sium include examples of the increasing sophistication of the methods used to study such topics as the localization of actin and other proteins in cells, the components and conditions necessary for the assembly of microtubules in vitro, and the functions of cilia and flagella, as well as examples of the more tentative approaches to the larger areas of ignorance that remain. In the case of the functions of actin and myosin in cell motility, an overdependence on muscle models may have slowed progress in the past because the search for similarities to muscle sometimes prevented the emergence of new approaches. However, the large amount of actin found in cells and the high ratios of actin to myosin, together with the recently discovered interactions of actin with new proteins not represented in muscle, are now stimulating a reconsideration of the classical question regarding the existence of a cytoskeleton and, since force generated in the cell must be transmitted by some means to be effective, the possible interaction of such an actin-based structural framework with the contractile elements. Force generation and transmission in mitosis remain a mystery that the theories of each decade in turn appear to solve. It seemed at one time that the presence of microtubules in the spindle coupled with their undeniable motile function in cilia and flagella would eventually provide an explanation of chromosome movement. This has not happened—at least to general satisfaction—and actin and myosin are now being implicated in force production. Perhaps the discovery of an interaction between the mechanisms of force generation and transmission will finally put to rest this seemingly perpetual mystery.

A 1963 conference on primitive motile systems is generally considered to be the benchmark for research on cell motility in the 1960's, and the 1975 conference will very likely serve the same function for such research in the 1970's. The three-volume report on the conference is comprehensive in its coverage and includes papers on almost all aspects of current research activity. A comparison of the published results of this conference with those of the earlier one in terms of topics covered and number of pages might be used as a measure of the development of research on cell motility. One can only hope that the tripling of the price of the books is a measure of the increasing affluence of biologists.

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Astronomy Transformed. The Emergence of Radio Astronomy in Britain. DAVID O. EDGE and MICHAEL J. MULKAY. Wiley-Interscience, New York, 1976. xviii, 482 pp. $25. Science, Culture, and Society.

During World War II, in the course of radar research, the British physicist J. S. Hey detected radio emission from the sun, noticed radar echoes from meteor trails, and also observed that much of the cosmic noise on radar sets came from the Galaxy, including a high-intensity source in the direction of Cygnus. In the postwar years, radar studies of meteor trails formed the main initial enterprise of a new research program at the University of Manchester under Bernard Lovell. Radio emissions from the sun and, soon, from discrete sources outside the solar system captured the attention of a team under Martin Ryle at the Cavendish Laboratory and of another at the University of Sydney under J. L. Pawsey. By the early 1950's, the combined work of the three groups was gaining recognition within the British scientific community under the name "radio astronomy," and the field was rapidly acquiring such institutional manifestations of identity as a Royal Astronomical Society Committee on Radio Astronomy.

By late 1952, radio astronomers knew that the sky had a bright radio background against which could be discerned a large number of discrete radio sources. It seemed necessary now to map these discrete sources systematically and to catalog their principal physical characteristics, notably position, spectra, intensity, polarization, angular extent, and radio redshift. Through the next decade, the development of instruments occupied a major part of the respective radio astronomical research programs. At the University of Manchester's Jodrell Bank, the staff concentrated on the design and construction of the 250-foot dish, a multipurpose instrument able to accommodate the interests of both the observers of meteor trails and the students of radio phenomena. The Jodrell Bank group also developed long-baseline interferometer techniques to measure the position and angular extent of radio sources in our own and nearby galaxies.

At Cambridge, impressed by the optical identification of highly energetic radio sources, Ryle aimed to detect sources that might appear to be weak because they were an immense distance away. To gather enough incident energy, Ryle's group proposed to probe radio emissions over a large horizontal area, but with the ingenious twist of relying upon a small number of elements in a rectangular array. By measuring the amplitude and relative phase of the signals striking the elements and then using a Fourier transform on these data, it was possible to obtain the frequency distribution of the signals. This technique, eventually known as "aperture synthesis," was especially powerful when used in conjunction with the earth's rotation, which carried one element around the other over a 24-hour period.

In the 1950's, employing a four-element interferometer, the Cambridge group conducted its 2C and 3C surveys. The results for the distribution of sources in relation to distance contradicted the prevailing steady-state theory of the universe, and the Cambridge group began advancing the view of an evolutionary universe characterized by higher density of sources at extragalactic distances. But the 2C and 3C results failed to distinguish clearly between close and distant weak sources. In fact, at Sydney an independent discrete-source survey, made with a large array in the form of a cross to form a pencil beam, contradicted the 2C and 3C results. About 1958, despite general recognition that a majority of discrete sources must be extragalactic, radio astronomy was colored by acute dissension regarding source counts, source characteristics, and cosmological claims.

In part to resolve the dispute, the 4C survey was undertaken at Cambridge from 1958 to 1964. This relied on full aperture synthesis, reported on 5000 sources, and tended to confirm the central conclusion of the earlier Cambridge work that there was an excess of weaker sources at great distances. After 1961, a survey in Sydney with the new Parkes 210-foot dish ended the dissension with Cambridge, since the two surveys converged on virtually the same density-source distance ratios. But even in the early 1970's there was still no satisfactory explanation of the enormous energies generated by the weak sources, which had stimulated Ryle and his colleagues to study them in the first place. And in the early 1960's, of course, just about the time the Cambridge and Sydney results were converging, quasars and pulsars were discovered.

Pulsars and quasars accelerated the undoubtedly inevitable shift in radio astronomy away from survey work, statistical approaches, and technical programs as such toward detailed study of the specific astrophysical problems, especially energy generation, raised by the sources themselves. Whatever the outcome of these studies may be, radio astronomy has already transformed man's centuries-old ideas of the universe. Some key participants in this remarkable development, notably J. S. Hey, have set down the basic technical history of the field. Now, with *Astronomy Transformed*, we have the first full-scale scholarly treatment of the subject in the centrally important arena of Britain.

David Edge, a onetime member of Ryle's group at Cambridge and now a historian of science at the University of Edinburgh, and Michael Mulkay, a sociologist of science at the University of York, explore the social, technical, and intellectual evolution of British radio astronomy in considerable detail. The origins of the field, its progress at Jodrell Bank, Cambridge, and Sydney, the source-distribution controversy, and the impact of quasars and pulsars form the core of the book. One wishes that the authors had provided more of an introduction to radio astronomy for lay readers, but their technical explications are clear enough for a scientifically literate audience and in some places, as in the discussion of aperture synthesis, are notable. The book is packed with information, technical and otherwise. Edge and Mulkay drew upon the scientific papers produced by radio astronomers and upon historical and biographical writings; they also conducted 20 lengthy interviews, analyzed citation patterns as well as membership in scientific societies, and assessed other types of objective data. Conceived as a contribution to the sociology as well as to the history of science, their book is history informed by sociological considerations, including the social origins of the field and the relationship between the structure of the research groups and the type of research done.

Edge and Mulkay found that the field of radio astronomy emerged through the activity of four different groups: wartime radar physicists who returned to academic physics; wartime government research teams that, like Pawsey's at Sydney, remained intact in governmental facilities; and, to a much lesser extent, optical astronomers or teams in academic departments of electrical engineering. The United States lagged behind Britain in the development of radio astronomy for almost a decade, and the assessment of origins suggests why. For one thing, American radar research teams dispersed after the war, and, once back in the academic world, American physicists turned to nuclear physics or to the rapidly emerging field of particle physics. One might add that in the United States, in contrast to the situation in Britain, radio physics was part of electrical engineering, where research interests were less likely to veer toward astronomical subjects. After the war American astronomers, for their part, were little inclined to initiate work in radio astronomy because their attention was absorbed by the newly completed 200-inch telescope at Mt. Palomar.

Edge and Mulkay argue convincingly that the emergence of radio astronomy does not conform to the prevailing sociological theories of the origins of new scientific specialties, and it does not fit Thomas Kuhn's model of scientific revolutions. A particularly important consideration in both regards is that radio astronomy encountered no resistance from practitioners in the established (optical) sector of astronomy; its practitioners never felt compelled to form a separate research community. With specific reference to Kuhn, Edge and Mulkay observe:

> The typical sequence in radio astronomy has not been the establishment of paradigmatic solutions within stable research areas, followed by anomaly, conflict, and reconceptualization. It has rather been the discovery of new fields of ignorance, followed firstly by exploration and the gradual establishment of scientific consensus and subsequently by at least a partial transfer of interest to further new problem areas.

Intellectually, Edge and Mulkay appropriately stress, radio astronomy supplemented optical astronomy rather than called it in question. Issuing a more general challenge to sociologists of science, Edge and Mulkay emphasize the role of ideas and techniques in molding the development of the field. Sociological analyses of different scientific fields, they rightly declare, will remain incommensurate unless scholars give "much more detailed attention to the *science* (i.e., the 'culture') under study than has been, to date, customary."

Their attentiveness to scientific culture and their exploitation of objective data permit Edge and Mulkay to make illuminating comparisons between Jodrell Bank and Cambridge. Lovell, expansive, active in international and national committee affairs, a scientific extrovert, operated mainly as the entrepreneur for Jodrell Bank and supplied little scientific leadership after the early years. In contrast Ryle, no conference-goer and something of a scientific introvert, exercised significant integrative leadership at Cambridge, coauthoring papers at least once with every long-term member of the staff. Yet the differences between Jodrell Bank and Cambridge, Edge and Mulkay propose, need not be explained by declaring that Ryle and Lovell were two different people. More fundamental is the fact that a series of different scientific, technical, and social situations prevailed that allowed two different styles of leadership to operate. At Jodrell Bank, the early commitment to both radio and radar techniques gave rise to differentiation of research groups and to a demand for a single, multipurpose research instrument. At Cambridge, where the group was much smaller, it seemed, as Ryle once remarked, "more economical to build a number of specialized instruments, each designed for a particular field of research." While the Jodrell Bank enterprise divided naturally into a loose federation of relatively autonomous research teams, the Cambridge operation, by its choice of instrumentation and research program, was no less naturally self-reinforcing, with a coherence in its research program that in scientific terms made the whole perhaps greater than the sum of its parts.

That the social structures of research groups may influence the kind of research they produce warrants emphasis, but the point may be overstressed. Individual personality, qualities of mind, and intellectual conviction of course also shape the development of science, even in groups. If Ryle performed an essential integrative role at Cambridge, one does not learn from this book just how, in tangible, day-by-day ways, he did it. Edge and Mulkay penetrate the laboratory walls only slightly. They report virtually nothing about the process of intellectual interaction, about seminars, planning conferences, or discussions. Indeed, they supply little in the way of anecdote, little about laboratory routine, and little sense of place, of mood, or of the excitement that at least occasionally must have erupted. The authors omit the names of interviewees they quote and, even when there seems no good reason for it, the names of people to whom their respondents refer.

Perhaps Edge and Mulkay felt constrained to set down so impersonal a narrative because most of their subjects are still alive. Perhaps they could not obtain access to information unless they promised their sources anonymity. Yet their book is so uncompromisingly impersonal as to suggest that they simply saw no point in addressing themselves to the human details because they interpreted social history or sociology to require focusing only on the general. Edge and Mulkay call for incorporating the technical dimension in the social study of science. It seems necessary to insist that the human dimension deserves an equal place. The human dimension is certainly compatible with social and intellectual analysis of a general type, and it is an essential part of the story. One hopes that attempts to explore the historical sociology of science will avoid robbing the subject of life.

Astronomy Transformed itself is not lifeless. It takes an important step in the direction of integrating the social with the cognitive development of a remarkable scientific field. While taking as their central subject the evolution of that field in Britain, Edge and Mulkay set their study in the context of its growth elsewhere in the world. Assessment of the relative significance of the British work—and it was without doubt considerable—after the early years will have to await studies of radio astronomy in other countries. Whatever such studies may purport to show, all will have to meet the scholarly standard established by Edge and Mulkay, which is a very high standard indeed.

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Chronicle of a Career

In at the Beginnings. A Physicist's Life. PHILIP M. MORSE. MIT Press, Cambridge, Mass., 1977. xxii, 376 pp., illus. $15.

Philip McCord Morse became seriously interested in physics as an undergraduate at the Case School of Applied Science in his native Cleveland in 1924 and has written his autobiography 53 years later, after a distinguished and varied career centered about his position as professor of physics at M.I.T., where he has been since 1931. He places himself in the second rank of physicists, having been close to many important developments but not having made any big discovery on his own. He was better than many who call themselves physicists today, and the reviewer can vouch for his excellence as a teacher.

His chronicle includes graduate study from 1926 to 1929 at Princeton, during the most exciting period that physics has known in this century, teaching at Ann Arbor and Princeton, postdoctoral study at Munich and Cambridge, participation in the founding of M.I.T.'s outstanding program in the physical sciences, work in antisubmarine warfare and underwater acoustics that contributed to the defeat of the Nazis, a role in the early development of computing at M.I.T., and a major role in the development of operations research.

Morse was coauthor, with E. U. Condon, of the first good book on quantum mechanics. His book *Vibration and Sound* and his two-volume work with Herman Feshbach entitled *Methods of Theoretical Physics* are standard sources in physics. His book with George Kimball on *Methods of Operations Research* is basic to that subject. He has also written on queuing theory and thermal physics.

The changes that have taken place in physics and its relation to the world during the half century reported on are implicit but somewhat dimly seen, owing to the purely factual nature of the account. There is a glimpse of the coherence, spirit, and smallness of the world of physics in the 1920's, in that by 1930 Morse knew K. T. Compton, H. N. Russell, Einstein, von Neumann, Robertson, Davisson, Germer, Wigner, Condon, Sommerfeld, Bragg, Pauling, Heisenberg, Bloch, Teller, Dirac, Kramers, Fermi, Oppenheimer, and Lorentz. Of people entering physics at that time, he says, "They had to be highly interested in physics. There was no fortune or fame to be won; achievement in research or teaching had to be its own reward." Further on, he contrasts that with the spirit of the 1960's, when physicists required fame, wealth, and power, when the "publish or perish" syndrome forced narrow specialization, and when one had to spend much time drawing up proposals for grants and contracts, knowing full well that if you can describe it in advance it is not research.

The most interesting part of the book describes Morse's activities and those of various of his colleagues during the Second World War. The work in underwater sound, for sweeping acoustic mines and decoying acoustic torpedoes, was a natural outgrowth of his interest in acoustics. More important was the antisubmarine warfare work, in which new ideas had to be developed (ideas that led to the discipline now called operations research), in which personal and psychological considerations were as important as statistical facts, and which contributed substantially to the defeat of the Nazis. It is a fascinating account. He contrasts that period, too, with later ones. He says it is almost impossible now, "after the militaristic imbecilities of Vietnam" (he might have added, and the vicious lawlessness of the CIA and presidential mendacity) to explain to younger colleagues why it was overwhelmingly important to win the Second World War.

On the larger question of the role of physics and related science in the world, one has to read between the lines, because of Morse's aversion to stating opinions that cannot be easily substantiated. Physicists were no more religious in the '20's than today, but they could believe in "the progress of mankind onward and upward forever," to quote the creed of one of the churches of that period. The world's space and resources seemed limitless then, and everyone believed that science would eradicate disease, poverty, hunger, and inequities. Fifty years later those ills are still not eradicated, and many new ills previously unthought of are upon us. The work of Jay Forrester and the Club of Rome, both referred to in Morse's book, suggests that "forever" may now be quite short and "progress" may have been reversed already. Whether science is good or evil (or just irrelevant for the world as a whole) will be debated indefinitely, especially by nonscientists, but surely physicists ought to give some thought to those questions. Morse's contribution in this regard concerns operations research, for which he had great hopes in the period immediately following the war. He "hoped that the scientific study of the cooperative actions of men and machines, begun in wartime, might be applied to more humane activities." He writes of the "more socially useful applications of operations research," "man's welfare," "long-range planning."

The last chapter discusses sociological problems in a slightly rambling but serious way. Morse states his conviction that there is no conflict between world planning and science (as though the idea had already occurred to him that there might be). It seems to the reviewer that he combines, on the one hand, a slight sense of disappointment that, although operations research has helped solve problems of libraries, police and fire departments, traffic control, NATO, rail-