

NOTES

1. We make no attempt here systematically to relate our methodological procedures to those used in anthropological studies. For preliminary discussions on the relevance of anthropology for the study of science, see Horton (1967) and the readings in Wilson (1970). More recent discussions include Shapin (forthcoming) and Bloor (1978).

2. Medawar (1964) formulates his argument in terms of the "processes of thought" which are misrepresented through scientific reports. While agreeing with the general point that these reports are a source of considerable obfuscation, we have severe reservations about any quest for the "thought processes" which "underly" the construction of these reports. As we argue in detail in Chapter 4, explanations of scientific activity in terms of thought processes are themselves considerably misleading.

3. The point has been made by a number of authors. See, for example, the discussions in Lakatos and Musgrave (1970) and Bloor (1974: 1976).

4. This argument is developed at length in Woolgar (1978).

5. This theme is taken up again in Chapter 6 with reference to the game of "Go." At the beginning of the game, any move appears as possible, or as good, as any other.

6. The rationale for this strategy and its effects on the relationship between observer and participants will be discussed in detail elsewhere.

7. A number of French authors have recently discussed laboratory science. See, for example, Lemaine et al. (1977), Callon (1978). For a remarkable history of the biology laboratory in the eighteenth century, see Salomon-Bayet (1978).

Latour, B. & Woolgar, S. (1979). *Laboratory Life: The Construction of Scientific Facts*. Princeton, NJ: Princeton Univ. Press.

Chapter 2

AN ANTHROPOLOGIST VISITS THE LABORATORY

When an anthropological observer enters the field, one of his most fundamental preconceptions is that he might eventually be able to make sense of the observations and notes which he records. This, after all, is one of the basic principles of scientific enquiry. No matter how confused or absurd the circumstances and activities of his tribe might appear, the ideal observer retains his faith that some kind of a systematic, ordered account is attainable. For a total newcomer to the laboratory, we can imagine that his first encounter with his subjects would severely jeopardise such faith. The ultimate objective of systematically ordering and reporting observations must seem particularly illusory in the face of the barrage of questions which first occur to him. What are these people doing? What are they talking about? What is the purpose of these partitions or these walls? Why is this room in semidarkness whereas this bench is brightly lit? Why is everybody whispering? What part is played by the animals who squeak incessantly in ante-rooms?

But for our partial familiarity with some aspects of scientific activity and our ability to draw upon a body of common sense assumptions, a flood of nonsensical impressions would follow the formulation of these

questions. Perhaps these animals are being processed for eating. Maybe we are witnessing oracular prophecy through the inspection of rats entrails. Perhaps the individuals spending hours discussing scribbled notes and figures are lawyers. Are the heated debates in front of the blackboard part of some gambling contest? Perhaps the occupants of the laboratory are hunters of some kind, who, after patiently lying in wait by a spectograph for several hours, suddenly freeze like a gun dog fixed on a scent.

Such speculations and the questions which give rise to them appear nonsensical precisely because we as observers do presuppose some knowledge of what the laboratory could be doing. For example, it is possible to imagine the purpose of walls and partitions without ever having set foot in a laboratory. We attempt to make sense not by bracketing our familiarity with the setting but by using features which we perceive as common both to the setting and to our knowledge or previous experience. Indeed, it would be difficult to provide any sensible account of the laboratory without recourse to our taken-for-granted familiarity with some aspects of science.

Clearly, then, the observer's organisation of questions, observations, and notes is inevitably constrained by cultural affinities. Only a limited set of questions is relevant and hence sensible. In this sense, the notion of a *total* newcomer is unrealisable in practice. At another extreme, an observer's total reliance on scientists' versions of laboratory life would be unsatisfactory. A description of science cast entirely in terms used by scientists would be incomprehensible to outsiders. The adoption of scientific versions of science would teach us little that is new about science in the making; the observer would simply reiterate those accounts provided by scientists when they conduct guided tours of their laboratory for visitors.

In practice, observers steer a middle path between the two extreme roles of total newcomer (an unattainable ideal) and that of complete participant (who in going native is unable usefully to communicate to his community of fellow observers). This is not to deny, of course, that at different stages throughout his research he is severely tempted towards either extreme. His problem is to select a principle of organisation which will enable him to provide an account of the laboratory sufficiently distinct from those given by scientists themselves and yet of sufficient interest to both scientists and readers not familiar with biology. In short, the observer's principle of organisation should provide an Ariadne's thread in a labyrinth of seeming chaos and confusion.

In this chapter, we follow the trials and tribulations of a fictional character, "the observer," in his attempts to use the notion of literary inscription² as a principle for organizing his initial observations of the laboratory.

Literary Inscription

Although our observer shares the same broad cultural knowledge as scientists, he has never seen a laboratory before and has no knowledge of the particular field within which laboratory members are working. He is enough of an insider to understand the general purpose of walls, chairs, coats, and so on, but not enough to know what terms like TRF, Hemoglobin, and "buffer" mean. Even without knowledge of these terms, however, he can not fail to note the striking distinction between two areas of the laboratory. One area of the laboratory (section B on Figure 2.1) contains various items of apparatus, while the other (section A) contains only books, dictionaries, and papers. Whereas in section B individuals work with apparatus in a variety of ways: they can be seen to be cutting, sewing, mixing, shaking, screwing, marking, and so on; individuals in section A work with written materials: either reading, writing, or typing. Furthermore, although occupants of section A, who do not wear white coats, spend long periods of time with their white-coated colleagues in section B, the reverse is seldom the case. Individuals referred to as doctors read and write in offices in section A while other staff, known as technicians, spend most of their time handling equipment in section B.

Each of sections A and B can be further subdivided. Section B appears to comprise two quite separate wings: in the wing referred to by participants as the "physiology side" there are both animals and apparatus; in the "chemistry side" there are no animals. The people from one wing rarely go into the other. Section A can also be subdivided. On the one hand, there are people who write and engage in telephone conversations; on the other hand, there are those who type and dial telephone calls. This division, like the others, is marked by partitions. In one area (the library) eight offices surround the perimeter of a conference room with table, chairs, and a screen. In the other area ("the secretariat") there are typewriters and people controlling the flow of telephone calls and mail.

What is the relationship between section A ("my office," "the office," "the library") and section B ("the bench")? Consulting the

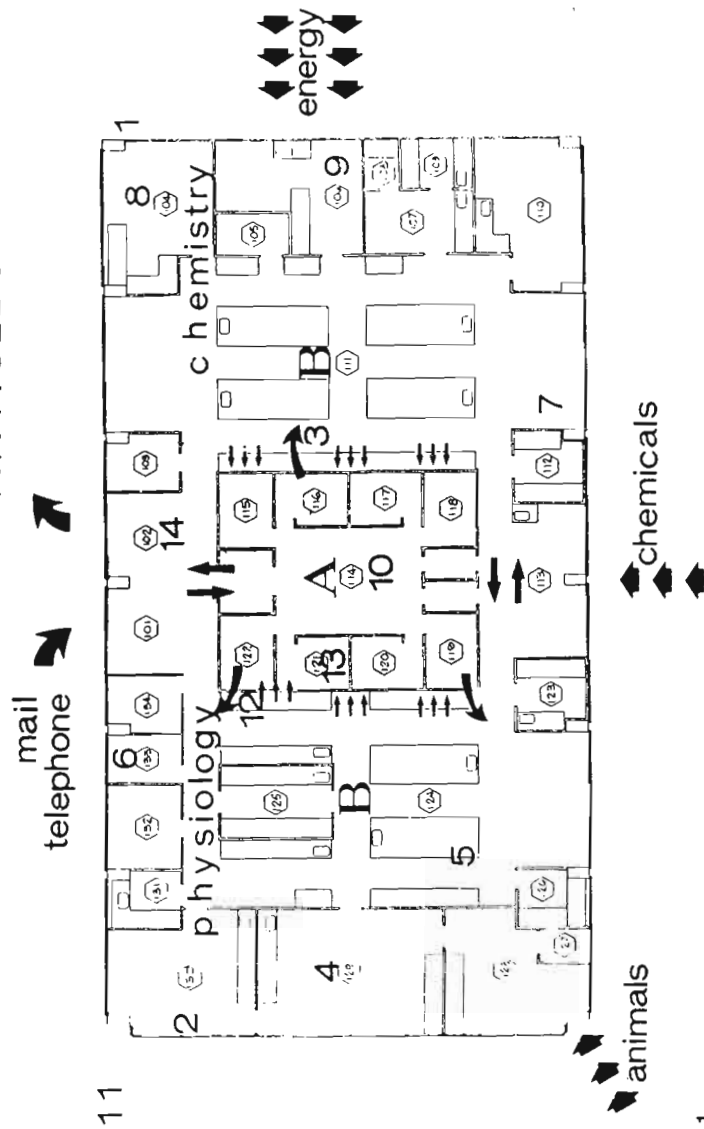


Figure 2.1
Map of the laboratory showing partitions and the main flows described in the text. The numbers on the map correspond to photographs in the file (page 91). The map shows the extent to which the differences between section A and B, and between the chemistry and physiology wings, are reinforced by the architectural layout of the laboratory.

map he has drawn, our observer tries to imagine another institution or setting with a similar division. It is hard to call to mind any factory or administrative organisation which has a similar set up. If, for example, it was a factory, we might expect the office space (section A) to be much smaller. If it was some kind of administrative agency, the bench space (section B) would be entirely superfluous. Although the relation between the two wings of the office space is common to many productive units, the special relation between office space and bench space is sufficient to distinguish the laboratory from other productive units. This is apparent on two counts. Firstly, at the end of each day, technicians bring piles of documents from the bench space through to the office space. In a factory we might expect these to be reports of what has been processed and manufactured. For members of this laboratory, however, these documents constitute what is yet to be processed and manufactured. Secondly, secretaries post off papers from the laboratory at an average rate of one every ten days. However, far from being *reports* of what has been produced in the factory, members take these papers to be the *product* of their unusual factory. Surely, then, if this unit merely processes paper work, it must be some sort of administrative agency? Not so: even a cursory look at the papers shows that the figures and diagrams which they contain are the very same documents produced in section B a few days or weeks previously.

It occurs to our observer that he might be able to make sense of laboratory activity according to one very simple principle. For him, the scene shown in Photograph 13,³ represents the prototype of scientific work in the laboratory: a desk belonging to one of the inhabitants of the office space (referred to as the doctors) is covered with paperwork. On the left is an opened issue of *Science*. To the right is a diagram which represents a tidied or summarised version of data sheets lying further to the right. *It is as if two types of literature are being juxtaposed*: one type is printed and published outside the laboratory; the other type comprises documents produced within the laboratory, such as hastily drawn diagrams and files containing pages of figures. Beneath the documents at the centre of the desk lies a draft. Just like the drafts of a novel or a report, this draft is scribbled, its pages heavy with corrections, question marks, and alterations. Unlike most novels however, the text of the draft is peppered with references, either to other papers, or to diagrams, tables or documents ("as shown in figure . . .," "in table . . . we can see that . . ."). Closer inspection of

the material lying on the desk (Photograph 13) reveals, for example, that the opened issue of *Science* is cited in the draft. Part of the argument contained in a *Science* article is said in the draft to be unrepeatable by virtue of what is contained in documents lying to the right of the desk. These documents are also cited in the draft. The desk thus appears to be the hub of our productive unit. For it is here that new drafts are constructed by the juxtaposition of two sources of literature, one originating outside and the other being generated within the laboratory.

It is no surprise to our observer to learn that scientists read published material. What surprises him more is that a vast body of literature emanates from within the laboratory. How is it that the costly apparatus, animals, chemicals, and activities of the bench space combine to produce a written document, and why are these documents so highly valued by participants?

After several further excursions into the bench space, it strikes our observer that its members are compulsive and almost manic writers. Every bench has a large leatherbound book in which members meticulously record what they have just done against a certain code number. This appears strange because our observer has only witnessed such diffidence in memory in the work of a few particularly scrupulous novelists. It seems that whenever technicians are not actually handling complicated pieces of apparatus, they are filling in blank sheets with long lists of figures; when they are not writing on pieces of paper, they spend considerable time writing numbers on the sides of hundreds of tubes, or pencilling large numbers on the fur of rats. Sometimes they use coloured papertape to mark beakers or to index different rows on the glossy surface of a surgical table. The result of this strange mania for inscription is the proliferation of files, documents, and dictionaries. Thus, in addition to the Oxford dictionary and the dictionary of known peptides, we can also find what might be called material dictionaries. For example, Photograph 2 shows a refrigerator which houses racks of samples, each of which bears a label with a ten-figure code number. Similarly, in another part of the laboratory, a vast supply of chemicals has been arranged in alphabetical order on shelves from which technicians can select and make use of appropriate substances. A more obvious example of these material dictionaries is the collection of preprints (Photograph 14, background) and thousands of files full of data sheets, each of which also has its own code number. Quite apart from these labelled and indexed collections

is the kind of paperwork (such as invoices, pay cheques, inventory schedules, mail files, and so on) which can be found in most modern productive units.

When the observer moves from the bench space to the office space, he is greeted with yet more writing. Xeroxed copies of articles, with words underlined and exclamation marks in the margins, are everywhere. Drafts of articles in preparation intermingle with diagrams scribbled on scrap paper, letters from colleagues and reams of paper spewed out by the computer in the next room; pages cut from articles are glued to other pages; excerpts from draft paragraphs change hands between colleagues while more advanced drafts pass from office to office being altered constantly, retyped, recorrected, and eventually crushed into the format of this or that journal. When not writing, the occupants of section A scribble on blackboards (Photograph 10) or dictate letters, or prepare slides for their next talk.

Our anthropological observer is thus confronted with a strange tribe who spend the greatest part of their day coding, marking, altering, correcting, reading, and writing. What then is the significance of those activities which are apparently not related to the marking, writing, coding, and correcting? Photograph 4, for example, shows two young women handling some rats. Despite the protocol sheet to the right, the numbered tubes on the rack and the clock in the foreground which controls the rhythm of the assay, the women themselves are neither writing nor reading. The woman on the left is injecting a liquid with a syringe and withdrawing another liquid with another syringe which she then passes on to the other woman; the second woman then empties the syringe into a tube. It is only then that writing takes over: the time and tube number is carefully recorded. In the meantime animals have been killed and various materials, such as ether, cotton, pipettes, syringes, and tubes have been used. What then is the point of killing these animals? How does the consumption of materials relate to the writing activity? Even the careful monitoring of the contents of the rack (Photograph 5) makes the situation no clearer to our observer. Over a period of several days, tubes are arranged in rows, other liquids are added, the mixtures are shaken and eventually removed for refrigeration.

Periodically, the routine of manipulation and rearrangement of tubes is interrupted. The samples extracted from rats are put into one of the pieces of apparatus and undergo a radical transformation: instead of modifying or labelling the samples, the machine produces a sheet of figures (Photograph 6). One of the participants tears the

sheet from the machine's counter and, after scrutinising it carefully, arranges for the disposal of the tubes. In other words, the same tubes which had been carefully handled for a week, which had cost time and effort to the tune of several hundred dollars, were now regarded as worthless. The focus of attention shifted to a sheet of figures. Fortunately, our observer was quite used to finding such absurd and erratic behaviour in the subjects of his studies. Relatively unperturbed, therefore, he braced himself for his next surprise.

It was not long in coming. The sheet of figures, taken to be the end result of a long assay, was used as the input to a computer (Photograph 11). After a short time, the computer printed out a data sheet and it was this, rather than the original sheet of figures, which was regarded as the important product of the operation. The sheet of figures was merely filed alongside thousands like it in the library. Nor was the series of transformations yet complete. Photograph 12 shows a technician at work on several data sheets produced by the computer. Soon after this photograph was taken, she was called into one of the offices to show the product of her labours: a single elegant curve carefully drawn on graph paper. Once again, the focus of attention shifted: the computer data sheets were filed away and it was the peaks and slopes of the curve which excited comment from participants in their offices: "how striking," "a well differentiated peak," "it goes down quite fast," "this spot is not very different from this one." A few days later, the observer could see a neatly redrawn version of the same curve in a paper sent out for possible publication. If accepted, this same figure would be seen by others when they read the article and it was more than likely that the same figure would eventually sit on some other desk as part of a renewed process of literary juxtaposition and construction.

The whole series of transformations, between the rats from which samples are initially extracted and the curve which finally appears in publication, involves an enormous quantity of sophisticated apparatus (Photograph 8). By contrast with the expense and bulk of this apparatus, the end product is no more than a curve, a diagram, or a table of figures written on a frail sheet of paper. It is this document, however, which is scrutinised by participants for its "significance" and which is used as "evidence" in part of an argument or in an article. Thus, the main upshot of the prolonged series of transformations is a document which, as will become clear, is a crucial resource in the construction of a "substance." In some situations, this process is very much shorter. In the chemistry wing in particular, the use of certain

pieces of apparatus makes it easy to get the impression that substances directly provide their own "signatures" (Photograph 9). While participants in the office space struggle with the writing of new drafts, the laboratory around them is itself a hive of writing activity. Sections of muscle, light beams, even shreds of blotting paper activate various recording equipment. And the scientists themselves base their own writing on the written output of the recording equipment.

It is clear, then, that particular significance can be attached to the operation of apparatus which provides some kind of written output. Of course, there are various items of apparatus in the laboratory which do not have this function. Such "machines" transform matter between one state and another. Photograph 3, for example, shows a rotary evaporator, a centrifuge, a shaker, and a grinder. By contrast, a number of other items of apparatus, which we shall call "inscription devices,"⁴ transform pieces of matter into written documents. More exactly, an inscription device is any item of apparatus or particular configuration of such items which can transform a material substance into a figure or diagram which is directly usable by one of the members of the office space. As we shall see later, the particular arrangement of apparatus can have a vital significance for the production of a useful inscription. Furthermore, some of the components of such a configuration are of little consequence by themselves. For example, the counter shown in Photograph 6 is not itself an inscription device since its output is not directly usable in an argument. It does, however, form part of an inscription device known as a bioassay.⁵

An important consequence of this notion of inscription device is that inscriptions are regarded as having a direct relationship to "the original substance." The final diagram or curve thus provides the focus of discussion about properties of the substance. The intervening material activity and all aspects of what is often a prolonged and costly process are bracketed off in discussions about what the figure means. The process of writing articles about the substance thus takes the end diagram as a starting point. Within the office space, participants produce articles by comparing and contrasting such diagrams with other similar diagrams and with other articles in the published literature (see pp. 69-86).

At this point, the observer felt that the laboratory was by no means quite as confusing as he had first thought. It seemed that there might be an essential similarity between the inscription capabilities of apparatus, the manic passion for marking, coding, and filing, and the literary

skills of writing, persuasion, and discussion. Thus, the observer could even make sense of such obscure activities as a technician grinding the brains of rats, by realising that the eventual end product of such activity might be a highly valued diagram. Even the most complicated jumble of figures might eventually end up as part of some argument between "doctors." For the observer, then, the laboratory began to take on the appearance of a system of literary inscription.

From this perspective, many hitherto strange occurrences fell into place. Many other types of activity, although superficially unrelated to the literary theme, could be seen as means of obtaining inscriptions. For example, the energy inputs (Photograph 1) represented intermediary resources to be consumed in the process of ensuring that inscription devices functioned properly. By also taking into account the supply of animals and chemicals, it was clear that a cycle of production which ended in a small folder of figures might have cost several thousand dollars. Similarly, the technicians and doctors who comprised the work force represented one further kind of input necessary for the efficient operation of the inscription devices and for the production and dispatch of articles.

The central prominence of documents in our discussion so far contrasts markedly with a tendency evident in some sociology of science to stress the importance of informal communication in scientific activity. For example, it has been frequently noted that the communication of scientific information occurs predominantly through informal rather than formal channels (Garvey and Griffith, 1967; 1971). This is particularly likely where there exists a well-developed network of contacts as, for example, in an invisible college (Price, 1963; Crane, 1969; 1972). Proponents of this argument have often played down the role of formal communication channels in information transfer, choosing instead to explain their continued existence in terms of an arena for the establishment of priority and subsequent conferral of credit (Hagstrom, 1965). Observations of the present laboratory, however, indicate that some care needs to be exercised in interpreting the relative importance of different communication channels. We take formal communication to refer to highly structured and stylised reports epitomised by the published journal article. Almost without exception, every discussion and brief exchange observed in the laboratory centred around one or more items in the published literature (Latour, 1976). In other words, informal exchanges invariably focussed on the substance of formal communication. Later we shall

suggest that much informal communication in fact establishes its legitimacy by referring or pointing to published literature.

Every presentation and discussion of results entailed the manipulation either of slides, protocol sheets, papers, preprints, labels, or articles. Even the most informal exchanges constantly focussed either directly or indirectly on documents. Participants also indicated that their telephone conversations nearly always focussed on the discussion of documents; either on a possible collaboration in the writing of a paper, or on a paper which had been sent but which contained some ambiguity, or on some technique presented at a recent meeting. When there was no direct reference to a paper, the purpose of the call was often to announce or push a result due to be included in a paper currently being prepared. Even when they were not discussing a draft, individuals devoted considerable energy to devising ways of attaining some readable trace. In these kinds of discussions, scientists anticipated that possible objections to their argument might appear in some forthcoming paper. More important for the present, however, is the omnipresence of literature in the sense that we have defined it, that is, in terms of written documents, only a few of which appear in published form.

The Culture of the Laboratory

To those familiar with the work of the laboratory, the above account will have little to say that is new. For an anthropologist, however, the notion of literary inscription is still problematic. As we said earlier, our observer has an intermediary status: while the broad cultural values which he shares with the scientists facilitate some familiarity with the commonplace objects and events in the laboratory, he is unwilling solely to rely on scientists' own versions of the way the laboratory operates. One consequence of his intermediary status is that his account so far has failed to satisfy any one audience. It could be said, for example, that in portraying scientists as readers and writers he has said nothing of the *substance* of their reading and writing. Indeed, our observer incurred the considerable anger of members of the laboratory, who resented their representation as participants in some literary activity. In the first place, this failed to distinguish them from any other writers. Secondly, they felt that the important point was that they were writing *about* something, and that this something was "neuroendocrinology." Our observer experienced the depressing sensation that his Ariane's thread had led him up a blind alley.

ARTICLES ABOUT NEUROENDOCRINOLOGY

We noted earlier that participants made sense of their juxtaposition of literatures by reference to a world of literature published outside the laboratory. To the extent that such literature represents the scriptures (Knorr, 1978) from which participants take the sense of their activities, we can only begin to understand what the literature is about by close inspection of the mythology which informs their activities. Our use of the term mythology is not intended pejoratively. Rather, it refers to a broad frame of reference within which can be situated the activities and practices of a particular culture (Barthes, 1957).

Our observer noticed that when asked by a total stranger, members of the laboratory replied that they worked (or were) "in neuroendocrinology." They went on to explain that neuroendocrinology was the result of a hybridisation which had taken place in the 1940s between neurology, described as the science of the nervous system, and endocrinology, the science of the hormonal system. It occurred to our observer that such location "in a field" facilitated the correspondence between a particular group, network, or laboratory and a complex mixture of beliefs, habits, systematised knowledge, exemplary achievements, experimental practices, oral traditions, and craft skills. Although referred to as the "culture" in anthropology, this latter set of attributes is commonly subsumed under the term paradigm when applied to people calling themselves scientists.⁶ Neuroendocrinology seemed to have all the attributes of a mythology: it had had its precursors, its mythical founders, and its revolutions (Meites et al., 1975). In its simplest version, the mythology goes as follows: After World War II it was realised that nerve cells could also secrete hormones and that there is no nerve connection between brain and pituitary to bridge the gap between the central nervous system and the hormonal system. A competing perspective, designated the "hormonal feedback model" was roundly defeated after a long struggle by participants who are now regarded as veterans (Scharrer and Scharrer, 1963). As in many mythological versions of the scientific past, the struggle is now formulated in terms of a fight between abstract entities such as models and ideas. Consequently, present research appears based on one particular conceptual event, the explanation of which only merits scant elaboration by scientists. The following is a typical account: "In the 1950s there was a sudden crystallization of ideas, whereby a number of scattered and apparently unconnected results suddenly made sense and were intensely gathered and reviewed."

The mythology through which a culture represents itself is not necessarily entirely false. A count of publications, for example, shows that the growth of papers dealing with neuroendocrinology after 1950 was exponential, and that neuroendocrinology, which made up only 3 percent of endocrinology as a whole in 1968, grew to 6 percent by 1975. In broad outline, then, the growth of neuroendocrinology appears to have followed the pattern of what some sociologists of science have termed "scientific development" (for example, Crane, 1972; Mulkey et al., 1975). However, the mythology of its development is very rarely mentioned in the course of the day-to-day activities of members of the laboratory. The beliefs that are central to the mythology are noncontroversial and taken for granted, and only enjoy discussion during the brief guided tours of the laboratory provided for visiting laymen. In the setting, it is difficult to determine whether the mythology is never alluded to simply because it is a remote and unimportant remnant of the past or because it is now a well-known and generally accepted item of folklore.

After his first few days in the setting, our observer was no longer told about neuroendocrinology. Instead, daily concerns focussed on a different set of specific cultural values which, although from time to time talked about as being in neuroendocrinology, appeared to constitute a distinct culture (or "paradigm"). Our criteria for identifying this specific culture is not simply that a specialty represents a subset of a larger discipline. This would be no more accurate than considering the Bouarées' nations as a subset of the larger Boukara ethnical group. Instead, we use culture to refer to the set of arguments and beliefs to which there is a constant appeal in daily life and which is the object of all passions, fears, and respect. Participants in our laboratory said that they were dealing with "substances called releasing factors" (for popular accounts, see Guillemin and Burgus, 1972; Schally et al., 1973; Vale, 1976). When they presented their work to scientifically informed outsiders, they formulated their efforts as attempting "to isolate, characterize, synthesise and understand the modes of action of releasing factors." This is the brief that distinguishes them from their other colleagues in neuroendocrinology. It is also their cultural trait, their particularity, and their horizon of work and achievement. The general mythology provides them with the tenet that the brain controls the endocrine system, and they share this with a large cultural group of neuroendocrinologists. Specific to their own culture, however, is an additional postulate that "control by the brain

is mediated by discrete chemical substances, so called releasing factors, which are of a peptidic nature" (Meites, 1970).⁷ Their skills, working habits, and the apparatus at their disposal are all organised around one specific material (the hypothalamus), which is deemed especially important for the study of releasing factors.

Our observer can now picture his informants as readers and writers of Neuroendocrinological literature who acknowledge certain texts published in the previous five years as major achievements. These tests record the structure of several releasing factors in sentences comprising words or phonemes which relate to substances called amino acids. In general, the structure of any substance of a peptidic nature can be expressed in the form of a string of amino acids (for example, Tyr-Lys-Phe-Pro).⁸ The texts that specify the structure of the first releasing factors were considered major breakthroughs by all informants (see Chapter 3). "In 1969 we discovered the structure of the thyrotropin releasing factor"; in 1971 they discovered or confirmed the structure of another releasing factor known as LRF; in 1972 they discovered the structure of a third substance called somatostatin (for general accounts, see Wade, 1978; Donovan et al., forthcoming).

The importance of articles specifying the structure of releasing factors is shown by the number of other articles which resulted. Papers written by other informants constituted the outside literature used in conjunction with internally produced inscriptions to generate new papers. Figure 2.2 shows the relative boom in the number of papers dealing with various substances after the initial specification of structure in so-called breakthrough papers. As a result of these publication explosions, the proportion of releasing factor publications in neuroendocrinology rose from 17 percent in 1968 to 38 percent in 1975. This suggests that the releasing factor "specialty" was responsible for the general increase in the importance of neuroendocrinology as a whole. Because of burgeoning outside interests, the laboratory's share of publication in the specialty actually decreased as a result of its success, from 42 percent in 1968 to 7 percent in 1975.⁹ To put this in perspective, however, it is worth noting that in 1975 publications in releasing factors represented 39 percent of all publications in neuroendocrinology; neuroendocrinology represented only 6 percent of endocrinology as a whole, and endocrinology is only one of many disciplines within biology. Put another way, publications by members of the laboratory in 1975 represented only 0.045 percent of those in endocrinology. Clearly, some caution should be exercised in general-

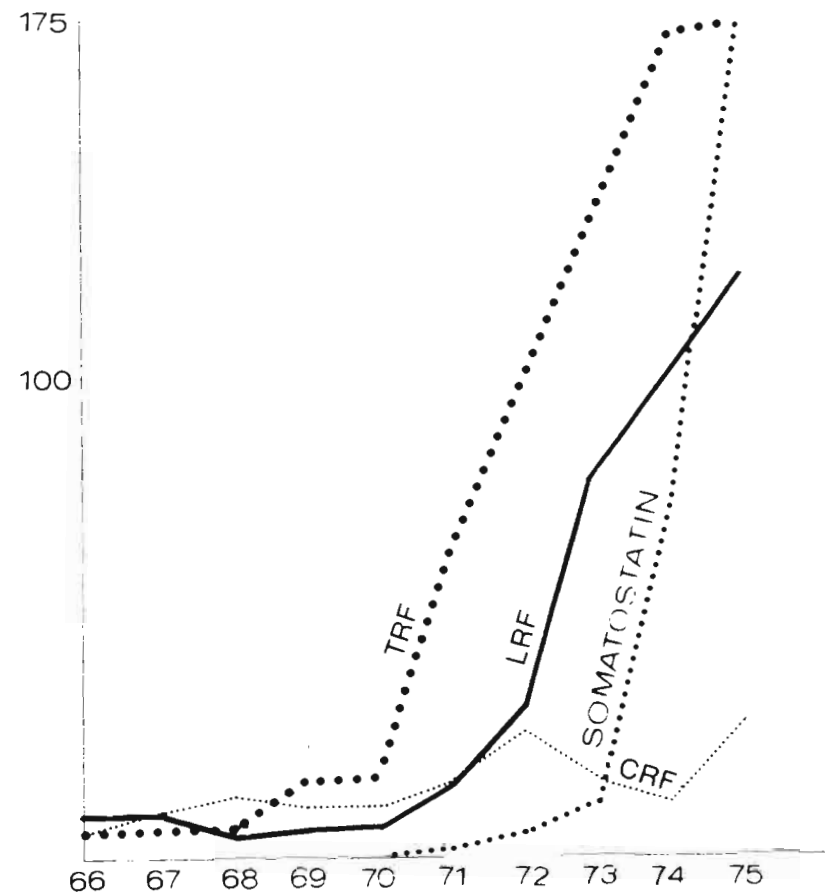


Figure 2.2

This diagram shows the number of articles published per year on each of four different releasing factors. The computation is based on the SCI, Permuterm and by combining the various spellings of releasing factors. The names chosen in this diagram are those used in the laboratory studied. TRF in 1970, LRF in 1971, and somatostatin in 1973, all show the same abrupt ascending curve. CRF, the structure of which is still unknown, is included for comparison.

ising the characteristics of scientific activity on the basis of this one laboratory.

So far we have said that each inscription device comprises a particular combination of machines, pieces of apparatus, and technicians. Articles are written on the basis of specific flow of outside literature, and with the use (either implicit or explicit) of part of the archives in the laboratory. These archives comprise a wide range of "material dictionaries," brain extracts, for example, as well as protocol books. Our observer should now be able to discern several distinct lines of activity in the laboratory, each of which corresponds to a specific type of article which is finally produced. For each type he should be able to identify the individuals concerned, their location in the laboratory, the technicians who assist, the inscription devices employed, and the type of outside literature to which their work relates. Three main lines of article production, referred to by participants as "programmes," could be clearly differentiated at the time of the study. As can be seen from Table 2.1, they do not contribute equally to the overall output of the laboratory, nor do they have the same cost and subsequent impact. By examining the three programmes in some detail, our observer hoped to be able to specify which characteristics of activity were peculiar to this laboratory.

The first type of article written in this laboratory concerned new natural substances in the hypothalamus (see Chapter 3). A substance is obtained by superimposing two sets of inscriptions, one from a recording device known as an assay in the physiology side of the laboratory and the other from "purification cycles" carried out in the chemistry side. Since the assay and purification cycle are inscription devices common to all three programmes, we shall describe them in some detail.

Despite the many different types of activity referred to as assays (for example, the bioassay, the *in vitro* and *in vivo* assays, direct or indirect assays, radioimmunological or biological assays) they are all based on the same principle (Rodgers, 1974). A recording mechanism (such as a myograph, a gamma counter, or a simple rating sheet) is connected up to an organism (either a cell, a muscle, or a whole animal) so as to produce an easily readable trace. A substance with a known effect on the organism is then administered to the organism as a control. The effect on the organism is inscribed and its recorded trace is taken as a baseline. An unknown substance is then administered and its effect recorded. The result is a recorded *difference* between two traces, a

Table 2.1

First Programme (isolation of new substance)	31 papers	15% of total	24 c.p.i.
Second Programme: Total (analog and functions)	78 papers	37% of total	—
Task One (analog)	—	—	—
Task Two (structure function)	52 papers	24% of total	7.6 c.p.i.
Task Three (clinical)	19 papers	9% of total	21 c.p.i.
Task Four (basic chemistry)	7 papers	3% of total	7.2 c.p.i.
Third Programme (mode of actions)	47 papers	22% of total	10.6 c.p.i.
Technical Papers	20 papers	9% of total	7 c.p.i.
General Articles	27 papers	13% of total	9 c.p.i.
Others	10 papers	5% of total	—
Total Mean	213 papers		12.4 c.p.i.

difference about which simple perceptive judgments ("it is the same," "it goes up," "there is a peak") can be made. If there is a difference, it is taken as the sign of an "activity" in the unknown substance. Since the central objective of the culture is to define any activity in terms of a discrete chemical entity, the unknown substance is taken to the other side of the laboratory for tests in the second main type of inscription device, the purification cycle.

The goal of the purification cycle is to isolate the entity which is believed to have caused the recorded difference between two traces. Samples of brain extract are subjected to a series of *discriminations* (Anonymous, 1974). This entails the use of some stationary material (such as a gel or a piece of blotting paper) as a selective sift which

delays the gradual movement of a sample of brain extract. (This movement can be variously due to gravity, electric forces, or cellular binding—Heftmann, 1967.) As a result of this process, samples are transformed into a large number of fractions, each of which can be scrutinised for physical properties of interest. The results are recorded in the form of several peaks on graph paper. Each of these peaks represents a discriminated fraction, one of which may correspond to the discrete chemical entity which caused an activity in the assay. In order to discover whether the entity is present, the fractions are taken back to the physiology section of the laboratory and again take part in an assay. By superimposing the result of this last assay with the result of the previous purification, it is possible to see an overlap between one peak and another. If the overlap can be repeated, the chemical fraction is referred to as a "substance" and is given a name.

Ideally, this shuttle between the assay (Photograph 4) and the purification cycle (Photograph 7) ends with the identification of an "isolated" substance. This is almost never the case, however, because most of the differences between activities in the assay disappear when the assay is repeated. The postulated substance CRF, for example, has been shuttling to and fro in six laboratories since 1954 (cf., Figure 2.2). Even when differences between activities do not disappear, the entity can often no longer be traced after a few steps of purification. As we shall see later, the elimination of these elusive and transitory substances (known as "artefacts") is the main concern of the tribe. Although the details of the elimination process are extremely complex, the general principle is simple.

Since most competitors' claims to have an "isolated" substance are put into quotation marks, it follows that the assertion that an entity is "isolated" depends primarily on the operation of local criteria. When this claim has been made within the laboratory, the chemical fraction breaks out of the shuttle between assay and purification and switches to another circuit of operations. This new circuit comprises an inscription device known as an Amino Acid Analyser (AAA), which automatically records the effects of the isolated sample on a series of other chemical "reagents" and allows this effect to be directly read in terms of certain letters of the amino acid vocabulary. Thus, the inscription of the substance is decipherable in letters, such as, for example, Glu, Pyro, His, rather than just in terms of peaks, spots, and slopes. However, this is not the end of the matter. By this stage, each component amino acid is known; but the particular order of the amino

acids has not yet been determined. To do this, the previous samples are taken to another room, where there are expensive inscription devices handled by full-time "PhD holders." The two main inscription devices, the "mass spectrometer" and the "Edmann degradation sequence," provide written spectra and diagrams which allow the specification of the configuration of amino acids which are present in the substance. These are great and rare moments in the work of the first programme. The determination of structure constitutes the most exciting and exhausting periods of work, which are remembered vividly by participants many years after. In the next chapter we shall follow the history of one of these substances in detail and return to a closer explanation of the activities mentioned here.

The concern of a *second* main programme in the laboratory is to reconstruct substances (whose structure has already been determined), using amino acids supplied by the chemical industry, and to evaluate their activity. The main objective of this programme is to produce artificially reconstructed substances, known as analogs, with properties which, because they are different from the original substances, will facilitate their use in medicine or physiology. The second research programme can be divided into four tasks.¹⁰ The first task is the chemical production of analogs. Instead of buying analogs or obtaining them from another investigator, the laboratory can supply substances relatively cheaply in its own inhouse chemical section. The production of analogs is largely mechanised, using apparatus such as the peptide automatic synthesizer. Many of the analytical inscription devices (such as the mass spectrometer, the amino acid analyser, or the nuclear magnetic resonance spectrometer) which are used in the original purification of a substance are also used in its artificial reconstruction. In the second programme, however, these inscription devices are used to monitor the reconstruction process rather than to produce new information. The second task concerns so-called "structure function relationships." Using a number of slightly different analogs, physiologists try to identify connections between bioassay effects and combinations of analogs which give rise to them. For example, the natural substance which inhibits the release of a substance called growth hormone, is a fourteen amino acid structure. By substituting a right-handed form for the left-handed form of the amino acid at the eighth position, a more potent substance is obtained. This has major implications for the treatment of diabetes. Consequently, the outcome of these kinds of trial and error operations, which

make up 24 percent of published papers, are of special interest to funding agencies and to the chemical industry (Latour and Rivier, 1977). A third task, which makes up to 9 percent of published papers, concerns the determination of structure function relationships in the effect of substances on humans. Most of the papers which result from this work are written in collaboration with clinicians. The aim is to devise analogs which most nearly match the natural substances required for clinical purposes. It would be desirable, for example, to devise an analog of LRF which would inhibit the release of LH instead of triggering it. This would make possible the production of a much better contraceptive pill than at present and thus represents a highly prized (and highly funded) research objective. The fourth and last task, which makes up only 3 percent of total research output, comprises research in collaboration with fundamental chemists on the configuration of molecules which make up the substance. The role of the laboratory in this work is mainly the provision of material, but the results are nevertheless very important for studies of "structure-function relationships."¹¹ As in the third task, first authors of papers resulting from this fourth task are based outside the laboratory.

So far we have discussed two main programmes: the isolation of new natural substances on the one hand and their reproduction by synthesis on the other. A *third* programme is said by participants to be aimed at understanding the mechanisms by which different substances interact. This work is carried out in the physiology section of the laboratory using bioassays. A variety of different trails, ranging from those generating crude behavioural responses to those which record the rate of DNA synthesis following hormonal contact, are used to try and assess how substances react together.

In terms of published papers, these three programmes accounted respectively for 15 percent, 37 percent, and 22 percent of the total output from the laboratory between 1970 and 1976. It is rarely the case, however, that participants refer to the programme in which they are working. The specification and particular arrangement of apparatus does not in itself correspond to the self-perceptions of work which they hold. Rather than saying, "I am doing purification," for example, they are much more likely to say, "I am purifying substance X." It is not purification in general which concerns them, but "the isolation of CRF"; it is not the synthesis of analogs, but the study of "D TRP 8 SS." Furthermore, objectives of each programme change in the course of a few months. Our notion of programme is thus

inadequate in that it is merely an intermediary device which our observer has used in becoming familiar with his setting. On the other hand, our observer now knows what distinguishes this laboratory from others and which papers are written on the basis of particular combinations of staff and inscription devices. We reserve for later discussion an appraisal of laboratory activity in terms of specific individuals, careers, historical periods, and items of apparatus.

THE "PHENOMENOTECHNIQUE"

We have so far related how our observer apprehended the laboratory in terms of the prevalence of written documents and of inscription devices. In particular, the notion of literature provided an organising principle with which the observer could make sense of his observations without relying solely on participants' accounts. "Literature" refers both to the central importance accorded a variety of documents and to the use of equipment to produce inscriptions which are taken to be about a substance, and which are themselves used in the further generation of articles and papers. In order to explicate the notion of literary inscription as applied to apparatus, we shall provide an inventory of the material setting of the laboratory.

One important feature of the use of inscription devices in the laboratory is that once the end product, an inscription, is available, all the intermediary steps which made its production possible are forgotten. The diagram or sheet of figures becomes the focus of discussion between participants, and the material processes which gave rise to it are either forgotten or taken for granted as being merely technical matters.¹² A first consequence of the relegation of material processes to the realm of the merely technical is that inscriptions are seen as direct indicators of the substance under study. Especially in apparatus such as the amino acid analyser (Photograph 9), the substance appears to inscribe its own signature (Spackmann et al., 1958). A second consequence, however, is the tendency to think of the inscription in terms of confirmation, or evidence for or against, particular ideas, concepts, or theories.¹³ There thus occurs a transformation of the simple end product of inscription into the terms of the mythology which informs participants' activities. A particular curve, for example, might constitute a breakthrough; or a sheet of figures can count as clear support for some previously postulated theory.

As we have already indicated, however, the cultural specificity of the laboratory does not reside in the mythology available to partici-

pants. After all, similar mythologies are available in other laboratories. Specific to this laboratory is the particular configurations of apparatus that we have called inscription devices. The central importance of this material arrangement is that none of the phenomena "about which" participants talk could exist without it. Without a bioassay, for example, a substance could not be said to exist. The bioassay is not merely a means of obtaining some independently given entity; the bioassay constitutes the construction of the substance. Similarly, a substance could not be said to exist without fractionating columns (Photograph 7), since a fraction only exists by virtue of the process of discrimination. Likewise, the spectrum produced by a nuclear magnetic resonance (NMR) spectrometer (Photograph 8) would not exist but for the spectrometer. It is not simply that phenomena *depend on* certain material instrumentation; rather, the phenomena *are thoroughly constituted by* the material setting of the laboratory. The artificial reality, which participants describe in terms of an objective entity, has in fact been constructed by the use of inscription devices. (Such a reality, which Bachelard (1953) terms the "phenomeno-technique," takes on the appearance of a phenomenon by virtue of its construction through material techniques.

It follows that if our observer was to imagine the removal of certain items of equipment from the laboratory, this would entail the removal of at least one object of reality from discussion. This was particularly apparent whenever equipment broke down or whenever new equipment was brought into the laboratory.¹⁴ Obviously, however, not all pieces of equipment condition the existence of phenomena and the production of papers in the same way. Taking away the trash can, for example, would be unlikely to harm the main research process; similarly, withdrawal of the automatic pipette would not prevent pipetting by hand, even though this takes longer. By contrast, if the gamma counter breaks down, it is difficult to measure amounts of radioactivity merely by sight! The observation of radioactivity is entirely dependent on the counter (Yalow and Berson, 1971). Clearly, the laboratory would stop operating without the pipes carrying water and oxygen which run between the laboratory and the plant (Photograph 1), but they do not account for the fact that the laboratory produces papers. Like Aristotle's notion of vegetative life, these pipes are a general condition of a superior life but they do not explain it. However, whereas Photograph 1 could have been taken in any factory setting, Photograph 3 is, by contrast, peculiar to a laboratory. This is

because apart from the hair dryer, electric motor, and two hydrogen bottles, all the other pieces of apparatus were invented specifically to assist in the construction of laboratory objects. The centrifuge (on the left of Photograph 3), for example, was devised by Svedberg in 1924 and was responsible for creating the notion of protein by allowing undifferentiated substances to be discriminated by spinning (Pedersen, 1974). The molecular weight of proteins could hardly be said to exist except by virtue of the ultracentrifuge. The Rotary Evaporator (on the right of Photograph 3), invented by Craig at the Rockefeller Institute in 1950 (Moore, 1975), enables the removal of solvents in most laboratory purification processes and superceded the previous use of the Claisen flask.

It is clear, then, that some items of equipment are more crucial to the research process than others. Indeed the strength of the laboratory depends not so much on the availability of apparatus, but on the presence of a particular configuration of machines specifically tailored for a particular task. Photograph 3 does not define the particular field in which the work of the laboratory is situated because centrifuges and rotary evaporators can be found in a wide variety of biologically inclined research institutions. However, the presence of bio- and radioimmuno-assays, the Sephadex columns, and the whole gamut of spectrometers, show that participants are concerned with neuro-endocrinology. A whole range of inscription devices, variously used to make points in different subfields, have been assembled in one place. The mass spectrometer, for example, is used in the production of papers on the structure of a substance; cell cultures are used to make points about the synthesis of DNA in the biosynthesis of the same substances.

The cultural specificity of the laboratory is also evident from the fact that some of its inscription devices can only be found in this setting. Most of the substances depend for their existence on bio- and radioimmuno-assays. Each assay comprises several hundred sequences, and sometimes occupies two or three people full time for several days or weeks at a stretch. The instructions for one assay (the TRF immunoassay) occupy six full pages and read like a complicated recipe. Since only relatively small steps, such as pipetting, can be automated, the process relies heavily on the routinised skills of the technicians. As a whole, the assay is an idiosyncratic process in that it depends on the skills of individual technicians and on the use of particular antisera, which themselves have to be obtained from

particular goats at particular times of the year. This is why so many substances exist only *locally* (see Chapter 4). The presence in this setting of what scientists refer to as "an exquisite bioassay for growth hormones" or of a "very sensitive assay for CRF" is highly valued by members, and is the source both of their pride as well as the points they make in the literature.

It would be wrong to contrast the material with conceptual components of laboratory activity. The inscription devices, skills, and machines which are now current have often featured in the past literature of *another field*. Thus, each sequence of actions and each routinised assay has at some stage featured as the object of debate in another field and has been the focus of several published papers. The apparatus and craft skills present in one field thus embody the end results of debate or controversy in some other field and make these results available within the walls of the laboratory. It is in this sense that Bachelard (1953) referred to apparatus as "reified theory." The inscription device provides inscriptions which can be used to write papers or to make points in the literature on the basis of a transformation of established arguments into items of apparatus. This transformation, in turn, allows the generation of new inscriptions, new arguments and potentially new items of apparatus (cf., Chapter 6). When, for example, a member of the laboratory uses a computer console (Photograph 11), he mobilises the power of both electronics and statistics. When another member handles the NMR spectrometer (Photograph 8) to check the purity of his compounds, he is utilising spin theory and the outcome of some twenty years of basic physics research. Although Albert knows little more than the general principles of spin theory, this is sufficient to enable him to handle the switchboard of the NMR and to have the power of the theory working to his advantage. When others discuss the spatial structure of a releasing factor, they implicitly make use of decades of research in elementary chemistry. Similarly, a few principles of immunology and a general knowledge of radioactivity are sufficient to benefit from these two sciences when using the radioimmunoassay in the quest for a new substance (Yalow and Berson, 1971). Every move in the laboratory thus relies in some way on other scientific fields. In Table 2.2 we list some of the larger items of equipment used in the laboratory, together with the field of origin and the date at which they were imported into the new problem area. In the next chapter we shall see why much of this equipment originated in fields thought to be "harder" than endocrinology.

Table 2.2

Name of Instrument	Date of First Conception	Date of First Introduction	Field of Origin	Usage in the Program	Remarks
Mass spectrophotometer	1910-1924	1959 for peptides, 1969 for releasing factors	physics (isotopes)	first programme	one PhD to operate it; takes one room
Nuclear magnetic resonance (high resolution) spectrometer	1937-1954	1957 for peptides (pep.), 1964 for releasing factors (R.F.)	physics (spin)	second programme, task one	used to check purity
Amino acid analyzer	1950-1954	within peptide chemistry	protein chemistry; analytic	first and second programmes	routine; machine; automatised
Peptide automatic synthesizer	1966	within pep. for R.F. 1975	biochemistry; synthetic	second programme, task one	routine; machine; automatised, new
Sephadex columns	1956-1959	1960-1962 for R.F.		first, second, and third programmes	essential part of purification and assays
Radioimmunoassay	1956-1960	1959 for pep.	nuclear physics; immunology; endocrinology	all programmes	most versatile and labour intensive instrument
High pressure liquid chromatograph	1958-1967	1973 for pep. 1975 for R.F.	analytical chemistry	first programme and second programme, task one	new, transformed in routine task
Countercurrent distribution chromatograph	1943-1947	1958 for R.F.	"	"	cold piece of machinery

Since the material setting represents the reification of knowledge established in the literature of another field, there is necessarily a time lag between the discussion of a theory in one field and the appearance of a corresponding technique in another. This is confirmed by the dates of first conception of various inscription devices. In general, inscription devices were derived from a well-established body of knowledge. Chromatography, for example, is still an active research area of chemistry. But the chromatography embodied in apparatus used in the laboratory dates from Porath's work in the 1950s (Porath, 1967). The mass spectrometer, a crucial analytical tool, is based on physics which is some fifty years old (Beynon, 1960). The same is the case for the laboratory's use of statistics and programming techniques. By borrowing well-established knowledge, and by incorporating it in pieces of furniture or in routine operational sequences, the laboratory can harness the enormous power of tens of other fields for its own purposes.

However, the accumulation of material theories and practices from other fields itself depends on certain manufacturing skills. For example, the mere existence of a discipline such as nuclear physics does not in itself ensure the presence of a beta-counter in the laboratory. Clearly, the use of such equipment presupposes their manufacture. Without Merrifield's invention, for example, there would be no solid phase synthesis and no way of automating peptide synthesis (Merrifield, 1965; 1968). But even without a company like Beckmann, there would still be a prototype at the Rockefeller Institute where it was invented and this could be used by other scientists. Apart from the automatic pipette, a simple time-saving device, both the principle and basic prototype of all the other apparatus used in the laboratory originated in other scientific laboratories. However, industry plays an important role in designing, developing, and making these scientific prototypes available to a larger public, as is clear if we imagine that there were only one or two existing prototypes of each item of new equipment. In this case, scientists would have to travel vast distances and there would be a dramatic fall in the rate of production of papers. The transformation of Merrifield's original prototype into the marketable, self-contained, reliable, and compact item of equipment sold under the name of Automatic Peptide Synthesizer, is a measure of the debt of the laboratory to technological skills (Anonymous, 1976a). If inscription devices are the reification of theories and practices, the actual pieces of equipment are the marketed forms of these reifications.

The material layout of the laboratory has been constructed from items of apparatus, many of which have long and sometimes controversial histories. Each item of apparatus has combined with certain skills to form specific devices, the styluses and needles of which scratch the surface of sheets of graph paper. The string of events to which each curve owes its very existence is too long for any observer, technician, or scientist to remember. And yet each step is crucial, for its omission or mishandling can nullify the entire process. Instead of a "nice curve," it is all too easy to obtain a chaotic scattering of random points of curves which cannot be replicated. To counter these catastrophic possibilities, efforts are made to routinise component actions either through technicians' training or by automation. Once a string of operations has been routinised, one can look at the figures obtained and quietly forget that immunology, atomic physics, statistics, and electronics actually made this figure possible. Once the data sheet has been taken to the office for discussion, one can forget the several weeks of work by technicians and the hundreds of dollars which have gone into its production. After the paper which incorporates these figures has been written, and the main result of the paper has been embodied in some new inscription device, it is easy to forget that the construction of the paper depended on material factors. The bench space will be forgotten, and the existence of laboratories will fade from consideration. Instead, "ideas," "theories," and "reasons" will take their place. Inscription devices thus appear to be valued on the basis of the extent to which they facilitate a swift transition from craft work to ideas. The material setting both makes possible the phenomena and is required to be easily forgotten. Without the material environment of the laboratory none of the objects could be said to exist, and yet the material environment very rarely receives mention. It is this paradox, which is an essential feature of science, that we shall now consider in more detail.

Documents and Facts

Thus far, our observer has begun to make sense of the laboratory in terms of a tribe of readers and writers who spend two-thirds of their time working with large inscription devices. They appear to have developed considerable skills in setting up devices which can pin down elusive figures, traces, or inscriptions in their craftwork, and in the art of persuasion. The latter skill enables them to convince others that

what they do is important, that what they say is true, and that their proposals are worth funding. They are so skillful, indeed, that they manage to convince others not that they are being convinced but that they are simply following a consistent line of interpretation of available evidence. Others are persuaded that they are not persuaded, that no mediations intercede between what is said and the truth. They are so persuasive, in fact, that within the confines of their laboratory it is possible to forget the material dimensions of the laboratory, the bench work, and the influence of the past, and to focus only on the "facts" that are being pointed out. Not surprisingly, our anthropological observer experienced some dis-ease in handling such a tribe. Whereas other tribes believe in gods or complicated mythologies, the members of this tribe insist that their activity is in no way to be associated with beliefs, a culture, or a mythology. Instead, they claim to be concerned only with "hard facts." The observer is puzzled precisely because his informants insist that everything is straightforward. Moreover, they argue that if he were a scientist himself, he would understand this. Our anthropologist is sorely tempted by this argument. He has begun to learn about the laboratory, he has read lots of papers and can recognise different substances. Furthermore, he begins to understand fragments of conversation between members. His informants begin to sway him. He begins to admit that there is nothing strange about this setting and nothing which requires explanation in terms other than those of informants' own accounts. However, in the back of his mind there remains a nagging question. How can we account for the fact that in any one year, approximately one and a half million dollars is spent to enable twenty-five people to produce forty papers?

Apart from the papers themselves, of course, another kind of product provides the means for generating documents in other laboratories. As we said above, two of the main objectives of this laboratory are the purification of natural substances and the manufacture of analogs of known substances. Frequently, purified fractions and samples of synthetic substances are sent to investigators in other laboratories. Each analog is produced at an average cost of \$1,500, or \$10 per milligram, which is much lower than the market value of these peptides. Indeed, the market value of all peptides produced by the laboratory would amount to \$1.5 million, the same as the total budget of the laboratory. In other words, the laboratory could pay for its research by selling its analogs. However, the quantities, the number,

and the nature of the peptides actually produced by the laboratory are such that there is no market for 99 percent of them. Moreover, nearly all the peptides (90 percent) are manufactured for internal consumption and are not available as output. The actual output (for example, 3.2 grams in 1976) is potentially worth \$130,000 at market value, and although it cost only \$30,000 to produce, samples are sent free of charge to outside researchers who have been able to convince one of the members of the laboratory that his or her research is of interest. Although members of the laboratory do not require their names to appear on papers which report work resulting from the use of these samples, the ability to provide rare and costly analogs is a powerful resource. If, for example, only a few micrograms were made available, this would effectively prevent the recipient from carrying out sufficient investigations to make a discovery (see Chapter 4).¹⁵ Purified substances and rare antisera are also considered valuable assets. When, for example, a participant talks about leaving the group, he often expresses concern about the fate of the antiseras, fractions, and samples for which he has been responsible. It is these, together with the papers he has produced, that represent the riches needed by a participant to enable him to settle elsewhere and write further papers. He is likely to find similar inscription devices elsewhere, but not the idiosyncratic antisera that permit a specific radioimmunoassay to be run. Besides samples, the laboratory also produces skills in the members of a workforce who from time to time leave the laboratory to work elsewhere. Here again, the skill is only a means to the end of publishing a paper.

The production of papers is acknowledged by participants as the main objective of their activity. The realisation of this objective necessitates a chain of writing operations from a result first scribbled on a sheet of paper and enthusiastically communicated to colleagues, to the final registering of published literature in the laboratory archives. The many intermediary stages (such as talks with slides, circulation of preprints, and so on) all concern literary production of one kind or another. It is thus necessary carefully to study the various processes of literary production which lead to the output of papers. We shall do this in two ways. Firstly, we shall consider papers as objects in much the same way as manufactured goods. Secondly, we shall attempt to make sense of the content of papers. By looking at literary production in this way we hope to broach the central questions posed by our observer: how can a paper be both so expensive to produce and

yet so highly valued? What exactly can justify participants' faith in the importance of the papers' contents?

THE PUBLICATION LIST

The range and scope of papers produced by the laboratory is indicated by a list kept and updated by participants. We used those items listed between 1970 and 1976. Although referred to by participants as the "publication list," a number of articles were included which had not in fact been published.¹⁶

Let us classify output according to the channel chosen by investigators. Fifty percent consisted of "regular" papers. Such items comprised several pages and were published in professional journals. Twenty percent of the output comprised abstracts submitted to professional congresses. A further 16 percent comprised solicited contributions to meetings, only half of which found their way into print as conference proceedings. Participants also contributed chapters to edited collections of papers, which made up 14 percent of the total output.

Another way of classifying papers is by the literary "genre" of articles. Differences in genre were defined both in terms of formal characteristics (such as the size, style, and format of each article) and by the nature of the audience. For example, 5 percent of all papers were addressed to lay audiences, such as lay readers of *Scientific American*, *Triangle*, and *Science Year* or to physicians for whom a simplified account of recent progress in biology is available in articles, such as those in *Clinician*, *Contraception*, or *Hospital Practice*. Although a relatively minor output in terms of quantity, this genre fulfills an important public relations function in that such articles can be useful in the long-term acquisition of public funds. A second genre, which made up 27 percent of total output, addressed scientists working outside the releasing factors field. Sample titles included: "Hypothalamus Releasing Hormones," "Physiology and Chemistry of the Hypothalamus," and "Hypothalamic Hormones: Isolation, Characterisation and Structure Function." The details of specific substances and assays or of the relations between them were rarely discussed in these kinds of articles, which could be found most frequently in advanced textbooks, reference books, nonspecialised journals, book reviews, and invited lectures. The information in these articles was often utilised by students or by colleagues in outside fields. Such papers are both incomprehensible to laymen and unremarkable to

colleagues within the field of releasing factors. They simply summarize the state of the art for scientists outside the field. A third genre, which made up 13 percent of the total output, included titles such as: "Luteinizing Releasing Factor and Somatostatin Analogs: Structure Function Relationships," "Biological Activities of SS," and "Chemistry and Physiology of Ovine and Synthetic TRF and LRF." These articles were specialised to the extent that they made little sense outside the speciality. They were characterised by an unusually high number of coauthors (5.7 compared with an average of 3.8 for all papers) and were usually presented at professional meetings within the field such as the Endocrine Society Meetings and Peptide Chemistry Symposia. Articles in this third genre enabled colleagues to catch up on the latest available information. Lastly, a genre which made up 55 percent of the total output comprised highly specialised articles as indicated by the following example titles: "(Gly) 2LRF and Des His LRF. The synthesis purification and characterisation of two LRF analogs antagonists to LRF" and "Somatostatin inhibits the release of acetylcholine induced electrically in the myenteric plexus." Such articles, which aimed to convey minute pieces of information to a select band of insiders, were published mainly in journals such as *Endocrinology* (18 percent), *BBRC* (10 percent), and *Journal of Medical Chemistry* (10 percent). Whereas papers falling within the first and second genres were thought to be important in a teaching context, only those articles in the latter two genres (the insider reviews and specialised articles) were regarded by members of the laboratory as containing new information.

By dividing the annual budget of the laboratory by the number of articles published (and at the same time discounting those articles in the laymen's genre), our observer calculated that the cost of producing a paper was \$60,000 in 1975 and \$30,000 in 1976. Clearly, papers were an expensive commodity! This expenditure appears needlessly extravagant if papers have no impact, and extravagantly cheap if papers have fundamental implications for either basic or applied research. It may therefore be appropriate to interpret this expenditure in relation to the reception of papers.

One preliminary method of examining the cost of production in relation to the received value of papers is through an examination of citation histories. Our observer used the SCI to trace the citations of the 213 items¹⁷ published by participants between 1970 and 1976. Items that were not cited (articles by laymen, unpublished lectures,

deleted
info for
reference
papers

and abstracts that were difficult to obtain) were then weeded out and the remainder divided into those highly likely to be cited and those (usually chapters of books or abstracts) that were not. Since the peak of citation activity rarely occurred later than the fourth year following publication, the observer calculated an index of each item's impact based on citations in the year of publication and in the subsequent two years.

The overall impact ratio (number of citations per item) was 12.4 c.p.i. for the five years for which it could be calculated (1970-1974). However, this figure conceals three main sources of variation. Firstly, impact ratio varied according to genre. For example, when only "regular" papers were considered, the impact ratio rose to 20 c.p.i. Furthermore, only 17 of the items identified as "regular" papers and published in what participants referred to as "good" journals had no impact whatsoever before the end of 1976. Secondly, impact ratio varied over time. It was 23.2 c.p.i. for the 10 items published in 1970, but only 8 c.p.i. for the 39 items published in 1974. This particular variation is explained by the fact that 1970 was the year of a major discovery (see Chapter 3). Thirdly, as is evident from the right-hand column of Table 2.1, impact ratio also varied by programme. Of the three programmes we characterised earlier, those items concerning the isolation and characterisation of substances had the highest impact ratio (24 c.p.i.). Only one other category of activity, production of analogs carried out in collaboration with clinicians (task three of the second programme), had comparable impact (21 c.p.i.). Items resulting from other activities had much less impact. The third programme, for example, made up 22 percent of overall output (in terms of items produced) but had an impact ratio of only 10.6 c.p.i. Task two of the second programme made up a similar proportion of overall output (24 percent) but had even less impact (7.6 c.p.i.).

If impact ratio is taken as a crude indicator of return on the initial costs of producing items of literature, it is clear that a higher level of return is not necessarily guaranteed by increased output. One dominant factor would appear to be the extent to which items can appear as "regular" papers. However, this is confused both by variations over time and by the particular activity associated with each item. We are left, therefore, with the somewhat tautological speculation that items which yield a high return are those with a high chance of addressing issues of concern outside the laboratory.

STATEMENT TYPES

Although citations revealed that items had varying impact, our observer felt that he had discovered little about why this was the case. One reaction to this kind of problem is to engage in more sophisticated and complex mathematical analysis of citation histories, in the hope that some clearly identifiable pattern of citations will emerge.¹⁸ But our observer was unconvinced that this would alleviate his basic difficulty of understanding why items were cited in the first place. Instead, he reasoned that there must be something in the *content* of papers which would explain how they were evaluated. Accordingly, our observer began to peruse some of the articles in order to ferret out possible reasons for their relative value. Alas, it was all Chinese to him! Many of the terms were recognisable as the names of substances, or of apparatus and chemicals which he had already come across. He also felt that the grammar and the basic structure of sentences was not dissimilar to those he used himself. But he felt entirely unable to grasp the "meaning" of these papers, let alone understand how such meaning sustained an entire culture. He was reminded momentarily of an earlier study of religious rituals when, having penetrated to the core of ceremonial behaviour, he had found only twaddling and waffling. In a similar way, he had now discovered that the end products of a complex series of operations contained complete gibberish. In desperation he turned to participants. But his requests for clarification of the meaning of papers were met with retorts that the papers had no interest or significance *in themselves*: they were only a *means* of communicating "important findings." When further asked about the nature of these findings, participants merely repeated a slightly modified version of the content of the papers. They argued that the observer was baffled because his obsessive interest in literature had blinded him to the real importance of the papers: only by abandoning his interest in the papers themselves could the observer grasp the "true meaning" of the "facts" which the paper contained.

Our observer might have become extremely depressed by participants' scorn, were it not for the fact that participants immediately resumed their discussion of drafts, the correction and recorection of galley proofs, and the interpretation of various traces and figures which had just been produced by inscription devices. At the very least, reasoned our observer, there must be a strong relationship between processes of literary inscription and the "true meaning" of papers.

The above disagreement between observer and participant hinged on a paradox which had already been hinted at several times during this chapter. The production of a paper depends critically on various processes of writing and reading which can be summarised as literary inscription. The function of literary inscription is the successful persuasion of readers, but the readers are only fully convinced when all sources of persuasion seem to have disappeared. In other words, the various operations of writing and reading which sustain an argument are seen by participants to be largely irrelevant to "facts," which emerge solely by virtue of these same operations. There is, then, an essential congruence between a "fact" and the successful operation of various processes of literary inscription. A text or statement can thus be read as "containing" or "being about a fact" when readers are sufficiently convinced that there is no debate about it and the processes of literary inscription are forgotten. Conversely, one way of undercutting the "facticity" of a statement is by drawing attention to the (mere) processes of literary inscription which make the fact possible. With this in mind, our observer decided to look carefully at the different kind of statements to be found in the papers. In particular, he was concerned to delineate the extent to which some statements appeared more fact-like than others.

At one extreme, readers are so persuaded of the existence of facts that no explicit reference is made to them. In other words, various items of knowledge are simply taken for granted and utilised in the course of an argument whose main burden is the explicit demonstration of some other fact. Consequently, it was difficult when reading articles consciously to note the occurrence of taken-for-granted facts. Instead, they merged imperceptibly into a background of routine enquiry, skills, and tacit knowledge. It was obvious to our observer, however, that everything taken as self-evident in the laboratory was likely to have been the subject of some dispute in earlier papers. In the intervening period a gradual shift had occurred whereby an argument had been transformed from an issue of hotly contested discussion into a well-known, unremarkable and noncontentious fact. The observer therefore posited a five-fold classificatory scheme corresponding to different types of statements. Statements corresponding to a taken-for-granted fact were denoted *type 5* statements. Precisely because they were taken for granted, our observer found that such statements rarely featured in discussions between laboratory members, except when newcomers to the laboratory required some introduction to them. The

greater the ignorance of a newcomer, the deeper the informant was required to delve into layers of implicit knowledge, and the farther into the past. Beyond a certain point, persistent questioning by the newcomer about "things that everybody knew" was regarded as socially inept. In the course of one discussion, for example, X repeatedly argued that "in the grid test rats do not react as if they were on neuroleptics." For X, the force of the argument was clear. But for Y, a scientist working in a different field, there were preliminary questions to ask: "What do you mean by a grid test?" Somewhat taken aback, X stopped, looked at Y, and adopted the tone of a teacher reading from a textbook: "The classic catalepsy test is a vertical screen test. You have a wire mesh. You put the animal on the wire mesh and an animal which has been injected with neuroleptic will remain in this position. An animal which is untreated, will just climb down" (IX, 83). For X, his earlier reference to the assay was a *type 5* statement which required no further explication. After this interruption, X adopted his previous excited tone and returned to the original argument.

Scientific textbooks were found to contain a large number of sentences with the stylistic form: "A has a certain relationship with B." For example, "Ribosomal proteins begin to bind to pre-RNA soon after its transcription starts" (Watson, 1976: 200). Expressions of this sort could be said to be *type 4* statements. Although the relationship presented in this statement appears uncontroversial, it is, by contrast with *type 5* statements, made explicit. This type of statement is often taken as the prototype of scientific assertion. However, our observer found this type of statement to be relatively rare in the work of scientists in the laboratory. More commonly, *type 4* statements formed part of the accepted knowledge disseminated through teaching texts.

Another kind of statement consisted of expressions with the form. "A has a certain relationship with B," which were embedded in other expressions: "It is still largely unknown which factors cause the hypothalamus to withhold stimuli to the gonads" (Scharrer and Scharrer, 1963). "Oxytocin is generally assumed to be produced by the neurosecretory cells of the paraventricular nuclei" (Olivecrona, 1957; Nibbelink, 1961). These were referred to as *type 3* statements. They contained statements about other statements which our observer referred to as *modalities*.¹⁹ By deleting modalities from *type 3* statements it is possible to obtain *type 4* statements. The difference between statements in textbooks and the above, many of which appeared in review articles (Greimas, 1976), can thus be charac-

terised by the presence or absence of modalities. A statement clearly takes on a different form when modalities drop. Thus, to state, "The structure of GH.RH was *reported to be X*" is not the same as saying, "The structure of GH.RH *is X*." Our observer found many different types of modality. One form of statement, for example, included a reference and a date in addition to the basic assertion. In other statements, modalities comprised expressions relating to the merit of the author or to the priority of work which had initially postulated the relationship in question: "[T]his method has *first* been described by Pietta and Marshall. Various investigators clearly established [ref.]" "More convincing evidence was provided by [ref.]" "[T]he first unequivocal demonstration was provided by [ref.]" (all quotations from Scharrer and Scharrer, 1963).

As mentioned above, many *type 3* statements were found in review discussions. Much more common among the papers and drafts circulated in the laboratory were statements which appeared rather more contentious than those in reviews.

Recently Odell [ref.] has reported that hypothalamic tissues, when incubated . . . would increase the amount of TSH. It is difficult to ascertain whether or not . . .

At this time we do not know whether the long acting effect of these compounds extract to their potential inhibitory activity (Scharrer and Scharrer, 1963).

Statements of this form appeared to our observer to more nearly constitute *claims* rather than established facts. This was because the modalities which encompassed expressions of basic relationships seemed to draw attention to the circumstances affecting the basic relationship. Statements containing these kinds of modalities were designated *type 2* statements. For example:

There is a large body of evidence to support the concept of a control of the pituitary by the brain.

The role of nitrogen 1 and nitrogen 3 of the imidazole ring of histidine in TRF and LRF seems to be different.

It is unlikely that racemization occurs during esterification with any of the above procedures, but little experimental evidence is available to support this point (Scharrer and Scharrer, 1963).

More precisely, *type 2* statements could be identified as containing modalities which draw attention to the generality of available evidence

(or the lack of it). Basic relationships are thus embedded within appeals to "what is generally known" or to "what might reasonably be thought to be the case." The modalities in *type 2* statements sometimes take the form of tentative suggestions, usually oriented to further investigations which may elucidate the value of the relationship at issue:

It should not be forgotten that hypothalamic tissues contain non-negligible quantities of TSH . . . which may further complicate the interpretation of the data. . . It would be interesting to ascertain whether or not their material is similar. . . It is somewhat puzzling that . . . (Scharrer and Scharrer, 1963).

Type 1 statements comprise conjectures or speculations (about a relationship) which appear most commonly at the end of papers, or in private discussions:

Peter [ref.] has suggested that in goldfish the hypothalamus has an inhibitory effect on the secretion of TSH.

There is also this guy in Colorado. They claim that they have got a precursor for H . . . I just got the preprint of their paper (III, 70).

It may also signify that not everything seen, said and reasoned about opiates may necessarily be applicable for the endorphins.

By this stage, then, our observer had identified five different types of statement. At first glance it seemed that these types could be arranged in a broad continuum such that *type 5* statements represented the most fact-like entities and *type 1* the most speculative assertions. It would follow that changes in statement type would correspond to changes in fact-like status. For example, the deletion of modalities in a *type 3* statement would leave a *type 4* statement, whose facticity would be correspondingly enhanced. At a general level, the notion that changes in statement type may correspond to changes in facticity seems plausible enough. At the level of empirical verification, however, this general scheme encounters certain difficulties.

In any given instance, there seems to be no simple relationship between the form of a statement and the level of facticity which it expresses. This can be demonstrated, for example, by considering a statement which contains an assertion about the relationship between two variables together with a reference. As it stands, our observer would classify this statement as a *type 3* where the modality is

constituted by the included reference. Undoubtedly, the deletion of the modality would leave a *type 4* statement. It is questionable, however, whether this would enhance or detract from the fact-like status of the statement. On the one hand, we could argue that the inclusion of a reference draws attention to circumstances surrounding the establishment of the relationship in question and that this, by implication, renders the relationship less indisputable and hence less likely to be taken for granted. By noting that human agency was involved in its production, the inclusion of a reference diminishes the likelihood that the statement will be accepted as an "objective fact of nature." On the other hand, it could be argued that the inclusion of a reference lends weight to a statement which otherwise appears to be an unsupported assertion. Thus, it is only by virtue of the reference that the statement achieves any degree of facticity.

The determination of the correct or more appropriate interpretation of the function of a modality will depend critically on our knowledge of the context in each particular case. If, for example, we have good grounds for supposing that the inclusion of a modality in a paper was a presentational device designed to enhance the acceptance of a statement, then the onus is upon us to provide details of the context in which this device was so used. There are, of course, those who argue that this kind of determinate relationship between context and a particular interpretation of a statement simply does not exist. For our purposes, however, it is sufficient to note that changes in the type of statement provide the *possibility* of changes in the fact-like status of statements. Even though, in any individual instance, we may not be able unambiguously to specify the direction of change in facticity, we retain the possibility that such changes *can* correspond to changes in statement types.

Because he was aware of the problems both of specifying the fact-like status of any given statement and of specifying the direction of change of facticity in any example, our observer felt he could not stake a great deal on the determinacy of correspondence between statement type and fact-like status. Nevertheless, he realised that the notion of literary inscription had provided a useful tool. Although he understood little of the content of the papers he was reading, he had developed a simple grammatical technique for distinguishing between types of statements. This, he felt, enabled him to approach the very substance of scientists' statements without having entirely to rely on participants for elucidation or assistance. Furthermore, to the extent that changes

in the grammatical form of scientists' statements provided the possibility of changes in their content (or fact-like status), he could portray laboratory activity as a constant struggle for the generation and acceptance of particular types of statement.

THE TRANSFORMATION OF STATEMENT TYPES

Despite the simplicity of the classificatory scheme presented above (and summarized in Figure 2.3), it at least provided our anthropologist with a tentative means of ordering his observations of the laboratory which was consistent with his earlier notion of literary inscription. Activity in the laboratory had the effect of transforming statements from one type to another. The aim of the game was to create as many statements as possible of type 4 in the face of a variety of pressures to submerge assertions in modalities such that they became artefacts. In short, the objective was to persuade colleagues that they should drop all modalities used in relation to a particular assertion and that they should accept and borrow this assertion as an established matter of fact, preferably by citing the paper in which it appeared. But how precisely is this achieved? What exactly are the operations which successfully transform statements?

Consider the following example, in which John interrupts K's description of an assay in which the effect of LH had apparently been blocked.

- John: Since melatonin inhibits LH we cannot be sure that you are not simply measuring melatonin.
- K: I don't believe these data on the release of LH by melatonin . . . not in my system (VI, 18).

Instead of simply accepting K's previous statement, John adds a modality ("we can not be sure") to the unstated assumption that the investigators were "not simply measuring melatonin." John thus casts doubt on an original unstated, and hence *type 5* statement by using a qualification about the consensual certainty which investigators ("we") are entitled to assume. As a result, the original *type 5* statement is transformed into a highly conjectural *type 2* statement. The transformation is made particularly effective in this case by the preceding justification for investigator's lack of sureness. "Since melatonin inhibits LH" constitutes the use of a *type 4* statement to justify the addition of a modality to the originally unstated assumption. K's response attempts to recast John's justificatory *type 4* statement

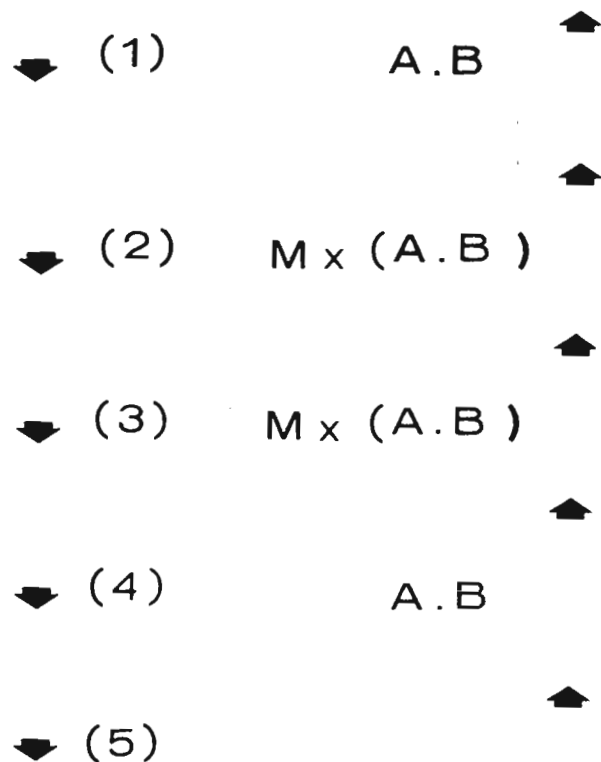


Figure 2.3

This diagram represents the different stages a statement—A.B—undergoes before becoming a fact. A fact is nothing but a statement with no modality—M—and no trace of authorship. The last stage—5—characterises the implicit dimension of something so obvious that it does not even have to be said. To move a statement from one stage to another, operations have to be performed. As indicated by the arrows, a given statement may move toward a fact-like status—from 1 to 5—or toward an artefact-like status—from 5 to 1—(see Ch. 4).

by adding a modality. By “not believing” circumstances surrounding the establishment of “melatonin inhibits LH,” K tries to undercut John’s attempt to undercut the unstated assumption that “you are not simply measuring melatonin.”

A second example is an excerpt from a paper written by John: “Our original observations (ref.) of the effects of somatostatin on the secretion of TSH have now been confirmed in other laboratories (ref.)” John had written an earlier paper, to which he first refers, and the statements contained therein had been subsequently confirmed. Whereas the statement, “the effects of somatostatin on the secretion of TSH,” had originally appeared as a claim of *type 2*, it now appears as an assertion embedded within references and enhanced by the modality “have now been confirmed.” In this way, John was able to borrow a statement made by others in order to transform his own initial statement into *type 3*.

The above examples demonstrate the use of two related operations. The first effects a change in the existing modality which can either enhance or detract from the facticity of a given statement. The second borrows an existing statement type in such a way that its facticity can be either enhanced or diminished (Latour, 1976).

The observer was now able to think of what had previously appeared a confused mixture of papers in terms of a network of texts containing a multitude of statements. The network itself comprised a large body of operations on and between these statements. It would thus be possible to document the history of a particular assertion as it became transformed from one statement type into another and as its factual status was continually diminished or enhanced as the result of various operations. We have already specified, in a preliminary way, the nature of operations by which statement types becomes transformed. Let us now examine in more detail one criterion for the success of an operation.

Our observer recalled that the inscriptions produced by certain configurations of apparatus were “taken seriously” if they could be read as being the same as other inscriptions produced under the same conditions. In simple terms, participants were more convinced that an inscription unambiguously related to a substance “out there,” if a similar inscription could also be found. In the same way, an important factor in the acceptance of a statement was the recognition by others of another statement which was similar. The combination of two or more apparently similar statements concretised the existence of some

external object or objective condition of which the statements were taken to be indicators. Sources of "subjectivity" thus disappeared in the face of more than one statement, and the initial statement could be taken at face value and without qualification (cf., Silverman, 1975). It is in this manner that our scientists, when noticing a peak on the spectrum of a chromatograph, sometimes rejected it as noise. If, however, the same peak was seen to occur more than once (under what were regarded as independent circumstances), it was often said that there was a substance there of which the peaks were a trace. An "object" was thus achieved through the superimposition of several statements or documents in such a way that all the statements were seen to relate to something outside of, or beyond, the reader's or author's subjectivity.²⁰ Similarly, the introduction, or rather the reintroduction, of an author's subjectivity as essentially linked to the production of a statement could be used to diminish the factual status of the statement. In the laboratory, "objects" were accomplished by the superimposition of several documents obtained from inscription devices within the laboratory or from papers by investigators outside the laboratory (cf., Chapter 4). No statement could be made except on the basis of available documents; statements were thus loaded with documents and modalities which constituted an evaluation of the statement. Consequently, grammatical modalities ("maybe," "definitely established," "unlikely," "not confirmed") often acted like price tags of statements, or, to use a mechanical analogy, like an expression of the *weight* of a statement. By adding or withdrawing layers of documents, scientists could increase or decrease qualifications and hence the weight of the statement was modified accordingly. For example, one referee's report included the following: "The conclusion that the effect of Pheno . . . [to] release PRL *in vivo* is mediated through the hypothalamus *is premature*." Three references were then given, which further pulled the rug from under the author's conclusion. Thus, although the author had presented his statement as a *type 2* or *3*, the referee recast it in terms of *type 1*. Consider also the following: "The authors used a Polytron which is a much more vigorous means of tissue disruption. To my knowledge, *there are no reports in the literature* of successful subcellular fractionation of brain tissue disruption." In this case the referee cast doubt on the use of a machine which produced the documents on which the argument is based. This was done by reference to a notable absence of any statements which might justify and hence enhance the authors' original

claim. As a result, the authors' (unsupported) claim must be read in conjunction with diminishing modalities such as "there is no support for this" and is consequently to be regarded as worthless.

With the notion of operations between (and on) statements in the literature, our observer began to feel more confident in his ability to understand the layout of individual papers. As a brief indication of the scope of the analysis which this permitted, let us look closely at one of the papers produced by the laboratory (Latour, 1976; Latour and Fabri, 1977).

The introductory paragraph refers to four articles, previously published by members of the laboratory, in which they posited the structure of a particular substance B. This referencing can be read as the invocation of documents which bear upon the present problem. More specifically, the use of these past papers can be read as providing support for the present enterprise. (The grounds for this particular reading are simply that the four papers themselves received 400 citations, all of which appear confirmatory.) At the same time, however, the papers are themselves taken as statement *type 3*, for which further support is to be provided by the present argument: "this short note reports data obtained in rats which *confirm and expand our early results*." The three following paragraphs summarize the way in which inscription devices were set up so as to obtain data. The information appears here in the form of *type 5* statements. In other words, knowledge is invoked which is so common to an audience of potential readers that no citations are necessary: "All synthetic preparations of substance B had full biological activity as ascertained in 4 or 6 point assays *in vitro* with factorial analysis."

In each of the next statements from the "results" section of the paper, reference is made to a figure.

"The results shown in Fig. 2 demonstrate that substance B significantly lowers blood levels of GH for 20 to 40 mn but not for 40 to 50 mn." Each figure thus acts as a tidied representation of documents (obtained from a radioimmunoassay) which is used in the text to support a particular point. It is not simply that "the results demonstrate that . . ." Rather, these results have an external reference and an independent existence which can be supported by the presence of "Fig. 2." The inclusion of "shown in Fig. 2" can thus provide an enhanced reading of an otherwise unsupported claim about the results. Subsequent discussion comprises three paragraphs, which refer back to the former "results" section ("These experiments show that . . .").

The "results" section is itself based on figures which are, in turn, dependent on the inscription devices described earlier. The result of this accumulation of back references is an impression of objectivity: the "fact" that "synthetic substance B inhibits GH in rats" can be taken by the reader as independent of the author's subjectivity and thus worthy of belief.

At the same time the establishment of one statement opens up discussion of others: "The mechanisms of action of the barbiturate in . . . are not well understood." The modality "are not well understood" is not intended to diminish some prior claim about "the mechanisms of action of the barbiturate." Instead, its inclusion in this context amounts to a tentative suggestion for areas of future work. The statement is thus of *type 1* or *2*. As a result, subsequent discussion focuses on this statement as a new proposition: "[W]e might as well envisage them [the mechanisms] as involving inhibition of secretion of endogenous substance B, a hypothesis which is not incompatible with the data." Finally, the new statement is linked to a deontic operation:²¹ "This hypothesis will best be approached by some type of radio-immunoassay *still to be developed*."

It should not be forgotten, however, that this paper is itself part of a long series of operations within the field. The SCI shows that between 1974 and 1977 this paper received 62 explicit citations from 53 papers. Of these, 31 appear simply to have borrowed the conclusion (that synthetic substance B inhibits GH as well as natural substance B in the rat) as a fact and used it in their introduction; eight papers focused solely on the final deontic operations in the paper in pursuing the suggestion for further work; two papers by the same author cited the above paper as confirmatory evidence of his own earlier work; and four papers used fresh data further to confirm the original statement.

★ Only one paper raised doubts about the use of the assay in obtaining one of the figures mentioned in the fifth statement ("there are discrepancies between their results and ours"). This one paper examined above thus provided the focus of a variety of operations performed by later articles. Its weight depended both on its use of earlier literature, inscription devices, documents, and statements as well as on subsequent reaction to it.

Conclusion

A laboratory is constantly performing operations on statements; adding modalities, citing, enhancing, diminishing, borrowing, and

proposing new combinations. Each of these operations can result in a statement which is either different or merely qualified. Each statement, in turn, provides the focus for similar operations in other laboratories. Thus, members of our laboratory regularly noticed how their own assertions were rejected, borrowed, quoted, ignored, confirmed, or dissolved by others. Some laboratories were seen to be engaged in the frequent manipulation of statements while elsewhere there was thought to be little activity. Some groups produce almost at a loss: they talk and publish, but no one operates on their statements. In such a case, a statement can remain cast as a *type 1*, a claim lingering in an operational limbo. By contrast, other assertions can be seen to change their status rapidly, following a kind of alternate dance, as they are proven, disproven, and proven again. Despite the large number of operations performed on them, they rarely change their form radically. These statements represent a mere fraction of the hundreds of artefacts and half-born statements which stagnate like a vast cloud of smog. Commonly, attention shifts from these to other statements. In some places, however, we can discern a clearer picture. One or other operation irrevocably annihilates a statement never to be taken up again. Or, by contrast, in situations where a statement is quickly borrowed, used and reused, there quickly comes a stage where it is no longer contested. Amidst the general Brownian agitation, a fact has then been constituted. This is a comparatively rare event, but when it occurs, a statement becomes incorporated in the stock of taken-for-granted features which have silently disappeared from the conscious concerns of daily scientific activity. The fact becomes incorporated in graduate text books or perhaps forms the material basis for an item of equipment. Such facts are often thought of in terms of the conditioned reflexes of "good" scientists or as part and parcel of the "logic" of reasoning.

By pursuing the notion of literary inscription, our observer has been able to pick his way through the labyrinth. He can now explain the objectives and products of the laboratory in his own terms, and he can begin to understand how work is organised and why literary production is so highly valued. He can see that both main sections (A and B) of the laboratory are part of the same process of literary inscription. The so-called material elements of the laboratory are based upon the reified outcomes of past controversies which are available in the published literature. As a result, it is these same material elements which allow papers to be written and points to be made. Furthermore, the

anthropologist feels vindicated in having retained his anthropological perspective in the face of the beguiling charms of his informants: they claimed merely to be scientists discovering facts; he doggedly argued that they were writers and readers in the business of being convinced and convincing others. Initially this had seemed a moot or even absurd standpoint, but now it appeared far more reasonable. The problem for participants was to persuade readers of papers (and constituent diagrams and figures) that its statements should be accepted as fact. To this end rats had been bled and beheaded, frogs had been flayed, chemicals consumed, time spent, careers had been made or broken, and inscription devices had been manufactured and accumulated within the laboratory. This, indeed, was the very *raison d'être* of the laboratory. By remaining steadfastly obstinate, our anthropological observer resisted the temptation to be convinced by the facts. Instead, he was able to portray laboratory activity as the organisation of persuasion through literary inscription. Has the anthropologist himself been convincing? Has he used sufficient photographs, diagrams, and figures to persuade his readers not to qualify his statements with modalities, and to adopt his assertions that a laboratory is a system of literary inscription? Unfortunately, for reasons which will later become clear (see Chapter 6), the answer has to be no. He cannot claim to have set forth an account which is immune from all possibility of future qualification. Instead, the best our observer has done is to create a small breathing space. The possibility of future reevaluation of his statements remains. As we shall see in the next chapter, for example, the observer can be forced back into the labyrinth as soon as questions are posed about the historical evolution of any one specific fact.

NOTES

1. We stress that "the observer" is a fictional character so as to draw attention to the process whereby we are engaged in constructing an account (see Chapter 1). The essential similarity of our procedures for constructing accounts and those used by laboratory scientists in generating and sustaining facts will become clear in the course of our discussion. The point is taken up explicitly in Chapter 6.

2. The notion of inscription as taken from Derrida (1977) designates an operation more basic than writing (Dagognet, 1973). It is used here to summarize all traces, spots, points, histograms, recorded numbers, spectra, peaks, and so on. See below.

3. A file of photographs of the laboratory is presented after Chapter 2.

4. See note 2.

5. This notion of inscription device is sociological by nature. It allows one to describe a whole set of occupations in the laboratory, without being disturbed by the wide variety of their material shapes. For example, a "bioassay for TRF" counts as *one* inscription device even though it takes five individuals three weeks to operate and occupies several rooms in the laboratory. Its salient feature is the final production of a figure. A large item of apparatus, such as the Nuclear Magnetic Resonance Spectrometer, is rarely used as an inscription device. It is used instead to monitor a process of peptide production. However, the same apparatus, a scale for instance, can be considered an inscription device when it is used to get information about a new compound; a machine when it is used to weigh some powder; and a checking device when used to verify that another operation has gone according to plan.

6. Our observer was well aware of the popularisation of the term due to Kuhn (1970) and of the subsequent debates over its ambiguity and significance for models of scientific development (see, for example, Lakatos and Musgrave, 1970).

7. We use the term "peptide" throughout the following argument. One classical textbook definition of the peptidic bond is as follows: "A covalent bond between two amino acids in which the alpha amino group of one amino acid is bonded to the alpha-carboxyl group of the other with the elimination of H₂O" (Watson, 1976). In practice, "peptide" is a synonym for a small protein. However, it is important to realise that such terms need *not* be defined as if they have a universal meaning beyond that of the specific culture in which they are used. As if they were the terms used by the tribe under study, we shall enclose such terms in quotes in our discussion and attempt to account for them in nontechnical terms.

8. There are only some twenty amino acids in the body; proteins and peptides are made up exclusively of these amino acids: each amino acid has a name, for example, tyrosine, tryptophene, and proline. In the text we often use a simple abbreviation of these names (which uses the three first letters of the amino acid name).

9. These very crude figures are intended merely to give a general idea of the scale. They are based on the volume of space devoted to different topics in the *Index Medicus*.

10. Once again, these divisions are extremely artificial in that they are much too large and rigid to correspond directly to members' appraisal of their activities. On the other hand, these programmes have become very stable and routinised by comparison with those of other laboratories. Our intention here is merely to provide the reader with the backcloth necessary for understanding subsequent chapters.

11. The observer would be told, for example, that "when a chemist shows the spatial configuration of somatostatin is such that a particular amino acid is very exposed on the outside of the molecular structure; it may be that by replacing or protecting it, some new activity will be observed."

12. It would be wrong to take differences between what is and is not technical in science as the starting point. These differences are themselves the focus of important negotiations between members. This idea has been especially developed in sociology of techniques by Callon (1975). See also Chapter 1 p. 21ff and Chapter 6.

13. The same tendency is evident in sociological discussions of science which uncritically adopt the attitude that material phenomena are manifestations of conceptual entities.

14. During the first year of the study a new method of chromatography was tried in the laboratory. Albert worked on it for a year trying to adapt it to the purification programme of the group. As soon as it became settled, Albert turned the instrument over to a technician, after which it became a purely "technical" matter.

15. These calculations are only approximate: they are based on the overall budget of the laboratory as computed from grant applications. The activation of the laboratory cost about one million dollars. This was simply to connect the space to the rest of the institute (Photograph 1): buying the equipment on the general market cost approximately \$300,000 a year; Ph.D. holders earn an average of \$25,000 a year, while for technicians the figure is nearer \$19,000 a year. The total wage bill tops half a million dollars a year. The total budget of the laboratory is one and a half million dollars a year.

16. The advantage of a well-kept publication list is that it includes every item produced by the group, including rejected articles, unpublished lectures, abstracts, and so on. The following figures are intended to convey an idea of the scale of article production. Of course, only a stable laboratory can provide a reliable publication list.

17. We use the term "item" to refer to all the different types of published materials, articles, abstracts, lectures, and so on.

18. It is interesting to note the differences between those who argue that the development of a theory of citing behaviour should necessarily precede the use of citation data by sociologists and those who argue that the development of a citation typology will enable the analyst to overcome technical difficulties in the use of citation data. See, for example, Edge (1976) and other contributions to the International Symposium on Quantitative Methods in the History of Science, Berkeley, California, August 25-27, 1976. See also the special issue of *Social Studies of Science* 7 (2; May 1977).

19. In its traditional aristotelian meaning a "modality" is "a proposition in which the predicate is affirmed or denied of the subject with any kind of qualification" (Oxford Dictionary). In a more modern sense, a modality is any statement about another statement (Ducrot and Todorov, 1972). The following discussion owes much to Greimas (1976) and Fabbri (private communication, 1976).

20. The notion of "object" is used here because it has a root in common with "objectivity." Whether a given statement is objective or subjective cannot be determined outside the context of laboratory work. This work is precisely intended to construct an object which can be said to exist beyond any subjectivity (see Chapter 4). As Bachelard (1934) put it "science is not objective, it is projective."

21. In semiotics, the term "deontic" is used to designate the type of modality which indicates what "ought" to be done (Ducrot and Todorov, 1972). Although very crude, this analysis is intended, like the rest of this chapter, to do no more than introduce the general problem of scientific literature. More precise discussion can be found in Gopnik (1973), Greimas (1976) and Bastide (forthcoming).

PHOTOGRAPH FILE