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supported by facts, he believed in the good of the inductive method in all walks of life. He put this to work in studies such as the influences of environment on parameters of behaviour of mice and men. He eventually succeeded in building up a *Fondation Française pour l'Etude des Problèmes Humains* in occupied Paris from 1941 to 1944, for which he was much maligned after the liberation.

Carrel's views on the ideal conditions for successful research, which arose from the very different situations he had found in France and in the U.S.A., and which Dr. Malinin judiciously extends to our times, make equally useful reading. Except in this chapter and in the section on Carrel's "immortal" strain of fibroblasts in tissue culture, where the conclusion is, however, at variance with that of J. A. Witkowski ('Dr. Carrel's immortal cells', *Med. Hist.*, 1980, **24**: 129–142), the author hardly pushes his analysis further than did the shorter biography in English by W. S. and P. D. Edwards (1974) (which is not mentioned in the "Selected References").

Thus it may perhaps be regretted that Carrel's relationship to the views on science expressed simultaneously by his countrymen, Charles Robert Richet (winner of a medical Nobel Prize in 1914) and Lecomte du Noüy (of the Pasteur Institute) are omitted or only cursorily mentioned in the present volume. Lecomte du Noüy in fact claimed in the Introduction to owe his writing of *Le temps et la vie* (1936) to Carrel's stimulus. Neither is the reader introduced to, for example, Carrel's posthumously published *Réflexions sur la conduite de la vie* (1950). Scholars will miss more the absence of footnotes than the lack of biographical references to a number of persons introduced in this book. The easily understandable descriptions of complex technical aspects involved in Carrel's experimentation will, however, be appreciated.

The book is nevertheless well worth reading by both active scientists and historians as a reminder of a man whose ideas were sometimes termed "visionary" and as a stimulant for future research into a captivating group of modern medical investigators.

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BRUNO LATOUR and STEVE WOOLGAR, *Laboratory life. The social construction of scientific facts*, Beverly Hills, Calif., and London, Sage Publications, 1979. 8vo, pp. 272, illus., £11.25 (£5.50 paperback).

About five years ago Bruno Latour, trained in France as a philosopher and semiotician, set out for deepest California to undertake an anthropological study of a tribe of scientists. Surviving both the perils of their exotic customs and the considerable temptations to go native, he has returned to relate (with Dr. Woolgar) his discoveries in (almost) standard sociological English.

Here are some of his findings: scientific laboratories and the devices they contain exist to produce "inscriptions". Sometimes the laboratory's inhabitants say that certain inscriptions and literary productions pertain to specific bits of natural "reality", for example, the neurohormone "TRH" (for these tribesmen call themselves, amusingly, "neuroendocrinologists"). The purpose of all their various and expensive inscription devices seems to be the production of what they call "facts".

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Statements of fact take the literary form of unconditional assertions “about” a reality which is “out there”, viz. that “TRH is Pyro-Glu-His-Pro-NH₂”. A fact is stabilized in their culture when the statement is said to match reality; at that time reality *accounts for* the statement. No human agency is referred to in statements of fact; that is why they are facts and not artefacts.

This is a puzzle. Facts are manifestly made, and socially made, for the anthropologist has observed the process of construction and seen the intense collective work required to stabilize a fact. Yet once stabilized as facts all human agency involved in their production is systematically stripped away. It may help the observer to realize that scientists work not only to establish facts but also to cast doubt upon the facticity of other scientists’ statements. This they may do by showing the human agency involved in the claims and by inserting conditional grammatical modes into the statements (“Bloggs believes that . . .”; “it may be that . . .”). The anthropologist begins to suspect that he must account not only for the social construction of facts but also for the illusion that no human agency is involved.

This book is an attempt at such an account of scientific activity. All the relevant claims and demonstrations cannot be summarized in a brief review, but here are some: Facts are not representations of objects “out there”, but are constituted solely by the use of inscriptions. The distinction between “the social” and “the scientific” is itself an artful contrivance of scientists: a strategy they use in the social production of facts. Reasoning processes in science and in commonsense discourse are not different in kind.

All this may well strike the historian of science or medicine as utterly outrageous. Any such claims are bound to derive from the most abstract and fanciful theorizing. Not so. This book is without question the most concrete and detailed account we have of how scientists actually behave, how they talk with one another, and how they interact with their technological devices. There can be no confusion between this type of sociology of science and the still-dominant sort which focuses upon the public relations of science. In style and in sympathy it is similar to Ludwik Fleck’s recently resurrected *The genesis and development of a scientific fact* (Chicago, 1979; orig. publ. 1935). Both Fleck and this book ought to be confronted by any historian of science or medicine seriously concerned with the actual practice of science as opposed to idealized accounts of its theoretical structures.

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