Laboratory Life

The Construction of Scientific Facts

Bruno Latour • Steve Woolgar

Introduction by Jonas Salk

With a new postscript by the authors

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PREFACETO SECOND EDITION

The most substantial change to the first edition is the addition of an extended postscript in which we set out some of the reactions to the book's first publication in the light of developments in the social study of science since 1979. The postscript also explains the omission of the term "social" from this edition's new subtitle. Other minor additions include a detailed Table of Contents, Additional References, and an Index. Readers tempted to conclude that the main body of the text replicates that of the original are advised to consult Borges (1981).

Wolvercote, August 1985

To The Salk Institute

"If sociology could not be applied in a thorough going way to scientific knowledge, it would mean that science could not scientifically know itself."

—Bloor (1976)

"Méfiez-vous de la pureté, c'est le vitriol de l'âme."

—M. Tournier (Vendredi)

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INTRODUCTION

Scientists often have an aversion to what nonscientists say about science. Scientific criticism by nonscientists is not practiced in the same way as literary criticism by those who are not novelists or poets. The closest one comes to scientific criticism is through journalists who have had an education in science, or through scientists who have written about their own personal experiences. Social studies of science and philosophy of science tend to be abstract or to deal with well-known historical events or remote examples that bear no relationship to what occurs daily at the laboratory bench or in the interactions between scientists in the pursuit of their goals. In addition, journalistic or sociological accounts seem sometimes to have the sole purpose of proving merely that scientists are also human.

A love-hate relationship exists toward scientists in some segments of society. This is evident in accounts that deal with facets ranging from tremendously high expectations of scientific studies to their cost and their dangers—all of which ignore the content and process of scientific work itself. In the name of "science policy," studies of scientific activity by economists and sociologists are often concerned with numbers of publications and with duplication of effort. While such examinations are of some value, they leave much to be desired because, in part, the statistical tools are crude and these exercises are often aimed at controlling productivity and creativity. Most importantly, they are not concerned with the substance of scientific thought and scientific work. For these reasons, scientists are not drawn to read what outsiders have to say about science and much prefer the views of scientists about scientific endeavors.

However, the present book is somewhat different from accounts usually written by nonscientists about science. It's based on a two-

year study by a young French philosopher which was carried out at The Salk Institute for Biological Studies and which was subsequently written up in collaboration with an English sociologist. Although I was not responsible for the initial invitation, I welcomed the opportunity to see if the approach that was contemplated would remedy some of the shortcomings of previous social studies of science.

The approach chosen by Bruno Latour was to become part of a laboratory, to follow closely the daily and intimate processes of scientific work, while at the same time to remain an "inside" outside observer, a kind of anthropological probe to study a scientific "culture"—to follow in every detail what the scientists do and how and what they think. He has cast what he observed into his own concepts and terms, which are essentially foreign to scientists. He has translated the bits of information into his own program and into the code of this profession. He has tried to observe scientists with the same cold and unblinking eye with which cells, or hormones, or chemical reactions are studied—a process which may evoke an uneasy feeling on the part of scientists who are unaccustomed to having themselves analyzed from such a vantage point.

The book is free of the kind of gossip, innuendo, and embarrassing stories, and of the psychologizing often seen in other studies or commentaries. In this book the authors demonstrate what they call the "social construction" of science by the use of honest and valid examples of laboratory science. This in itself is an achievement for they are, in a sense, laymen to laboratory science and are not expected to grasp its fundamentals, but merely expected to comprehend only that which is easiest to understand, such as the superficial aspects of laboratory life.

In reading this book about my colleagues who have been observed under a sociologist's microscope, I realized how "scientific" a study of science could be when viewed by an outsider who felt impelled to imitate the scientific approach he observed. The authors' tools and concepts are crude and qualitative, but their will to understand scientific work is consistent with the scientific ethos. Their courage, and even brashness, in this undertaking reminds me of many scientific endeavors in which nothing stands in the way of the pursuit of an inquiry. This kind of objective observation by an outsider of scientists at work, as if they were a colony of ants or of rats in a maze, could be unbearable. However, this seems not to be so, and for me the most interesting part of the work and of its outcome, is that Bruno Latour, a philosopher-sociologist, began a sociological study of biology and

along the way came to see sociology *biologically.* His own style of thought was transformed by our concepts and ways of thinking about organisms, order, information, mutations, etc. Curiously, instead of sociologists studying biologists, who in turn are studying life processes —in a sort of infinite regression—here are sociologists coming to recognize that their work is only a subset of our own kind of scientific activity, which in turn is only a subset of life in the process of organization.

The final point, intended to suggest that this book is not unworthy of the attention of scientists, is in the bridge made between science or scientists and the rest of society. The word "bridge" is not quite right and I doubt that it would be acceptable to the authors because they claim to go much further. One of their main points is that the social world cannot exist on one side and the scientific world on the other because the scientific realm is merely the end result of many other operations that are in the social realm. "Human affairs" are not different from what the authors call "scientific production," and the chief accomplishment they claim is to reveal the way in which "human aspects" are excluded from the final stages of "fact production." I have doubts about this way of thinking and, in my own work, find many details which do not fit this picture, but I am always stimulated by attempts to show that the two "cultures" are, in fact, only one.

Whatever objection may be raised about the details and by the author's arguments, I am now convinced that this kind of direct examination of scientists at work should be extended and should be encouraged by scientists themselves in our own best interest, and in the best interest of society. Science, in general, generates too much hope and too much fear, and the history of the relationship of scientists and nonscientists is fraught with passions, sudden bursts of enthusiasm, and equally sudden fits of panic. If the public could be helped to understand how scientific knowledge is generated and could understand that it is comprehensible and no more extraordinary than any other field of endeavor, they would not expect more of scientists than they are capable of delivering, nor would they fear scientists as much as they do. This would clarify not only the social position of scientists in society, but also the public understanding of the substance of science, of scientific pursuits and of the creation of scientific knowledge. It is sometimes discouraging that although we dedicate our lives to the extension of knowledge, to shedding light and exemplifying rationality in the world, the work of individual scientists, or the work of

scientists in general, is often understood only in a sort of magical or mystical way.

Even if we do not agree with the details of this book, or if we find it slightly uncomfortable or even painful in places, the present work seems to me to be a step in the right direction toward dissipating the mystery that is believed to surround our activity. I feel certain that in the future many institutes and laboratories may well include a kind of in-house philosopher or sociologist. For myself, it was interesting to have Bruno Latour in our institute, which allowed him to carry out the first investigation of this kind of which I am aware and, most interestingly, to have observed the way in which he, and his approach, was transformed by the experience. It would be very useful for this critique itself to be criticized. This would both help the authors (and other scholars with similar interests and background) to assist scientists to understand themselves through the mirror provided, and help a wider public understand the scientific pursuit from a new and different and rather refreshing point of view.

—Jonas Salk, M.D.

La Jolla, California
February 1979

Chapter 1

FROM ORDER TO DISORDER

5 *mins.* John enters and goes into his office. He says something very quickly about having made a bad mistake. He had sent the review of a paper. . . . The rest of the sentence is inaudible.

5 *mins. 30 secs.* Barbara enters. She asks Spencer what kind of solvent to put on the column. Spencer answers from his office. Barbara leaves and goes to the bench.

5 *mins. 35 secs.* Jane comes in and asks Spencer: "When you prepare for I.V. with morphine, is it in saline or in water?" Spencer, apparently writing at his desk, answers from his office. Jane leaves.

6 mins. 15 secs. Wilson enters and looks into a number of offices, trying to gather people together for a staff meeting. He receives vague promises. "It's a question of four thousand bucks which has to be resolved in the next two minutes, at most." He leaves for the lobby.

6 mins. 20 secs. Bill comes from the chemistry section and gives Spencer a thin vial: "Here are your two hundred micrograms, remember to put this code number on the book," and he points to the label. He leaves the room.

Long silence. The library is empty. Some write in their offices, some work by windows in the brighly lit bench space. The staccato noise of typewriting can be heard from the lobby.

9 mins. Julius comes in eating an apple and perusing a copy *of Nature.*

9 mins. 10 secs. Julie comes in from the chemistry section, sits down on the table, unfolds the computer sheets she was carrying, and begins to fill in a sheet of paper. Spencer emerges from his office, looks over her shoulder and

says: "hmm, looks nice." He then disappears into John's office with a few pages of draft.

9 *mins. 20 secs.* A secretary comes in from the lobby and places a newly typed draft on John's desk. She and John briefly exchange remarks about deadlines.

9 *mins. 30 secs.* Immediately following her, Rose, the inventory assistant, arrives to tell John that a device he wants to buy will cost three hundred dollars. They talk in John's office and laugh. She leaves.

Silence again.

10 mins. John screams from his office: "Hey Spencer, do you know of any clinical group reporting production of SS in tumour cells?" Spencer yells back from his office: "I read that in the abstracts of the Asilomar conference, it was presented as a well-known fact." John: "What was the evidence for that?" Spencer: "Well, they got an increase in … and concluded it was due to SS. Maybe, I'm not sure they directly tested biological activities, I'm not sure." John: "Why don't you try it on next Monday's bioassay?"

10 mins. 55 *secs.* Bill and Mary come in suddenly. They are at the end of a discussion. "I don't believe this paper," says Bill. "No, it's so badly written. You see, it must have been written by an M.D." They look at Spencer and laugh, . , (excerpt from observer's notes),

Every morning, workers walk into the laboratory carrying their lunches in brown paper bags. Technicians immediately begin preparing assays, setting up surgical tables and weighing chemicals. They harvest data from counters which have been working overnight. Secretaries sit at typewriters and begin recorrecting manuscripts which are inevitably late for their publication deadlines. The staff, some of whom have arrived earlier, enter the office area one by one and briefly exchange information on what is to be done during the day. After a while they leave for their benches. Caretakers and other workers deliver shipments of animals, fresh chemicals and piles of mail. The total work effort is said to be guided by an invisible field, or more particularly, by a puzzle, the nature of which has already been decided upon and which may be solved today. Both the buildings in which these people work and their careers are safeguarded by the Institute. Thus, cheques of taxpayers' money arrive periodically, by courtesy of the N.I.H., to pay bills and salaries. Future lectures and meetings are at the forefront of people's minds. Every ten minutes or so, there is a telephone call for one of the staff from a colleague, an editor, or some official. There are conversations, discussions, and arguments at the benches: "Why don't you try that?" Diagrams are scribbled on blackboards. Large numbers of computers spill out masses of print-out. Lengthy data sheets accumulate on desks next to copies of articles scribbled on by colleagues.

By the end of the day, mail has been dispatched together with manuscripts, preprints, and samples of rare and expensive substances packed in dry ice. Technicians leave. The atmosphere becomes more relaxed and nobody runs anymore. There are jokes in the lobby. One thousand dollars has been spent today. A few slides, like Chinese idiograms, have been added to the stockpile; one character has been deciphered, a miniscule, invisible increment. Minute hints have dawned. One or two statements have seen their credibility increase

(or decrease) a few points, rather like the daily Dow Jones Industrial Average. Perhaps most of today's experiments were bungled, or are leading their proponents up a blind alley. Perhaps a few ideas have become knotted together more tightly.

A Philippine cleaner wipes the floor and empties the trash cans. It has been a normal working day. Now the place is empty, except for the lone figure of an observer. He silently ponders what he has seen with a mild sense of bewilderment . . . (Observer's Story).

Since the turn of the century, scores of men and women have penetrated deep forests, lived in hostile climates, and weathered hostility, boredom, and disease in order to gather the remnants of so-called primitive societies. By contrast to the frequency of these anthropological excursions, relatively few attempts have been made to penetrate the intimacy of life among tribes which are much nearer at hand. This is perhaps surprising in view of the reception and importance attached to their product in modern civilised societies: we refer, of course, to tribes of scientists and to their production of science. Whereas we now have fairly detailed knowledge of the myths and circumcision rituals of exotic tribes, we remain relatively ignorant of the details of equivalent activity among tribes of scientists, whose work is commonly heralded as having startling or, at least, extremely significant effects on our civilisation.

It is true, of course, that in recent years a wide variety of scholars have turned their attention to science. Frequently, however, their interest has focused on the large-scale effects of science. There are now a number of studies of the size and general form of overall scientific growth (e.g., Price, 1963; 1975), the economics of its funding (Mansfield, 1968; Korach, 1964), the politics of its support and influence (Gilpin and Wright, 1964; Price, 1954; Blisset, 1972), and the distribution of scientific research throughout the world (Frame et al., 1977). But it is easy to be left with the impression that research with such macroconcerns has enhanced rather than reduced the mystery of science. Although our knowledge of the external effects and reception of science has increased, our understanding of the complex activities which constitute the internal workings of scientific activity remains undeveloped. The emphasis on the external workings of science has been exacerbated by the application of concepts to science which are peculiar to social scientists of differing persuasions and theoretical commitments. Rather than making scientific activity more understandable, social scientists have tended through their use of highly specialised concepts to portray science as a world apart. A

plethora of different specialised approaches have variously been brought to bear on science, such that the resulting overall picture is largely incoherent. Analyses of citations in scientific papers tend to tell us little about the substance of the papers; macroanalyses of science funding remain virtually silent on the nature of intellectual activity; quantitative histories of scientific development have tended to overemphasise those characteristics of science which most readily lend themselves to quantification. In addition, many of these approaches have too often accepted the products of science and taken them for granted in their subsequent analysis, rather than attempting to account for their initial production.

Our dissatisfaction with these approaches was considerably worsened by the realisation that very few studies of science have undertaken any kind of self-appraisal of the methods employed. This is surprising in that one might automatically expect students of science to be constantly aware of the basis for their pretensions to produce "scientific" findings: it might be reasonable to expect scholars concerned with the production of science to have begun to examine the basis for their own production of findings. Yet the best works of these scholars remain mute on their own methods and conditions of production. It can, of course, be argued that a lack of reflexivity is inevitable in an area which is still comparatively young, and that excessive attention to methodological issues would detract from the production of badly needed, albeit preliminary, research findings. But, in fact, the little evidence available suggests that new research areas do not usually postpone discussions of methodological issues in favour of the early production of substantive results. Rather, methodological clarification and discussion take place at an early stage of development (Mulkay et al., 1975). Perhaps a more plausible explanation of the lack of methodological reflexivity in social studies of science is simply that such an approach would be inconsistent with the dominance of macroconcerns noted already. Attention to the details of one's own methodology would thus constitute an enterprise radically different from concerns with overall development, or the implications of growth for science policy and funding.

Partly as a result of our dissatisfaction, and in an effort both to penetrate the mystique of science and to provide a reflexive understanding of the detailed activities of working scientists, we decided to construct an account based on the experiences of close daily contact with laboratory scientists over a period of two years (see Materials and Methods below).

The Observer and the Scientist

When an outside observer first expresses interest in the activities of working scientists, he can expect one of a variety of different reactions. If he is a fellow professional scientist working in a different field, or if he is a student working towards final admission into the scientific profession, the outsider will usually find that his interest is easily accommodated. Barring any circumstances involving extreme secrecy or competition between the parties, scientists can react to expressions of interests by adopting a teaching role. Outsiders can thus be told the basic principles of scientific work in a field which is relatively strange to them. However, for outsiders who are completely ignorant of science and do not aspire to join the ranks of professional scientists, the situation is rather different. The most naive (and perhaps least common) reaction is that nonscientific outsiders simply have no business probing the activities of science. More commonly, although working scientists realise that a variety of nonscientific outsiders, such as historians, philosophers, and sociologists can and do have professional interests in science, the precise point of their questions and observations is a source of some bewilderment. This is understandable in that working scientists do not normally possess more than outline knowledge of the principles, theories, methods, and issues at stake within disciplines other than their own. An observer who declares himself to be an "anthropologist of science" must be a source of particular consternation.

On the one hand, lack of knowledge can lead to marked disinterest in the reports produced by outsiders about science. A common response of this kind is that scholarly tracts in social studies of science seem "rather dull." If nothing else, this kind of comment provides a salient reminder of the perceived irrelevance for scientists of many social studies of science. On the other hand, lack of familiarity with disciplines outside natural science can provoke suspicion. Thus, it is often assumed that outsiders' interests must focus on the seedier aspects of scientific life because investigators are seen to be posing questions which are essentially irrelevant to practical scientific activity. Consequently, the fodder deemed most appropriate for such investigators tends to be tales of scandal and intrigue, of behaviour which fails the usual high standards of scientific enquiry or which is unethical, of the exchange of great ideas over coffee, or of renowned acts of genius and various eureka experiences. This is not to suggest that outsiders necessarily take such information at face value. Never-

theless, it is clear that the kind of information provided by scientists will have a significant effect in shaping investigators' reports and that the information provided depends, in turn, on the nature of the relationship between scientist and investigator. It is important, therefore, to look briefly at the nature of this relationship and at the way it may affect the production of reports about science.

We were fortunate that the discussion in this volume is informed by research carried out at an institution with an avowedly well-developed tradition for the cultivation of a wide range of scientific and philosophical interests. In particular, the founders had established the principle that the institution should house research interests which encompassed a range of "life sciences" well beyond those of mainstream biology. For example, a department of linguistics was conceived as an integral part of the institution. Partly as a result of this general principle, problems of initial access were considerably lessened. Under the auspices of the head of one particular laboratory, one of us was given office space for two years in immediate proximity to the day to day activities of working scientists. However, despite the alleviation of institutional obstacles to entry, the outside observer remained a source of some puzzlement for members of the laboratory. What exactly were his specific motives and objectives in studying the laboratory?

It is perhaps tempting for an outside observer to present his interests in terms of established categories of scholarly investigation, rather than in a way which might exacerbate participants' curiosity or sense of suspicion. For example, the label of "historian" or "philosopher" might be more readily acceptable than either "sociologist" or "anthropologist." The term "anthropologist" is readily associated with the study of "primitive" or "prescientific" belief systems. The term "sociologist" gives rise to a plethora of different interpretations, but essentially it can be seen by the working scientist to concern a range of phenomena, all of which impinge in some way on matters of social and political intrigue. Not surprisingly, therefore, the application of the term "sociology" to a study of scientific activity will be regarded by many scientists as dealing primarily with all these "nonscientific" aspects of science. Sociological interest in science thus appears to concern a variety of behavioural phenomena which fall into a residual category: these phenomena unavoidably impinge upon scientific practice by virtue of the fact that scientists are social beings; but they are essentially peripheral to the practice itself. In this view, social

phenomena occasionally make their presence felt in instances of extreme secrecy, fraud, or on other relatively infrequent occasions. It is only then that the kernel of scientific logic and procedure is severely threatened and scientists find their work disrupted by the intrusion of external factors.

The Social and the Scientific: A Participant's Resource

A number of sources testify to the prevalence of this conception of sociology and "things social" among scientists. Firstly, this view is consistent with the relatively frequent perception by scientists that sociologists are engaged in some kind of scholarly muckraking. In response to enquiries from investigators who have declared their lack of scientific expertise, information is provided which concerns events essentially external to science. Secondly, a method commonly used by scientists to fault or cast doubt on the claims of others is to draw attention to the social circumstances of the production of the claim. For example, the assertion that

> X observed the first optical pulsar

can be severely undermined by use of the following formulation:

> X thought he had seen the first optical pulsar, having stayed awake three nights in a row and being in a state of extreme exhaustion.

In the second version, the inner logic of systematic scientific procedure has been disrupted by the intrusion of social factors. As we shall see in more detail in due course, "social factors" here refer both to "staying awake three nights" as well as to the transformation of a straightforward "observation" into emphasis on the process of "thinking about seeing something." For the observation to have been successful, science should have proceeded either in isolation from such "social factors" or, as is sometimes the case with "great" scientists, in spite of them. Given the presence of such "social factors," no ordinary scientist can pursue science successfully. Observations, claims, and achievements can thus be explained away or faulted by the invocation of social circumstances. Thirdly, although the invocation of social circumstances can be used to detract from scientific achievement, it is also possible to recast social factors as an integral part of routine

scientific procedure. As a result, the "social factors" in question no longer appear extraneous to science. Because they are no longer about the "social," these factors pass beyond the realm of sociological expertise. For example, in the case of the discovery of pulsars (Woolgar, 1978), a number of radio astronomy groups complained that their Cambridge rivals had unduly delayed the release of news of their discovery. In other words, attempts were made to lessen the nature of the Cambridge achievement by drawing attention to the way in which communication about the discovery had been handled. One of very many commentators made the following double-edged comment:

> The truth is that Hewish and the whole Cambridge group had for several months achieved a screen of security and secrecy which, in itself, was almost as much of an accomplishment as the discovery itself (Lovell, 1973:122).

By way of reply to similar criticisms, Cambridge spokesmen claimed that the need for secrecy was merely part of a normal scientific process:

> In the long history of science, it has, I think, been regarded as the right of an individual or group making a scientific discovery to follow up this discovery without any obligation to publish their first preliminary result (Ryle, 1975).

The argument here is that what had been regarded as grounds for casting doubt on the scientificity of Cambridge's conduct, was in fact integral to the normal process of science. Behaviour dubbed "secretive" (the term itself was hotly contested by Cambridge participants) was held to be a normal part of scientific procedure rather than an extraneous social factor which could be used to fault Cambridge behaviour. Moreover, several participants argued that because such behaviour was a normal part of the scientific process, it did not merit any special attention by sociological outsiders.

We shall return in due course to a detailed discussion of the use by scientists of similar procedures in dealing with the circumstances associated with their activities. But our argument is not just that the distinction between "social" and "intellectual" is prevalent among working scientists. More importantly, this distinction provides a resource upon which scientists can draw when characterising either their own endeavours or those of others. It is therefore important to

investigate the nature of this distinction and the way it is used by scientists. The extent to which the distinction between "social" and "intellectual" is accepted as unproblematic by observers of science may have significant consequences for the reports about science which they produce.

The Social and the Scientific: The Observer's Dilemma

At one extreme, we can envisage the wholesale adoption by an observer of the distinction mentioned above. In this case, the observer holds an assumption that scientific phenomena occupy a realm largely distinct from that of social phenomena, and that it is only to the latter that the concepts, procedures, and expertise of sociology can be applied. As a result, the procedures and achievements central to scientists' work become largely immune from sociological explanation. Approaches which implicitly adopt this standpoint have been roundly criticised on several grounds. Rather than repeat these criticisms in detail, we shall merely outline some of the main critical themes. Firstly, the decision to concentrate only on "social" rather than "technical" aspects of science severely limits the range of phenomena that can be selected as appropriate for study. Put simply, this means that there is no point in doing sociology of science unless one can clearly identify the presence of some politician breathing down the necks of working scientists. Where there is no such obvious interference by external agencies, it is argued, science can proceed without the need for sociological analysis. This argument hinges on a particularly limited notion of the occasional influence of socio-political factors; the substance of science proceeds unaffected if such factors are absent. Secondly, emphasis on "social" in contradistinction to "technical" can lead to the disproportionate selection of events for analysis which appear to exemplify "mistaken" or "wrong" science. As we shall show, an important feature of fact construction is the process whereby "social" factors disappear once a fact is established. Since scientists themselves preferentially retain (or resurrect) the existence of "social" factors where things scientific are thought to have gone wrong, the adoption of the same viewpoint by an observer will necessarily lead him to the analysis of the way social factors affect, or have given rise to, "wrong" beliefs. As Barnes (1974) has argued, however, there is at least a very real need for a symmetrical approach to the analysis of beliefs (cf., Bloor, 1976). Scientific