The Discovery Track

Social Control and Multiple Discovery in Science. The Opiate Receptor Case. SUSAN E. COZZENS. State University of New York Press, Albany, 1989. xiv, 236 pp. $44.50; paper, $14.95. SUNY Series in Science, Technology, and Society.

Brainstorming. The Science and Politics of Opiate Research. SOLOMON H. SNYDER. Harvard University Press, Cambridge, MA, 1989. xii, 208 pp., illus. $22.50.

Apprentice to Genius. The Making of a Scientific Discovery. ROBERT KANIGEL. Macmillan, New York, 1986. xvi, 271 pp.

Every fall, at least among the scientific elite, attention is riveted on Stockholm. The autumnal announcement of the Nobel prizes is marked by celebration, endless analysis, and, not infrequently, bitter disappointment. This year's prize in physiology or medicine, awarded to Michael Bishop and Harold Varmus for studies on cellular oncogenes and cancer, is a case in point. The award's legitimacy was quickly challenged by Dominique Stéhelin, who claimed to have done the critical experiments as a postdoctoral fellow in the laureates' lab. Stéhelin, who has remained independently active in the field, argues that he deserved to share the prize. Bishop and Varmus disagree. The Nobel committee declines to comment. Persons familiar with the field hold either polar view as well as every position between. And though the media see the dispute as newsworthy, aficionados of cancer research were surprised neither with the winners' selection nor with the dispute.

Questions raised by but transcending the details of this Nobel dispute have long engaged scientists and sociologists of science. These questions include: Who can legitimately claim the credit for a scientific discovery? Are there clearly defined criteria for establishing credit? What are the roles of supervisor, mentor, and "bench scientist" in a discovery? How is the documentation of discovery—publications, presentations, press conferences—orchestrated with an eye to establishing credit? Are the agendas of scientific and career advancement always coincident? If these questions are difficult to answer in a case like that of the current Nobel Prize, they are manifoldly more complicated when multiple groups lay claim to a discovery. Then the question of whether the discoveries were simultaneous or sequential complicates the issue. However thorny, these questions are of profound importance. Understanding the motivations of scientists, as well as the incentives and circumstances that support discovery, is central to developing and implementing productive scientific environments.

The discovery of the opiate receptor provides a rich case study in which to explore these questions. The authors of these three books have all chosen this case to probe the culture of modern biomedical research. Depending on which book you read, you will have a very different view about who discovered the opiate receptor.

Beginning students in the neurosciences now learn that specific ligand-receptor interactions are fundamental to the functioning nervous system. Yet in the early 1970s the importance of the mechanism was hypothetical. Three lines of evidence pointed to the opiates as a model system to test the importance of neural ligand-receptor interactions. First, a basic structural similarity among opiates that killed pain as effectively as morphine suggested that a receptor mechanism was involved. Second, the action of the opiate molecules was "stereospecific." Third, opiate antagonists were discovered. Ligands that bind a receptor may be either agonists or antagonists. An agonist causes a receptor to function. An antagonist blocks function. Thus, the opiate system was ideal to functionally test experimentally for the existence of a specific receptor.

Scientists knew that the first isolation of an opiate receptor would be a prize achievement, for it would have profound impact on understanding the nervous system. That's what happened. Discovery of the opiate receptor immediately raised the question "What was the receptor's normal biological function?" This in turn led to the discovery of endorphins and a whole class of small peptide mediators of nervous system function. Finally, finding the opiate receptor opened the age of biochemical and molecular neurobiology with all its practical consequences. Now it was possible to develop improved pain killers, understand how methadone could be most effectively used to treat heroin addiction, and allow the design of "rational neuroactive drugs," as well as of drug companies.

The discovery of the opiate receptor is thus a scientific achievement of "Nobel" quality, and it is an achievement to which there are no fewer than four sets of claimants (Avram Goldstein *et al.*, Solomon Snyder and Candace Pert, Eric Simon *et al.*, and Lars Terenius).

Susan Cozzens, in *Social Control and Multiple Discovery in Science*, uses the tools of the sociologist to examine whether discovery of the opiate receptor was an incident of multiple (that is, independent and simultaneous) discovery and how the scientific community has chosen to regard its provenance. On the basis of extensive interviews with the principals and members of the scientific community, contemporary reports in the press, and bibliometric analysis (that is, analysis of patterns of citations in the scientific literature) Cozzens clearly demonstrates that the responsibility for discovery of the opiate receptor is by no means clear-cut. Claims to credit seem in large part a function of self-reporting and scientific norms. Cozzens shows clearly that there was a pattern of revisionist thinking by the scientific contenders in how they reported the importance of their work and that of their competitors. As the years progress, the scientists claim more independence and are less generous to their peers. There is a wealth of information in Cozzens's book about the culture of contemporary biomedical research as well as the ability to judge "who has done what." Cozzens paints a fascinating picture of both inter-laboratory and intra-laboratory rivalries. Mentors and proteges see their contributions to discovery very differently.

Cozzens also draws a telling conclusion about our ability ever to know for sure who made the discovery:

> Access to private conversation among scientists during the course of an active controversy over a social category like "multiple discovery" will be a rich source of information about norms and counter-norms of scientific behavior. The indications from the opiate receptor case of a clear delineation between private and public statements suggest that participant observation would be the best methodological tool for such a study.

Cozzens is commended for her data collection and analysis. Would she could be commended for providing a well-written book. Unfortunately *Social Control* reads like the doctoral dissertation it began as. It could have benefited tremendously from more felicitous editing.

On the basis of *Brainstorming* the reader could only draw the conclusion that Solomon Snyder was the primary discoverer of the opiate receptor. Snyder claims that although there were others in the "receptor field" who made conceptual and methodologic contributions, it was an outsider (Snyder) who solved the problem. He draws a picture of an inbred and pre-molecular field

of biology transformed by his quantitative biochemical studies of stereospecific binding of radiolabeled morphine analogs. Is this the whole story? Compare it with the versions told in Cozzens's book or in the contemporary press (including the pages of *Science*) or with the ongoing description by Candace Pert (Snyder's former graduate student) of her role in the discovery of the opiate receptor (evidenced in 1989 *Current Contents*).

Against this background Snyder's portrayal of the events is too simple and just does not ring true. Nowhere in his description is there evidence pointing to a continuing heated debate in which colleagues and former students publicly dispute Snyder's claims of discovery.

Snyder is at his best when describing the experimental basis of the transformation of neurobiology from a descriptive to a molecular science. The reader will be rewarded by a lucid primer from a master. Less rewarding is the very brief and naive treatment of the politics of drug research in the early 1970s. Also striking in its absence is any mention of Nova Pharmaceuticals, the start-up company built around Snyder whose premise is rational drug development as a consequence of understanding ligand-receptor interactions in neurobiology.

Robert Kanigel's *Apprentice to Genius* seems to place the questions posed by Cozzens and the different versions of discovery in context. In this enjoyable book, Kanigel, a professional science writer, traces the scientific genealogy of Snyder and Pert, two of the most colorful players in the opiate receptor story. Making extensive use of interviews and anecdote, Kanigel depicts how, in a mentor-to-protege chain starting with James Shannon and moving to Bernard Brodie and then to Julius Axelrod, the legacy of creativity and empirical style has passed to Snyder and then to Pert. It appears that a pattern of scientific style as well as competition between mentor and protege is inherited. The personal and scientific rivalry between Pert and Snyder that is detailed by Cozzens and is avoided in *Brainstorming* seems strangely similar to the stories Kanigel tells about Brodie and Axelrod.

The reader interested in the culture of science will be rewarded by reading all three books. Only then is the Rashomon-like, subjective nature of the discovery of the opiate receptor revealed. Who really deserves credit in a field where progress comes about in incremental steps? For those who must choose to read only one book, Cozzens's is richest for data about how credit in science is really established. Snyder will teach the most science by delivering an excellent introduction to modern neurobiology. Kanigel is very interesting, a good read, and out of print. Let the reader beware that the answer to the question "Who discovered the opiate receptor?" may not be very clear.

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Solid Structures

Introduction to Quasicrystals. MARKO V. JARIĆ, Ed. Academic Press, San Diego, CA, 1988. x, 285 pp., illus. $49.50. Aperiodicity and Order, vol. 1.

Quasicrystals are crystalline solids that have symmetries (icosahedral, for example) that were formerly thought to be forbidden by the laws of crystallography. They were first discovered as icosahedral alloys of aluminum and manganese in 1984. Quasicrystals differ from ordinary crystals in that the atomic arrangement is not a periodic repetition through space; rather, the structure consists of a non-periodic but highly ordered arrangement that mathematicians call "quasiperiodic," which can be thought of abstractly as arising from an irrational projection from a higher-dimensional periodic lattice.

Since its outset, the science of quasicrystals has consisted of two distinct subfields: metallurgy and mathematics. On the one hand, there has been an active search in the laboratory for new quasicrystalline alloys, and progress has been made in making these materials more defect-free, characterizing their mechanical and electrical properties, and understanding the similarities of these new alloys to ordinary crystalline and glassy solids. On the other hand, the discovery of quasicrystals has stimulated advances in theoretical work on the mathematics of nonperiodic tilings of space and on physical models for novel kinds of order in matter.

This duality is reflected in the present volume. It contains six chapters, four written by theoretical physicists and two by metallurgists. In its goal of providing an introduction to the subject as a whole and tying together its two parts, the book is a partial success. The chapter by T. C. Lubensky does the best, giving some history of quasicrystals, connections with experimental results, and a general discussion of quasiperiodic-tiling theory. Unfortunately, this contribution appears at the end of the book; the reader encountering the subject for the first time would do best by starting there. Another weakness is that little effort has been made to unify the points of view represented in the different chapters; for example, the introductory remarks of the last chapter are partly undercut by those of the first.

This book will pay the greatest dividends to serious students of quasicrystals. It contains several gems: In a chapter by Schaefer and Bendersky there are the essential details that are the lifeblood of metallurgy, such as the fact that 6% Si in AlMn improves the formation of icosahedral alloys. In the last half of the book (and especially Lubensky's chapter), there is one of the best pedagogical discussions to be found anywhere of the "Landau theory" of solidification—the classic phenomenological theory of structural ordering, excitations, and hydrodynamics—which has been applied extensively and successfully to quasicrystals. And the reader will find in Bak and Goldman's and Lubensky's chapters a wealth of information about the six-dimensional geometry used in the theory of icosahedral quasiperiodic tilings.

A drawback of this book is that the most recent information it contains is from 1987. Quasicrystal science has advanced considerably in the last three years—alloys have been discovered with dramatically fewer imperfections, new models of the stability of quasicrystals have emerged, and the mathematics of tilings has progressed. Fortunately, this book is the first of a planned series, with two other volumes already available. These have kept pace with new developments and offer some particularly noteworthy and readable contributions by Roger Penrose, Linus Pauling, Veit Elser, and others.

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Parasitology at MBL

The Biology of Parasitism. A Molecular and Immunological Approach. PAUL T. ENGLUND and ALAN SHER, Eds. Liss, New York, 1988. xvi, 544 pp., illus. $90; paper, $45. MBL Lectures in Biology, vol. 9.

The Biology of Parasitism course at the Marine Biology Laboratory at Woods Hole, Massachusetts, has been in existence since 1980 to teach modern concepts of molecular biology and immunology to young investigators of parasites responsible for human disease. This collection of essays by current and former faculty attempts to convey the intellectual spirit of the course by presenting major concepts rather than by reviewing the literature or presenting data.

The book is divided into three sections entitled Biology of Parasites and Parasitic Disease, Parasite Immunology, and Parasite Molecular Biology, Biochemistry, and Genetics, with a total of 28 essays from an impressive group of investigators. Notable