Book Review/Report

ETHNOGRAPHY OF A NOBEL PRIZE

PAUL RABINOW, *Making PCR: A Story of Biotechnology*, University of Chicago Press, Chicago, 1996, -vii, 190 pp. (ISBN: 0-226-70147-6)

Making PCR is an anthropologist's account of how one of the late Twentieth-Century's most significant 'inventions' *happened*. The polymerase chain reaction (PCR) is what allows one to amplify a specific target DNA exponentially, thus giving researchers unlimited amounts of precise genetic material for their work. Rabinow's book provides a history of the structures, people and techniques that had to be in place to yield PCR. Similar in structure to Sharon Traweek's anthropological study of particle physics (*Beamtimes and Lifetimes*, Harvard University Press, Cambridge, 1988), *Making PCR* opens and closes with philosophical questions about the nature of scientific practice. All of the chapters except the Introduction include interesting interviews with the key figures (David Gelfand, Tom White, Robert Fildes, Jeff Price, Ellen Daniell, Randy Saiki, Henry Erlich, and Shirley Kwok) of whom there are also photos in pp. 170-171. The interviews make the book readable without sacrificing the philosophical discussions which are frequently absent from journalistic popularizations of scientific discovery.

Since the top-down views of imposing theory on scientific practice (hypothetico-deductive model, positivism) has lost favor through the writings of Feyerabend, Kuhn, Hanson, *etc.*, new bottom-up accounts of scientific practice are beginning to appear in the works of Bruno Latour, Evelyn Fox Keller, Donna Haraway, Sharon Traweek and others. *Making PCR* is firmly in this category frequently called 'Science Studies'. Rabinow's Introduction admirably addresses the change in attitude and the increasing rejection of Merton's norms of scientific practice: "universalism, communalism, disinterestedness, and organized skepticism" (p. 10). Rabinow is not rejecting these norms (which he reiterates on p. 159) because "many scientists believe that these norms guide their practice" (p. 13). Rabinow is concerned with context. Indeed, the important question is raised of what PCR *is*: Is it a concept, a technique, or is it an experimental system? Who should get credit, the scientist who came up with techniques that were later combined under a concept, the scientist with the concept, or those that actually got it to work in the lab? Kary Mullis, we shall see, gets the credit and the Nobel Prize for the *concept*. For Mullis' own view on the matter, see his new book *Dancing Naked in the Mind Field* (Pantheon, New York, 1998).

Chapter One, "Toward Biotechnology", is more historical than the philosophical Introduction. Here Rabinow discusses the necessary structures for PCR. The big turning point is the U.S. Supreme Court ruling that allowed the patenting of life forms. This, combined with the growing frustration of postdocs in science with the funding wars in academia, made working in industry, a former taboo, much more attractive. Cetus, in particular, was attractive because its "organizational structure was less hierarchical and more interdisciplinary" than other corporations and academia (p. 36).

Much of the second chapter, "Cetus Corporation: A Credible Force", is concerned with the company's financial situation and how their prospectus and annual reports represented their work and its prospects. The chapter starts with Cetus' public stock offering in 1981 and continues through 1983. In the early 80s, they were working in "diagnostics (chla-

HYLE – An International Journal for the Philosophy of Chemistry, Vol. 4 (1998), 167-169.

mydia, maternal infections in early pregnancy), cancer therapeutics (inteferons and monoclonal antibodies), and agriculture" (p. 51). They had arrangements with companies like Standard Oil, National Distillers, Shell, and Roussel-Uclaf. Cetus decided to put much effort into research on the cancer therapeutic interleukin-2. Cetus was in a race with DuPont to produce ample quantities for lab use.

Against this background, the events of Chapter Three, "PCR: The Experimental Milieu + the Concept", unfold. Cetus wanted to do work with the beta-globin mutation that causes sickle-cell anemia. They wanted to come up with a quick and simple diagnostic tool. The standard method, Southern blotting was time-consuming. This is "relevant to PCR, not only because of its general methodological importance, but because Kary Mullis hated using it" (p. 82) and started thinking about how else to make the DNA probe diagnostic more dependable. There are two possibilities, amplify the signal (better probe) or amplify the target (better, larger sample). The concept of PCR, without going into detail that is inappropriate here, is to amplify the target by getting the DNA to reproduce itself. Rabinow quotes Mullis: "the strands of DNA in the target, and the extended oligonucleotides, would have the same base sequences. In effect, the mock reaction would have doubled the number of DNA targets in the sample!" (p. 96). Nobody believed it would work. Internal conflicts at Cetus began to become more pronounced. The interviews throughout the book (later reflection) highlight this tension. For political reasons, Mullis, who was difficult to work with, switched from the Chemistry section of Cetus to the Human Genetics section. It is at this point that the work to make PCR a tool began.

These developments are addressed in Chapter Four, "From Concept to Tool". The squabbling got worse before patenting and publishing. Mullis left Cetus in September 1986. The patent wasn't granted until June 1987, and the first commercial uses did not happen until late 1988. So "one could hold the view that all of the crucial work done to develop the value of PCR as a research tool and diagnostic method was done by others at Cetus after Mullis left, and that after 1986 he contributed nothing to this value" (p. 133). This chapter in particular, does much to explain the pressure scientists will face in a corporate setting. PCR's success was around the corner.

The story is not over, though. It was time for a "Reality Check" (Chapter Five). It had finally become clear that "PCR is a fundamental tool that makes feasible such megaprojects as the Human Genome Initiative" (pp. 135-136). At the same time, divisions between management and the scientists grew worse. On top of this, Cetus had trouble with the FDA over the interleukin-2 trials. Finally in 1991, Hoffmann-LaRoche bought PCR and Chiron bought Cetus.

We are still left with questions about what we can draw from this particular story. Rabinow's first answers in his Conclusion are not entirely satisfactory: "How typical the configuration I have identified is, was, or will be can be debated and contested. I have absolutely no idea how many 'Whites' or 'Mullises' there are out there, even if one knew how to study such things." (p. 159). This is not a fault because the method he lays out cannot yield a general conclusion (no 'master narrative'). It is not satisfactory in the sense that we still *want* tidy master narratives. Rabinow's final interviews with the principals show their bitterness, not about the decline of Cetus, but the fact that Mullis alone was awarded the Nobel in 1993, and thus receives all the credit. With much insight, Rabinow points out that "committees and science journalists like the idea of associating a unique idea with a unique person, the lone genius. PCR is, in fact, one of the classic examples of teamwork" (p. 161). Tom White, R&D director at Cetus until PCR was bought by Hoffmann-LaRoche (where he now works), notes that this was the first Nobel awarded to work done at the new biotechnology companies. The book ends with a consideration of whether or not PCR is a revolution (as it is often called in the journals). Mullis thinks not. He says, and Rabinow agrees, that it is not a political or a scientific revolution (there was no paradigm shift). But Rabinow disagrees with Mullis' claim that it was "business as usual exploring genes" for six reasons (these can be found on p. 168). The most important of these can be summed up in Rabinow's use of Lévi-Strauss' concept of *bricolage*. Mullis used the "diverse skills and diverse resources" on hand at Cetus to create a new tool in an area for which he was not trained.

The book is a clearly-written account that does not oversimplify the science, but does not let the technical considerations bog down the text, either. It has much to recommend it to those interested in philosophical issues in chemistry. First, there are the broad questions mentioned above about the actual practice of scientists. Secondly, the book points out the tensions between the academy and industry in terms of prestige and grant money. Through the interviews, we see how some of the scientists trained in chemistry or biochemistry in the 60s (*e.g.* Mullis, Gelfand, White, and Fildes) ended up at Cetus working on the biotechnology projects. Lastly, as I mentioned above, the ontological status of scientific discoveries is addressed. How this question interacts with questions of patents, money, and Nobel prizes is something worth thinking about long after you have finished reading Rabinow's book. My main complaint with *Making PCR* is that the text has no index.

Richard L. Bilsker:
Department of Fine Arts and Humanities,
Charles County Community College,
La Plata, MD 20646-0910, USA;
RichardB@charles.cc.md.us

SECOND ISPC CONFERENCE

Cambridge, U.K., August 3-7, 1998

Which other place could have been more suitable for the Second ISPC Conference on the Philosophy of Chemistry than Sidney Sussex College at Cambridge, United Kingdom? There are only few doubts that Sidney Sussex is the college where one of the world's most famous chemists and masters of (what Sir Arthur Conan Doyle called) deduction, Sherlock Holmes, did his first promising steps into the world of science. The very world of the science of stuff changes was the subject of this conference of the International Society of the Philosophy of Chemistry, held from 3 to 7 August.

About 20 participants from six countries attended two lecture sessions with 12 oral presentations and a business meeting. KLAUS RUTHENBERG (Coburg University of Applied Sciences, Germany) spoke about "Philosophy and Alchemy". He pointed out that Alchemy has been closer to natural philosophy than modern chemistry is. Referring to some modern attempts to clarify the interrelations between alchemy, chemistry, and philosophy – particularly those of Theobald, Geiseler, and Liedtke – he discussed peculiarities of chemistry and alchemy with regard to explanatory approaches of the neglect of chemical issues in philosophy (of science). HEINRICH ZOLLINGER (Federal Institute of Technology, Zürich, Switzerland) gave a talk on "Logic, Psychology and Serendipity of Scientific Discoveries: a Case Study in Contemporary Chemistry". Discussing the development of reaction mechanisms for nucleophilic substitution on diazo salts – in which he has been involved personally – he stressed that chemistry consists of logic and intuition. He used the terminology of Thomas Kuhn (e.g. 'normal science', 'crisis') to interpret the historical example and claimed that chemists should know more about the philosophical basis of their own science. In his contribution "Meaning and Misunderstanding: Translation

HYLE, Vol. 4 (1998), 169-170.