Simplification in Scientific Work: An Example from Neuroscience Research

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Social Studies of Science, Volume 13, Issue 2 (May, 1983), 205-228.

Stable URL:
http://links.jstor.org/sici?sici=0306-3127%28198305%2913%3A2%3C205%3ASISWAE%3E2.0.CO%3B2-G

● *ABSTRACT*

This paper presents an empirical analysis of simplification processes in the scientific work place. Any scientific task involves complex sets of problems and contingencies. But the conclusions produced do not represent all details of the work performed, nor do consumers take full account of the complexity of results. This paper examines the ways in which chains of inference are simplified at all stages of the research work, from research design and sampling to publication. What is deleted in the 'fact-making' process? What constraints operate to make this deletion necessary? This paper examines institutional and intersectional constraints and processes which affect the final work. The analytic approach of the paper is symbolic interactionist/Pragmatist, and field data were collected by participant observation.

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> The facts of the world in their sensible diversity are always before us, but our theoretic need is that they should be conceived in a way that reduces their manifoldness to simplicity. . . The simplified result is handled with far less effort than the original data. . .
>
> — William James[1]

Scientific work involves the representation of chaos in an orderly fashion. When scientists (like everyone else) describe nature, their descriptions make the complicated turbulence of the world appear,

Social Studies of Science (SAGE, London, Beverly Hills and New Delhi),
Vol. 13 (1983), 205-28

at least in part, orderly, predictable, and bounded. This process is central to all attempts to describe the world to oneself or others, as William James pointed out long ago.[2] If you never sort the chaos of the world, it never becomes sensible.

Much philosophy of science has been devoted to theories about how the outside world is 'filtered' by perceptual apparatus — how chaos is ordered, both internally and in description. Social studies of science have also studied factors in the types of choices which are made in forming representations: how are problems chosen? What is bias? What do technological limits have to do with problem choice? How do 'paradigms' limit that which is described? How do research traditions admit some problems and screen out others?

Recent work in the sociology of science has indicated that there is an enormous variety in the methods used, and in the conditions under which the complexity of nature gets made simpler and more manageable. Sometimes local politics decide how the field of investigation will be limited. At other times, a narrow focus on one 'building block' seen as part of a larger theoretical whole effectively screens out large blocks of complexity. Or the availability of a given piece of machinery will constrain the complexity of the problem — without X-ray crystallography, for instance, it is difficult to perceive the complex form of the double helix. There is always *reconciliation* between *theoretical* commitments and the *constraints on material resources*, including time and staff.

This paper examines one aspect of the reconciliation process between theoretical commitments and constraints on resources, which I shall show involves the work of simplifying the solving of problems and the representation of data.

Philosophy of science and intellectual history have focused almost exclusively on the development of theoretical commitments in science, omitting mention of material constraints. By contrast, sociologists and economists have often focused exclusively on those material aspects constraining scientific development. From either perspective, the reconciliation process per se remains unexamined.

Scientists themselves lose sight of their reconciliation work both in execution and description. Scientific work is complicated. Any set of scientific tasks involves multiple problems, qualifications, exigencies, demands and audiences. To work without getting lost in endless contingencies, scientists must draw boundaries and exclude some kinds of artefacts and complications from consideration.[3]

In other words, part of doing science is transforming problems with many contingencies into those simple enough to work on. In 'The Structure of Ill-Structured Problems', Simon discusses this transformation.[4] He notes that computer scientists ordinarily distinguish between 'well-structured' and 'ill-structured' problems. Well-structured problems are those for which all possible contingencies can be programmed — no new contingencies arise as a result of the problem-solving process. Ill-structured problems, on the other hand, develop new and unpredictable contingencies in the course of solution.

In fact, the number of significant well-structured problems in the real world is almost nil. Scientists break ill-structured problems into pieces which they work on *as if* they were well-structured, in order to get the work done. Creating well-structured problems includes ignoring complexity: uncertainties in the environment, subjects' reactions, unforeseen interaction effects. In order to actually do the research, lines and boundaries must be drawn around complications, implications, and exceptions. Goals, images and tasks simple enough to manage are developed. Simon notes that in the process of transforming ill-structured problems into well-structured ones, the relationships *between* well-structured problems are ignored. Learning to manage all of the complications that arise from these relationships is a major part of professional training and 'maturity'.

The process of creating well-structured problems from ill-structured ones is an essential part of scientific work. However, *in conjunction with* the deletion of descriptions of this process from scientists' descriptions of their work, scientific 'facts' become reified and their production histories lost. Those histories are further obscured by the shorthand of presenting results, both in publication and in the process of production. Retrieving those histories, by observing the process of deletion, should provide us with some important data about the connection between work process and 'facts'.[5]

The radical programme in the sociology of science has begun to investigate some aspects of the 'social construction' of scientific reality. Much of this work (notably Latour and Woolgar's *Laboratory Life*) has focused on the 'facticity' which occurs at the publication and analysis stages. Latour and Woolgar have developed a scale of measurement of reification of facts with regard to the way they are presented in written material or analysis ('deletion of modalities'). Published scientific conclusions do not represent all details of the work performed.[6] Rather, results are presented which are partial or schematic maps of the original work, emphasizing the theoretical development. In scientific publications, these simplified maps may form the basis for further research in other work sites, as other labs use them to help shape their ongoing work. But the presentation of scientific facts often includes the tacit assumption that they are to be read as fully detailed presentations of the work, thus fully detailed maps. As many have noted,[7] published scientific conclusions tend to present results as faits accomplis, without mention of production or decision-making processes.

Another aspect of the deletion of work descriptions has been explored by Collins and Pinch,[8] who begin with the observation that there is no such thing as a *fully* detailed rendition of scientific work. Variation and discretion are always present in the scientific work site, and complete replication is a chimera. Something differs in every experimental replication. They use the example of ESP research to examine the conditions under which this variability is invoked, or in which more rigid rules of replication are applied. Because mechanisms for examining the gap between work processes and work representations (publications) are informal and unspecified in many cases, legitimate and illegitimate designations can be made 'behind the scenes' by manipulating the rules of replication. Wynne has explored a similar theme from a historical perspective.[9] Lynch has also discussed the idea of the lack of specification and the re-creation of diagnostic categories in the clinical setting.[10]

This research points to a wide, informal zone in which bargaining takes place about how well-structured problems must be. The zone includes negotiation at many stages of scientific work, from research design to publication. There is ample room at all stages to lose sight of the process of transformation from ill-structured to well-structured.

The fitting process between developing theoretical commitments and the constraints on material resources can be examined at a number of analytic levels, as well as at different stages of the research process. Large-scale debates or historical change in scientific thought can be looked at in relationship to changing technologies, professional movements, and economic change. On a smaller organizational scale, individual laboratories or research programmes can be studied to reveal how they fit theories with constraints. Similarly, scientists' representations of their work can be studied at many stages, from selection of problems to organization of project to final publication.

In this paper, I shall look at how scientists in one laboratory simplified their results at several stages of the research process. Simplification is examined with regard to material (including technical) and methodological constraints, as well as theoretical development. The paper shows how, at the laboratory level, the constraints/theory negotiation process gives rise to simplified representations of scientific research. The work is analyzed at the stages of research design, data collection and early analysis. One part of the bargaining about how well-structured problems must be, and what complexity must accordingly be screened out, is thus examined.

Analytic Framework and Methods

This paper employs a symbolic interactionist approach to the study of science. In the context of the problem of simplification, this means two things: a Pragmatist philosophical basis *and* a commitment to empirical studies of work.

Long before the radical programme in the sociology of science began to examine science in a relativist light, Pragmatist philosophers had begun to do so. Peirce, Mead, Dewey and Bentley were all concerned with delineating the relationship between scientific work, the representation of scientific process, and the development of 'facts'.[11] They held that in science, as elsewhere, meaning is contructed from experience. In Mead's essay, 'Scientific Method and the Individual Thinker' (1917),[12] he states:

> Whenever we reduce the objects of scientific investigation to facts and undertake to record them as such, they become events, happenings, whose hard factual character lies in the circumstance that they have taken place, and this quite independently of any explanation of their taking place. When they are explained they have ceased to be facts and have become instances of a law, that is, Aristotelian individuals, embodied theories, and their actuality as events is lost in the necessity of their occurrence as expressions of the law; with this change their particularity as events or happenings disappears.[13]

For Pragmatists, then, scientific theories are based in the constraints and problems posed by action and work. Theories about what something *is* are built by trying to *use it* to change something else. As Dewey describes the process of reification or 'fact-making,' scientists also regularly forget that meanings or theories arise in an ever-changing work context. As Mead notes above, they freeze facts and attribute reality to them.

Symbolic interactionism grows from a tradition of empirical studies of work and conflict, as well as from Pragmatist philosophy. One learns about people by observing what they do, including what they argue about and consider to be mistakes and difficulties.[14]

The analysis employed here, like that of Becker,[15] Strauss,[16] and Gerson,[17] is focused on work, particularly science as a kind of work. From a Pragmatist perspective, it is also concerned with reification of that work.

The analytic premises are that: (1) one does not understand work simply by examining its products, nor (2) understand it *without* reference to the products; that (3) work involves joint effort over time, and thus is both interactive and processual; and that (4) meaning does not inhere in the nature of scientific work, but is continuously renegotiated by workers and consumers.[18] The units of analysis employed here are tasks and activities, not individuals or their allegiance to theories.

The data are drawn from participant observation and interview data in a psychophysiological research lab ('the Lab'). The Lab conducts both clinically-oriented and basic research. Electroencephalography (EEGs, or 'brain wave' recording), psychometry, and a variety of computer analyses are among the technologies used in the research.

I used the grounded theory method in this study.[19] This is a qualitative, inductive method. It includes observing work processes, interviewing respondents, and developing analytic codes for these data. The codes are constantly compared with one another during the course of observation in order to maximize validity.

The following section presents the constraints and pressures which forced scientists in the Lab to simplify their findings, and the types of actions which they took in so doing.

Constraints and Resources

It may seem obvious at first glance that scientists must establish budgets of time and energy, and that they act within material constraints which define the scope and substance of the problems they can solve. Yet such constraints have not been well explored with respect to their dialectical relationship with scientific results, especially for problem formulation and analysis. Rather, they are seen as amorphous 'forces' or irritations to be circumvented, or analyzed for their inequitable distribution within the scientific system.

Constraints and the selection of resources are discussed here in light of the development of research commitments — that is, of ways of carrying out work. Constraints and resources are factors which set limits on the amount of freedom scientists have to negotiate. Gerson defines four types: time, money, skill (technology) and sentiment.[20] Response to constraints and choices of resources shape the way the work is done. Patterns of commitment develop from these choices. Commitment in this sense is defined by Becker as the development of a series of 'side bets' which constrain one's relationship to a particular line of action.[21] When the bets do not materialize, commitment patterns shift.

Simplification is one method of proceeding with work problems which, because of limited resources, cannot be handled in their full complexity. It is thus an important part of describing commitment patterns. The following section describes several types of *constraints* on resources faced by neuroscientists, and responses to those constraints:

1. *Intersection*. Intersection with specialists in other fields necessitates a simplification of terminology and concepts for the uninitiated on both sides, as well as a selection of technologies for the best 'fit'.
2. *Clinical*. Demands from clients or outside agencies for simple, immediately usable explanations pressure scientists to provide simple explanations.
3. *Technical*. Exigencies which force researchers to gather data quickly, from a small area, or with limited technical capacity.
4. *Conclusion pressures*. Pressures on researchers to give results before full proof (by their own standards) is reached or before sufficient data are gathered.
5. *Formatting*. Requirements that research results be formatted in certain ways can constrain the amount of complexity

that will be communicated.

6. *Editing rules*. Editing rules are 'rules of thumb' which have been developed by scientists either to fit data to proper formats or styles, or to expedite analytic work. They involve deleting or altering aspects of the data or presentations.
7. *Specialization*. Specialization refers to the appropriation of a set of tasks by one individual or group within the laboratory. Specialties both focus and narrow a field of investigation.

1. Intersection Constraints

Lines of work are not closed, static arrangements. Scientific lines of work intersect with one another, share work and perspectives.[22] They form alliances and exchange technologies. Intersections between scientific lines of work can entail simplification. This stems in part from the need to translate, to simplify technical language for the uninitiated. Intersection also involves fitting and selecting. When multiple sets of skills, techniques, and ways of working are joined, plans, goals and evaluation criteria are renegotiated.[23] Ultimately, work procedures become standardized into a form mutually usable for each of the intersecting parties.

Intersectional constraints are not simply pressures to sell science to a receptive lay audience or to package it for market trends. These constraints are part of ongoing peer exchange and interaction at all levels.

Work in the Lab intersected with several disciplines: clinical psychology, physiology, education, psychometry, and neurology. Outside researchers and clinicians were coming into the Lab and techniques from those lines of work were being incorporated into the ongoing work. All of these disciplines have extremely complicated and well-articulated ways of structuring tasks. The selection necessitated by intersection constrained this complexity. Only what was negotiated as 'relevant' became part of the structures of tasks in the work project. This negotiated relevance simplifies tasks and techniques imported from intersecting worlds.

Many Lab staff, for instance, were learning to perform neurological exams.[24] Several attended a seminar in psychological neurology and expressed a great interest in neurological theories of localization of function. The intersection with neurology imported a standardized set of clinical techniques into the Lab. In comparison with the psychophysiologists at the Lab, neurologists as a group have well-packaged, routine methods for determining location of abnormalities in the brain. Using neurological examination techniques, they predict the exact location of brain tumours, for instance. Various speech and cognitive disorders are differentiated precisely.

Thus, when Lab staff imported neurological techniques, they also imported theories about localization of function in the brain. In the process, they simplified their own ideas about localization in favour of techniques and theories that could give certainty of clinical results.

Intersection meant that researchers with different backgrounds, terminologies, and assumptions were coming together to work on a problem of mutual importance.

Like many situations requiring translation work, there were interpreters (familiar with multiple languages/specialties), and those who sought a 'universal language' which would cut across specialties. There were also those who refused to learn any language but their 'native' one. (Statisticians and engineers most frequently got away with this!) From this mix of competences, translation problems were common. As one staff member from a clinical discipline, in interview, describes:

> Well, there's the problem of developing common terms, that's one of the biggest problems. . . I think one of the things that's going to hold the field back and that's going to make it difficult for a long time to come is people who are trained in one aspect without realizing that they're not making the bridge. You know, they think they're getting it, but they don't know — they don't have enough of a context to know if they're making it across or not, if you will. And so there's going to be years of argument about things, argument about what's meant about something, what something means, because there hasn't been an attempt to know both sides and to bridge the common language. There's tremendous problems between say, clinical psychologists and what I would call neuropsychologists. Because of the changes in funding, and the threats to the changes in funding in clinical psych, a number of clinical psychologists now want to move into neural psychology. . . Now what they say, and I've heard this in different training meetings and stuff, is that in a sense they don't have to learn the actual neurology. All they have to do is be good psychometricians. But the point is, who's establishing the validity for assessments?

As this description indicates, techniques are sometimes lifted from one context to another when disciplines intersect, with the result that the context of development is lost. There is almost always some deletion of context involved in 'lifting' the techniques and language from their original line of work. Negotiations about relevance overlook problems, complications, and historical information about errors which have been part of the store of knowledge of the original line of work. Within the original lines of work, these problems and complications have helped form the basis for decisions about use of a technique, or evaluations about 'appropriateness' and robustness of findings.

Because the simplification process is not discussed in published reports, however, this deletion becomes invisible. There is thus no clear way of measuring comparability of a simplified term before and after intersection. Thus, after imported terms and techniques become standardized in the new context, they acquire a meaning which may or may not be comparable to that used in another scientific world. The process has no end point; imported techniques and language develop within each new context. In turn, they may then be 'lifted' in yet another intersection. The whole process may be quite rapid, as evidenced for example by the widespread simplification and adaptation of the right brain/left brain concept from neurosciences.[25]

2. Clinical Constraints

Another kind of constraint faced by respondents was pressure from clinical subjects. These subjects, who had learning disabilities, were not patient with the length of time it took to produce answers through basic research. Clients, and their parents, saw time passing and the disability continuing. Such research concerns as localization of brain function, the intricacies of right and left handedness, genetics, and subcortical processes are not theirs. One staff member described the pressure for clinical results thus:

> I hear 'my kid is getting older'. . . and all I have to offer them is some tidbit from basic research on handedness or whatnot. I want to be able to offer something concrete to individual children.

Clinical pressure also comes from the relationship between research results and legal and insurance eligibility criteria. Adult subjects can qualify for workman's compensation if they are judged disabled enough. Similarly, children can be channeled into special education classes. Pressures for eligibility certification may conflict with the technical perspective of the researchers. Researchers' complex definitions of impairment are not taken into account by social service agencies and the law. Scientists who certify clients are forced to adopt the formats and definitions of the service agencies, and to meet those requirements — and thus to simplify their own technical definitions.

The general relationship between clinical research, basic research, and simplification processes has not been explored in either medical sociology or sociology of science.[26] At the Lab, respondents were looking for ways to use clinical services to pay for basic research, either by treating clients or by marketing diagnostic devices or packages. As they discussed these plans, it became apparent that research concerns were being evaluated for their market appeal, and that those with appeal were often simplified or stripped down versions of complex basic research questions. Further (and comparative) research into these sorts of transitions and the search for applications is needed to elucidate the basic-applied research relationship with regard to simplification.[27]

3. Technical Constraints

Simplification is often forced by the exigencies of limited time or technical resources. In every stage of the research project, these are budgeted for what respondents hope will be maximum yield of data. The budgeting described here involves cutting and selecting. Time schedules for data observation are made. Time sampling constraints are selected for data collection. Computer and equipment storage capacity, space on the oscilloscope screen or the polygraph record are allocated. Work space must be obtained and evaluated. These decisions require negotiations about what is essential for the work. Like negotiations between different lines of work, there is a selection and trimming of qualifications and complications from the data and results to fit technical constraints. Several constraints here were subjects' time, their behaviour, and technical capacity limitations.

3a. Subjects' Time

Researchers have a strikingly small amount of time with their subjects, especially when contrasted with the complexity of the problems being studied. In the experiments I observed, subjects spent several hours at the Lab. But measuring equipment had to be recalibrated, experiments explained to subjects and some tasks repeated. Thus, the total time of direct data collection with neurophysiological equipment was less than four hours. By the time the editing process was complete, it might be less than two and as little as one hour. In addition, the equipment was set up to sample subjects' behaviour at fixed intervals, not to record it continuously. Therefore one does not in fact have one or two solid hours of behaviour on record, but perhaps half an hour if recorded continuously.

The exigencies of timing require simplifying certain aspects of the research. The subjects cannot engage in many ordinary forms of behaviour during the experiment. They must limit themselves as nearly as possible to actions and movements that fit experimental design constraints. This means trying to control ordinary acts like sneezing, eyeblinking, and scratching. Chatting, disagreeing with the experimenter about the meaning of an activity or question, or disattending to a question are prohibited. Any activity which will take up necessary time is discouraged; the subject's time is a scarce resource and must be rationed. These time constraints at the data collection stage require judgements not only about what activities are necessary, but about what *are* data.[28] All activity which will not be used to form inference chains is eliminated. At the data analysis and interpretation stages of the work, this becomes editing and discarding anomalous findings.

3b. Technical Capacity Constraints

Storage and presentation capacity present important parameters into which data must be squeezed. These include data-viewing areas on oscilloscopes or on polygraph readout charts, and the technical capacity of computers. The central process which occurs in the squeezes is negotiation between staff about what is essential, relevant, and necessary. When computer capacity is at a premium, evaluations must be made about what data to keep and what to store.

Where there is only room on a piece of chart paper for eight channels of signals from the brain, only eight electrodes will be used to sample physiological activity, regardless of the loss of complexity for the results. When an oscilloscope screen can display only 9 inch data segments, then those segments must be represented simply enough for the human eye to read and make sense of.

4. Conclusion Pressures

As discussed above, researchers often have more results than there is time or technical capacity to analyze or present. They are then forced to select only what is essential about results, and to delete qualifications in favour of presenting 'streamlined' findings. Choices *among* results create a different kind of simplification. When researchers are forced to choose among results, they do not have the opportunity to specify the relationships between those results.

Within the work site, these conclusion pressures were experienced as temporal. Researchers felt forced to reach conclusions before they were satisfied about *either* their grounds for choosing *or* about the relationship between results. They spoke in this case of 'over-precision' in their work about what they felt to be vague or unproved assumptions. They were able to be precise about results (as funding agencies, departments or deadlines required), but were uneasy or unclear about why it was *those* results which were chosen. In other words, they were forced to act with certainty although they did not understand the general context of choice.[29]

Pressure from the granting agencies often forced researchers to, in the word of one, 'Pick a result, any result'. At the beginning of the research, the staff had collected an enormous amount of data. Because of the timing schedule of the granting agency, they had to present results for a continuation proposal before they really *had* results. The outcome, in their eyes, was a rather arbitrary selection of marketable conclusions for presentation to the agency.

This kind of pressure also arose internally in the laboratory. Researchers were faced with piles of computer printouts of 'basic data', notebooks filled with information about subjects and experiments. In the absence of strong hypotheses, they felt pressured to specify relationships within the data, sometimes without obvious rationales. One described it as a hunt for a needle in a haystack. The situation in the Lab was accentuated because researchers were collecting baseline information, not testing a narrow hypothesis. Yet the problem of sheer quantity, and the resulting simplification through pressure to choose results, is common in scientific research.[30]

Respondents described this pressure as 'over-precision'. When pressured to select, relationships between results became either fuzzily specified, unspecified, or specified in a manner which respondents felt to be cavalier. Respondents repeatedly stressed that they knew they were being 'too' precise about the indicators they had. They felt that the relationship between these indicators and what they knew about the brain itself was unclear. Meanwhile, more detailed information about the indicators was still being compiled. One researcher made a wry parallel between doing brain research by using ten skull electrodes and making a map of the Atlantic floor by flying over in a helicopter and scooping a cup of water every 500 miles! One might be able to analyze the water chemical content with great precision, but creating a map would be impossible.

5. *Formatting Constraints*

Scientists are always called upon to account for the work that they do: by clients or patients in a clinical setting; by other scientists (both in the workplace and in the discipline); by funding agencies; and by employers. These different audiences and markets have different concerns and requirements for information. The results of research must thus be formatted with respect to the needs of particular audiences. By 'formatting' in this sense, I mean organizing the appearance of the body of work, its results, and its interpretation.

Formatting ranges from casual conversation (for example, the explanation for the colleague in another field who says, 'So tell me what you're into these days') to extremely formal reports (for example, the funding agency demands that results be filed in a particular format and closely match the original grant proposal). Some formats require simplification of the research results. Others do not. For instance, certain kinds of publication standards demand more rigorous questioning of the data for more complex results; some formal presentations require specification of artefacts, complications and qualifications.

This section discusses pressures to format reports, popular speeches and articles, and to 'format' oneself for presentation and career purposes.

5a. Funding Agencies

Funding agencies demand results in special formats which may or may not fit the methodological or substantive requirements of the experimenter. They also demand results on preset schedules, which can act to compound this formatting pressure. Lab respondents were consistently concerned with the pressure from funding agencies to provide certainty of results, and to present those results in a manner which they considered over-simplified.

Grant applications, like publication space, afford only a small space within which to present results, compared to the large amount of available data. This comment by a respondent illustrates the problem (she is discussing a grant application with one of the other researchers at the Lab):

A: Leave out covariance, it's not clear.
B: If we include covariance we'll have to explain it, and we only have eight pages.

One respondent felt that the response to this pressure on the part of Lab staff was to conduct what he called 'opportunistic research.' He contrasted this with 'programmatic research'. The distinction here is between research which responds primarily to the concerns of funding agencies, both with regard to topic choice and selection of results, and that which sets its own goals and sticks to them despite the vicissitudes of funding.[31]

Preparation of reports and applications to funding agencies actually occur at several phases of research. While applications usually involve research design, they sometimes occur at later stages, in the form of continuing funding applications. With multiple funding sources, application and reporting procedures may be ongoing at all phases of a project. Similarly, multiple projects in one work site may have different funding trajectories.

In part, it is this mixture of trajectories which helps constrain formatting. The formats demanded by funding agencies are often geared toward single-source, single-trajectory research. Juggling multiple projects and agencies requires even more fitting, squeezing, and selecting to create compatible multiple formats.

Simplification results when the formats of the applications and reports must be geared toward the simpler, particular demands of one agency or application format.

5b. Pressure to Popularize

Partly as a result of clinical demand, partly as a way of seeking wide recognition, or spreading the word about science to popular audiences, many respondents participated in popularizing activities: conferences, writing articles for the popular press, newspaper interviews, and so on.

Such activities involved presenting complex scientific findings to audiences with little assumed knowledge of the issues involved, within a brief and highly constrained format. Simplification here involved selecting topics of interest to given audiences and presenting them in a brief time period. It also involved responding to audience demand for certain kinds of results. For instance, articles or lectures about right/left asymmetry in the brain have been in great demand. The public wants to know what asymmetry is and how to use different 'sides of the brain'.[32]

5c. Self-Marketing Pressures

Just as there is a pressure to popularize results, individual researchers feel constrained to present themselves in a saleable way. This involves calculating the 'coming thing' in terms of markets for skills, and a careful presentation of self to potential clients.[33] Thus, the individual scientist must gauge the current market for certain skills and simplify their self-presentation by stressing the most marketable one. Often, scientists must also explain their specialties to clients or potential employers, deleting technical terms (and thus sometimes information) or emphasizing only one aspect of the experience.

Self-marketing impacts the work at various phases. It is perhaps especially important at the problem selection phase, where calculating the coming market for individual careers may influence problem selection. During the publication/reporting phases, self-marketing involves formatting results for specialized audiences in order to accrue expert credentials. It may involve emphasizing or interpreting one aspect of the data to accentuate one part of the work commitment, either on an individual or group level.

6. *Editing*

Editing rules are standing agreements within a work site about what constitutes acceptable and unacceptable data. Editing can be either first level grammatical (for example, 'delete sneezes from the polygraph record', or 'clean specks of dirt off the computer tape'); or it can be more complexly aesthetic (for example, '*this* explanation is more elegant', or 'I'm not convinced that *that* one is scientific').[34] Editing can be ad hoc or rule-governed.

I observed three editing rules in the Lab. First, there was a decision rule for all data deemed contaminated by error: throw it out. Second, researchers selected the sample from which they drew data in anticipation of certain kinds of editing decisions: subjects were selected to create data which would be easy to deal with. This was a use of filtering rules at the design level: heading off certain kinds of 'messiness' before getting to the data collection/interpretation phases of the work. Third, other filtering rules were used during the data collection and interpretation stages in order to identify and reject contaminated data.

6a. *A Scientific Decision Rule: When in Doubt, Throw it Out*

A rule of thumb pervades science: all data contaminated by any error are discarded. Like the 'medical decision rule' described by Scheff,[35] it has the effect of systematically influencing decisions about the 'diagnosis' of the data at every level. This rule in the scientific work place has the effect of giving priority to any artefact in a data set. Although there may be good data in the set, the artefact or error there necessitates throwing out the entire segment. This rule is invoked when bias or error cannot be clearly demarcated, either by deletion of the artefact's effect or other filtering. It operates where information cannot be broken down into subcomponents for analysis *or* where it is assumed that components are too closely related to break down for analysis. Thus it is invoked when error or bias in one part of the data contaminates all of it. For instance, data about brain waves were broken down into one-second segments for representation on a polygraph print-out. These one-second segments of data were then analyzed separately by lab staff, who coded them for artefacts and amount of usable signal. If a short 'blip' appeared in one corner of the segment and it did not appear to affect the rest of the segment, it would be edited out and the rest of the segment used. In this case, the data could be broken down into subcomponents and it was assumed that something insignificant caused the blip. On the other hand, if the blip was spread out across the segment, or if it might have affected the rest of the segment by throwing off the reading, then the entire segment was discarded.

The decision rule operates at the interpretation and data analysis stages of the work. Decisions about 'contamination' or artefacts are an important part of the analytic process. The decision rule acts to simplify data by discarding information which would render results messy, and thus more complicated.

6b. Filtering at the Design Stage: Purifying the Sample

In anticipation of data which will have errors or artefacts, researchers at the Lab began with a sample of subjects who fit certain criteria. The subjects were all right-handed, white, male and middle-class, within a five-year age group. This screening process circumvented certain kinds of 'artefacts': left-handers in neurophysiological research tend to have ambiguous brain waves with regard to left-right differences, as do females and Blacks; lower-class subjects may have difficulty with some of the tasks being tested, due to differences in training. According to Lab staff, this makes it difficult to screen for differences in brain function.

Such designs have been criticized on political grounds because of systematic bias in the criteria by which groups are deemed 'messy' or 'ambiguous'.[36] But whatever the bias, sample purification has the effect of simplifying inference chains by reducing the number of steps and amount of qualification needed to reach a conclusion.

6c. Filtering at Later Stages: Identifying and Rejecting Contaminated Data

Identifying and rejecting contaminated data is an ongoing procedure in the work place: it involves learning and applying a set of editing rules designed to prepare the raw information coming from the subject, or recorded already on the polygraph, for analysis and higher-level interpretation. If a subject sweated heavily, sneezed, or did anything to add extraneous information to the brain wave readout during an experiment, the task was repeated and the information from the contaminated segment of data crossed out on the polygraph record. The information never appeared in the initial formal analysis on the computer. Later, segments of data were discarded by staff editors if they failed to meet statistical criteria or were judged to be the product of behaviour irrelevant to the task at hand (for example, the subject started to doze, and the resultant record simply records his sleeping brain waves).

Some of the rejection of contaminated data was automated. Electronic editors were programmed to delete segments with information in frequencies above or below a certain threshold. Implicit in this technique is a foreknowledge about what thresholds are found during ordinary behaviour in subjects, and thus which information is coming from another source (for example, muscle movement or electronic noise) and needs to be deleted.

Whether or not editing is automated, and no matter at what stage of the production it is used to filter out complications, the result is data which in some sense are simpler. Editing provides a set of standard operating procedures for data processing, by which the other forms of simplification noted here are achieved.

7. Specialization

Specialization is the appropriation of a set of tasks by one group: for example, by neurophysiologists whose specialty is the study of the limbic system. Locally, specialization in the Lab referred to the development of an individual's skills and work emphases: 'talk to John about hooking up the electrodes, that's his specialty'. Individually, staff had developed areas of expertise which were not pre-assigned on the basis of rank or discipline: reassuring subjects, detecting eye movement artefacts, setting up new computer programs. It was in specializations for detecting artefacts that I observed simplification processes.

Like doctors with 'favourite diseases', scientists sometimes have pet artefacts. These are artefacts for which a given researcher will habitually look; one respondent at the Lab, for example, was especially interested in (and thus sensitive to) artefacts stemming from muscle movements.

The effect of focusing on a 'pet artefact' can become the opposite of editing. That is, *more* time is spent examining a few of the difficulties in an inference chain before accepting the results. But because time is limited, such in-depth examination of one artefact or set of qualifications means that, in the end, others get ignored or slighted. Thus, the results become simplified, but with a stress on a particular artefact in the process.

Similarly, a line of work can develop pet emphases or problem areas. When attention is focused on one problem area, or on a technology devoted to one approach, everything outside the area of expertise may be 'black-boxed'. The resultant picture of the data becomes simplified by glossing over or discarding information outside the expert realm.[37]

Summary

Analytically, we may distinguish two types of simplification, based on the description of constraints presented here. Simplification *of* results, and simplification as a result of choice *between* results, differ in important ways.

The first type of simplification (of results) requires that work already done, or results acquired, be ignored (relegated to the back files, jettisoned, discounted). In this case, it is the work of building inference chains which is not recorded, or, having once been recorded, is now not presented. Ill-structured problems are thus transformed into well-structured problems by the expedient of ignoring progressive complications or qualifications. In some cases, they are simply declared to be well-structured and worked on as if they were.

The second type of simplification involves another kind of ignoring. The relationship of problems — whether well-structured or ill-structured — *to one another* is ignored. As scientists are forced to choose between results, but have no time to develop rationales or assess interconnections, they simplify the consequences of problems' relationships to one another.

The interlock of the two types of simplification gives us a clue about the cycle described in the opening of this paper: discovery-reification-description deleted.[38] A problem is first transformed into a well-structured problem by stripping it of complications. As results from this well-structured problem are chosen under time or funding pressures, scientists skip the work of weaving these results into an interconnected theory. A *fact* emerges which is simultaneously stripped of its complexities and isolated from its relationship to a larger work/historical context.

The concept of simplification and the various processes and constraints described here can be used to analyze complex scientific work patterns. It describes an important part of the construction of results which is deleted in the final, publication stage of work.[39] Simplification processes are routinely used in scientific research, and the attendant bias and noise in the conclusions cannot be analyzed without accounting for this and other work processes.

The variations and distribution of simplification across work sites need to be investigated. The distribution of these constraints as a result of funding, intellectual traditions, competition between sites, and large-scale political and institutional processes needs to be understood. In order to understand the nature of the relationship between scientific ideas and social factors, these need to be empirically examined.

• *NOTES*

This paper was presented to the annual meeting of the American Sociological Association at San Francisco, September 1982. It reports work done as part of a larger study of neuroscience research. I wish to thank Howard S. Becker, Herbert Blumer, Elihu M. Gerson, M.S. Gerson, Carl Hewitt, Gail Hornstein, Lynda Koolish, Michael Lynch, Anselm Strauss, Rachel Volberg, Tom Whitmore and William Wimsatt for comments and suggestions on earlier versions.

1. W. James, 'The Sentiment of Rationality', in *Essays in Pragmatism* (New York: Hafner, 1948), 3-36, quote at 4.
2. Ibid.
3. S.L. Star and E.M. Gerson, 'Management of Anomalies in Scientific Research: I. Varieties of Anomaly', and 'Management of Anomalies in Scientific Research: II. Properties of Artifacts', in preparation.
4. H. Simon, 'The Structure of Ill-Structured Problems', *Artificial Intelligence*, Vol. 4 (1973), 181-201.
5. Fleck's work, originally published in 1935, refers to this phenomenon in bacteriology: L. Fleck, *Genesis and Development of a Scientific Fact* (Chicago: The University of Chicago Press, 1979). More recent studies are B. Latour and S. Woolgar, *Laboratory Life: The Social Construction of Scientific Facts* (Beverly Hills, Calif.: Sage, 1979); and K. Knorr-Cetina, *The Manufacture of Knowledge* (Oxford and New York: Pergamon, 1981).

6. H.M. Collins, 'The TEA Set: Tacit Knowledge and Scientific Networks', *Science Studies*, Vol. 4 (1974), 165-86; B. Barnes and J. Law, 'Whatever Should Be Done with Indexical Expressions?', *Theory and Society*, Vol. 3 (1976), 223-37.

7. J. Dewey, *The Quest for Certainty* (New York: Minton Balch, 1929); J.P. Stanley and S.W. Robbins (Wolfe), 'Secret Agents and Truncated Passives', *Forum Linguisticum*, Vol. 2 (1977), 33-46; H. Garfinkel, M. Lynch and E. Livingston, 'The Work of a Discovering Science Construed with Materials from the Optically Discovered Pulsar', *Philosophy of the Social Sciences*, Vol. 11 (1981), 131-58; Knorr-Cetina, op. cit. note 5.

8. H.M. Collins and T.J. Pinch, *Frames of Meaning: The Social Construction of Extraordinary Science* (London: Routledge and Kegan Paul, 1982). See also Collins, 'Upon the Replication of Scientific Findings: A Discussion Illuminated by the Experience of Researchers into Parapsychology', paper presented to the 4S/ISA Conference, Cornell University, 4-6 November 1976; and G.D.L. Travis, 'Replicating Replication? Aspects of the Social Construction of Learning in Planarian Worms', *Social Studies of Science*, Vol. 11 (1981), 11-32.

9. B. Wynne, 'Physics and Psychics: Science, Symbolic Action, and Social Control in Late Victorian England', in B. Barnes and S. Shapin (eds), *Natural Order: Historical Studies of Scientific Culture* (Beverly Hills, Calif.: Sage, 1979), 167-84.

10. M. Lynch, ' "Turning Up Signs" in Neurobehavioral Diagnosis', paper presented to the American Sociological Association, San Francisco, California, September 1982.

11. C.S. Peirce, *Collected Papers* (Cambridge, Mass.: Harvard University Press, 1931-35), 6 Vols.; G.H. Mead, *The Philosophy of the Act* (Chicago: The University of Chicago Press, 1938); J. Dewey and A. Bentley, *Knowing and the Known* (Boston, Mass.: Beacon Press, 1960); A. Bentley, *Inquiry into Inquiries* (Westport, Conn.: Greenwood Press, 1975); J. Dewey, *Logic: The Theory of Inquiry* (New York: Holt, Rinehart and Winston, 1938).

12. In J. Dewey, A. Moore, H.C. Brown, G.H. Mead, B.H. Bode, H.W. Stuart, J.H. Tufts and H.M. Kallen, *Creative Intelligence: Essays in the Pragmatic Attitude* (New York: Henry Holt, 1917), 176-227.

13. Ibid., 197-98.

14. A summary of the historical synthesis and emergence of these viewpoints can be found in B. Fisher and A. Strauss, 'George Herbert Mead and the Chicago Tradition of Sociology (Part Two)', *Symbolic Interaction*, Vol. 2 (1979), 9-20, and in their article, 'Interactionism', in T. Bottomore and R. Nisbet (eds), *A History of Sociological Analysis* (New York: Basic Books, 1978), 457-98.

15. See, for example, H. Becker, B. Geer, E.C. Hughes and A.L. Strauss, *Boys in White: Student Culture in Medical School* (Chicago: The University of Chicago Press, 1961); Becker, *Sociological Work* (Chicago: Aldine, 1970); Becker, *Art Worlds* (Berkeley, Calif.: University of California Press, 1982).

16. See, for example, A. Strauss, *Professions, Work and Careers* (San Francisco, Calif.: The Sociology Press, 1971); and Strauss, *Negotiations: Varieties, Contexts, Processes, and Social Order* (San Francisco, Calif.: Jossey Bass, 1978).

17. E.M. Gerson, 'Scientific Work and Social Worlds', forthcoming in *Knowledge*; also Gerson, *Scientific Work Organization: The Population Realignment in Biology, 1880-1925*, in preparation.

18. See E.C. Hughes, *The Sociological Eye* (Chicago: Aldine, 1971), and G.H. Mead, 'The Nature of Scientific Knowledge', in Mead. op. cit. note 11, 45-62.

19. A full discussion of the method may be found in B. Glaser and A. Strauss, *The Discovery of Grounded Theory: Strategies for Qualitative Research* (Chicago: Aldine, 1967), later adumbrated in Glaser, *Theoretical Sensitivity* (Mill Valley, Calif.: The Sociology Press, 1978).

20. E.M. Gerson, 'On "Quality of Life"', *American Sociological Review*, Vol. 41 (1976), 793-806.

21. H.S. Becker, 'Notes on the Concept of Commitment', *American Journal of Sociology*, Vol. 66 (1960), 32-40. See also B. Geer, 'Occupational Commitment and the Teaching Profession', in H.S. Becker, B. Geer, D. Riesman and R.S. Weiss (eds), *Institutions and the Person: Papers Presented in Everett C. Hughes* (Chicago: Aldine, 1968), 221-34.

22. Gerson, 'Scientific Work and Social Worlds', op. cit. note 17.

23. Anselm Strauss discusses the general phenomenon of intersections and segmentations in social worlds in 'A Social World Perspective', in N. Denzin (ed.), *Studies in Symbolic Interaction*, Vol. 1 (1978), 119-28. See also R. Bucher, 'Pathology: A Study of Social Movements within a Profession', *Social Problems*, Vol. 10 (1962), 40-51, and Bucher and A.L. Strauss, 'Professions in Process', *American Journal of Sociology*, Vol. 66 (1961), 325-34.

24. 'Neurology' is the branch of internal medicine which deals with diseases of the nervous system; 'psychophysiology' is a basic research discipline.

25. S.L. Star, 'Sex Differences and the Dichotomization of the Brain: Methods, Limits, and Problems in Research on Consciousness', in R. Hubbard and M. Lowe (eds), *Genes and Gender: Pitfalls in Research on Sex and Gender* (New York: Gordian Press, 1979), 113-30.

26. Some of the philosophical differences between basic and clinical science are discussed in P.A. Heelan, 'The Nature of Clinical Science', *The Journal of Medicine and Philosophy*, Vol. 2 (March 1977), 20-32. A discussion of the clinical-basic interface and its effect on scientific work can be found in S.L.Star, *Scientific Theories as Going Concerns: The Development of the Localizationist Perspective in Neurophysiology, 1870-1906* (unpublished PhD dissertation, Dept. of Sociology, University of California, San Francisco, 1983).

27. T. Pinch discusses these issues in his 'Theoreticians and the Production of Experimental Anomaly: The Case of Solar Neutrinos', in K.D. Knorr, R. Krohn and R. Whitley (eds), *The Social Process of Scientific Investigation, Sociology of the Sciences Yearbook*, Vol. IV (Dordrecht and Boston, Mass.: D. Reidel, 1980), 77-106.

28. W. Wimsatt points out that rare effects, especially, tend to be screened out by time pressure and automated editing: private communation, March 1982.

29. This issue is discussed in another context by D. Light, 'Uncertainty and Control in Professional Training', *Journal of Health and Social Behavior*, Vol. 20 (1979), 310-22.

30. Such a situation is also subject to heuristic bias, which bears on the outcome of results: see W. Wimsatt, 'Reductionist Research Strategies and their Biases in the Units of Selection Controversy', in T. Nickles (ed.), *Scientific Discovery: Case Studies* (Dordrecht and Boston, Mass.: D. Reidel, 1980), 213-59. See also P. Slovic and A. Tversky (eds), *Judgment under Uncertainty: Heuristics and Biases* (Cambridge: Cambridge University Press, 1982).

31. Of course, the 'pure research' programme with no funding constraints or

material accountability is a fiction — nevertheless, the distinction the respondent is making here may be viewed along a continuum.

32. An example of a popularized version of psychophysiological research, including asymmetry, can be found in C. Hampden-Turner, *Maps of the Mind: Charts and Concepts of the Minds and Its Labyrinths* (New York: Macmillan, 1982).

33. Such carefully controlled presentation, of course, routinely takes place outside research or professional situations: see E. Goffman, *The Presentation of Self in Everyday Life* (New York: Doubleday, 1959). Latour and Woolgar's concept of the 'credibility cycle' also addresses this issue: see *Laboratory Life*, op. cit. note 5, esp. 200-08.

34. A discussion of editing work can be found in Becker, *Art Worlds*, op. cit. note 15. M. Lynch also pointed out to me the connection between editing rules and the ethnomethodological concept of 'rendering' an object — that is, expressing a phenomenon in terms of concepts developed in a scientific tradition: private letter, 20 March 1982.

35. The idea that for doctors 'it is better to impute disease than to deny it and risk overlooking or missing it': T. Scheff, *Being Mentally Ill: A Sociological Theory* (Chicago: Aldine, 1966), cited and paraphrased by E. Freidson, in *Profession of Medicine: A Study of the Sociology of Applied Knowledge* (New York: Harper and Row, 1970), 255.

36. R. Hubbard, M.S. Henifin and B. Fried, *Women Look at Biology Looking at Women* (Cambridge, Mass.: Schenkman, 1979); S.J. Gould, *The Mismeasure of Man* (New York: Norton, 1981).

37. Wimsatt, op. cit. note 30.

38. H. Blumer points out that the distinction between *discovery* and *demonstration* is crucial when discussing simplification processes. During the discovery process, there is uncertainty about the ultimate status of the phenomenon being investigated. In demonstrating a theory, the configuration of simplification processes has become standardized and is used simply to show an instance of an already-proven theory. Private communication, December 1982.

39. I am, of course, aware that my analysis also applies to the present paper!

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