priest class, of either the advanced or the primitive groups. There is no Rosetta stone for the Americas. At the moment, one can only compare poorly understood texts, search for known astronomical correspondences, and interview relatively non-Europeanized remanent populations. At this stage, native American astronomy is largely conjecture. The papers in this book run the gamut from well-argued, reasonable inference to incomprehensibility and physical error.

As a fundamental astronomer, I have a prejudice in favor of clearly stated mathematical inferences, with their associated uncertainties also stated, numerically when possible. I find the discussions of the calendric systems of the Maya (by Gibbs) and of the Inca (Zuidema) particularly satisfying in this respect. It is evident that these two cultures had developed empirical systems to the point not only of sophistication but of byzantine complexity; no wonder only priests were keepers of the calendar. This realization makes Remington's fieldwork among the present-day Maya all the more poignant. Similarly, the investigations of apparent observational sites used by more primitive North American tribes, reported by Eddy (Great Plains medicine wheels) and Williamson *et al.* (Anasazi observatories), seem to be astronomically sound and well documented.

At the other extreme is the commentary by Kelly on Mayan texts and inscriptions, which appears to be a mixture of numerology and error. There are masses of numbers, largely Mayan dates that a nonspecialist can neither understand nor verify, used in arithmetic attempts to find correspondences with astronomical phenomena. There is an erroneous Julian-calendar-Julian-day number correspondence (p. 59), a geometric error in supposing a lunar eclipse at new moon (p. 63), and either a calculational or a typograhical error (4352 instead of 4532, p. 66). Three of Kelley's tables, though attractive as art, fail to convey information to this astronomer. Finally, Kelley gives great weight to supposed eclipses, which are alleged to be possible within 18 days of a lunar nodal passage. In fact, the moon passes through a node every 13.7 days, so it is *always* within Kelley's limit; according to this, there should be one or two eclipses every month! In reality, no eclipse is ever possible more than about 33 hours from a node crossing; thus many of Kelley's comments on probable or certain eclipses are simply false, which considerably damages his entire argument.

In part, this is an exaggerated case of a larger problem. The use of astronomy and statistics is sometimes superficial. Wedel objects that Eddy demands observational uncertainties, but one finds confusions between "exactly" and "within reasonable certainty," between "exactly" and "observably," and also be-