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LABORATORY LIFE

The Social Construction of Scientific Facts

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Introduction by Jonas Salk

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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 it 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 specialty. 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 items[17] published by participants between 1970 and 1976. Items that were not cited (articles by laymen, unpublished lectures,

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 figures 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 recorrection 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 . . . woud 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

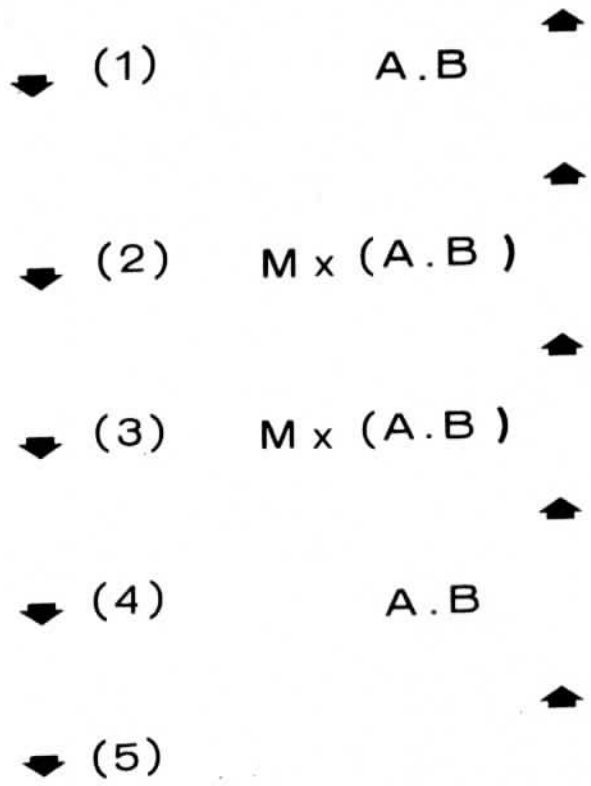

Figure 2.3
This diagram represents the different stages a statement—A.B—undergoes before becoming a fact. A fact is nothing but a statement with no modality—M—and no trace of authorship. The last stage—5—characterises the implicit dimension of something so obvious that it does not even have to be said. 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 an 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 *re*introduction, 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 hypothalmus *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 radioimmunoassay *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 H_2O" (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.