Art and artifact in laboratory science

A study of shop work and shop talk in a research laboratory

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Preface

These prefatory remarks are actually written as a postscript to the volume that follows. The volume is a descriptive study of social activities in a neurosciences laboratory which focuses on how electron-microscopic phenomena were made sensually available and objectively accountable. It is based on fieldwork I conducted in the laboratory during 1975 and 1976. A draft of the volume was written by the end of 1978, and was accepted as my dissertation at University of California, Irvine in March, 1979.

I mention this because the situation in the science studies field has changed dramatically since the time I conducted the study and wrote the chapters in this volume. “Laboratory studies,” also known as “the anthropology of science,” have become something of a minor fashion in the sociology of science since 1978. At roughly the same time as I was doing my fieldwork, Bruno Latour and Steve Woolgar performed a field study of a biochemistry laboratory at Salk Institute, which they published in 1979, and Karin Knorr-Cetina performed an ethnographic study of a protein chemistry lab in Berkeley, California. I did not know of these other studies until after I had completed my fieldwork and had finished most of my writing in 1978. In addition to the coincidence of there being three concurrent and independent studies of laboratories in California, each described scientific practice in its “natural” setting in remarkably similar ways. This is not to say that there were no differences between the studies, but that their criticisms of previous accounts of scientific methods, and the alternative view of scientific practices they developed covered much common ground.

In addition to the studies mentioned above, a number of other laboratory ethnographies have been produced by Zenzen and Restivo, McKegney, Law and Williams, Collins, Pinch.
especially the American studies with which I was most familiar, seemed to have little bearing on my interests in laboratory practices and the methods I chose for pursuing those interests. In contrast, Harold Garfinkel and his students and colleagues had developed an approach to the study of work that directly guided my selection of topics, methods of study, and manner of exposition. This approach, known as ethnomethodological studies of work, is discussed in Chapter 1. Despite the changes in the sociology of science mentioned above, ethnomethodological studies of scientific work are still very distinct from all other variants in the science studies field, as should be evident by comparing the present volume to other laboratory ethnographies.

Ethnomethodological studies of work in the sciences are explicitly concerned with the technical production of order in specialized scientific and mathematical disciplines. They are exclusively pre-occupied with the production of social order, in situ, and not with defining, selecting among, and establishing orders of relevance for the antecedent variables that impinge upon “actors” in a given setting. Accordingly, readers of the present volume will notice, and some will be frustrated by the fact, that there will be little or no discussion of the general norms and reward structure in science, of “cycles of credit,” or of how the microstructure of the lab is connected to larger social and historical “forces.” The neglect of such issues will be complemented by a peculiar interest in the details of conversation and co-ordinated practice within the confines of a particular laboratory.

I have chosen not to devote extensive discussion to my theoretical motives for studying the details of laboratory talk and action. I hope that the descriptions and analyses of laboratory shop talk and shop talk that follow will be of sufficient interest that the reader will not demand a special warrant for why anyone would want to be concerned with such matters. Aside from the rather brief exposition in Chapter 1, I do not spell out such a warrant in advance, but present my reasons and arguments in a more or less piecemeal fashion throughout the body and notes of the volume. The vision of social order that animates this work is one which insists that reasons and justifications are occasional; that is, suited to the situation at hand and not to a transcendental context of inquiry. Therefore, I have attempted to refer to established scholarly themes and literary works in an almost tangential way; as they are touched off by descriptions of specific laboratory situations.

Newport Beach, CA

Notes
1 Latour and Woolgar (1979).
2 Knorr-Cetina (1981); also see Knorr (1977).
4 Zenzen and Restivo (1982).
5 McKegney (1979).
6 Law and Williams (1982); Williams and Law (1980).
7 Collins (1975) provides an early ethnography on the topic of replication, although his approach was mainly limited to interviews about scientific work. A body of later papers by Collins and his colleagues are presented together in Collins (1981).
9 Travis (1981).
14 Goodfield (1981) closely chronicles the life of an individual scientist but makes little attempt to encompass the social process of laboratory research.
15 Some of the better analyses of scientific discourse are: Morrison (1979; 1981), O’Neill (1980); Woolgar (1980); Mulkay and Gilbert (1982); Bazerman (1981); and Knorr and Knorr (1978).
16 See Medawar (1969); Holton (1978); Barnes (1974); and Bloor (1976; 1973), among numerous other programmatic treatments which are cited in later chapters.
17 Merton, in a classic article (1942) argues for a set of norms for science, while Mitroff (1977) formulates a set of “counter-norms.”
18 See Mulkay (1976) and Gieryn (1984) for discussions of ideological uses of normative versions of science.
19 Although in an early statement Merton (1952) called for case studies of science, case studies in the Mertonian tradition, such as Barber and Fox (1958) and Gaston (1973) do not critically examine technical activities in science in any way comparable to the later ethnographies in the constructivist and ethnomethodological traditions.
20 A number of written works by members of that community were completed since 1978, and are not given the ample coverage they deserve in this volume. These include Livingston’s study of mathematicians (1983), Morrison’s studies of written accountability in science (1979; 1981), Garfinkel et al.’s study of the optical discovery of a pulsar (1981), Wieder’s study of primate experiments (1980), and Schrecker’s study of didactic experiments in chemistry (1980). Some of these studies are discussed in Lynch, Livingston and Garfinkel (1983). Representative works by the above authors are scheduled to appear in Garfinkel (ed.) (forthcoming).
21 Merton (1942).
22 See Latour and Woolgar (1979), ch. 5, “Cycles of Credit.”

1 Introduction: methodological issues in the study of scientific work

This volume is a study of science which confronts its topic in the shop work of a particular brain science laboratory. Throughout the following chapters scientific inquiry is identified with the production of technical work and technical talk on the lab floor. The fundamental matter of interest in this study is the phenomenon of the social accomplishment of natural scientific order. This topic entails an examination of the substantively scientific work and technical talk in the laboratory as a proper object of sociological inquiry. It is proposed in this study that the detailed contents of scientific accounts, as they implicate a referential environment of scientific objects and events, are socially organized in ways that do not merely reiterate formulations of scientific method. It is the aim of the study to substantiate the possibility of an analyzable social basis in the local production of accounts of natural objects in laboratory research.

The analyses of scientific shop work and shop talk which are presented in the following chapters are based on a field study of a particular university research laboratory. I was given the opportunity to visit this biological sciences laboratory in January 1975 through arrangements made by a professor in the School of Social Sciences at the University of California, Irvine who was a friend of the lab’s principal investigator. I visited the lab several times a week for a six-month period after making the initial contact. These visits continued on a less frequent basis for another year and a half. During this “field work” I attempted to learn as much as I possibly could, given the amount of time I had available and given my limited background in the biological sciences, about the natural science practices that composed the lab’s research and training programs. I was given a rather informal course of training in the substantive and methodological features of the lab’s
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research into a regenerative process in the brain. During this training I took the opportunity to inquire into several topics on the social organization of lab inquiry. These topics were informed and enriched during my attempts to understand the technical details of talk and conduct in the lab environment and were not strictly founded on a naturally theoretic interest in the "social aspects" of science.

During my earlier visits to the lab facility I was given "tours" by lab members who were kind enough to show me the various instruments and data formats used in the lab work and to give me patient explanations about the lab's research problems and methods. At this time I was also told about informal aspects of lab work which were not available in reports of research findings or typified descriptions of scientific logic and method.

After these "tours" I focused on a particular specialized area of the lab's researches, the electron microscopic documentation of a neural regeneration phenomenon, "axon sprouting." It was impossible for me to monitor the total range of technical approaches which were used in the lab, and I found that "specifying" my inquiry to a single one of the lab's several competences facilitated a more detailed understanding of the shop work and shop talk. Even within the narrow area I studied, my competence with electron microscopic work never approached a practitioner's skills with preparing brain tissues for microscopic photography. I was unable to participate in the lab's researches, though I achieved a competence in some of the analytic skills used in assembling and interpreting electron microscopic displays of brain tissues. These limited competences gave me considerably more access to the talk and conduct which I witnessed in the lab than would have been possible had I relied solely on the analytic skills of a social scientist while observing members' activities.

While I was engaged in the largely practical inquiry of learning about brain research in the lab, I compiled ethnographic field notes and audiotape recordings. In the field notes I kept a journal of the particular troubles I had in learning about the lab's researches, as well as a daily log of events which struck me as particularly interesting. These notes were not organized under any well-defined set of topics, though they tended to focus on events in the work which were conspicuously tied to the local circumstances of the work. Such matters as arguments over visible microscopic phenomena, mistakes and other "troubles" in the routine work, remarks about research artifacts, and expressions of uncertainty by practitioners were of particular interest because of the way they exhibited features of scientific work which often go unmentioned in written accounts of scientific theory and method.

I also made note of sequential aspects of the ordinary work of lab members as they went about their daily tasks. These temporal features of the work's performance were interesting to me for the way they showed an actual order to the performance of "method" in contrast to the schematic order of a "methods" recipe.

Tape recordings were made of spontaneously occurring conversations between lab members while they were engaged in lab practices. In my analysis of these tapes I focused on those conversations which had a demonstrable relevance to the collaborative work being performed along with the talk. This shop talk was analyzed for how it was integrally part of the courses of action which made up the daily work of the lab, as that work involved substantive determinations of neural events and structures.

This field experience gave access to features of natural scientific practice which are not elaborated upon in scholarly treatments of scientific theory and method. In addition, such features are not reported in methods sections of scientific research papers. Methods reports supply step-by-step maxims of conduct for the already competent practitioner to assimilate within an indefinite mix of common sense and unformulated, but specifically scientific, practices of inquiry. These unformulated practices are necessarily omitted from the domain of study when science studies rely upon the literary residues of laboratory inquiry as the observable and analyzable presence of scientific work.

Science studies

Almost without exception, philosophical accounts, biographical stories, historical interpretations, and "empirical studies" in the science studies literature address the practices of science as achievements to be explained in a language disengaged from any laboratory task at hand. 1 It is not as though these accounts deny that science is accomplished in laboratory settings, but that they do not provide detailed access to the practical achievement of day-to-day inquiries. 2 They instead give decontextualized versions of methodic production and logical reasoning, reconstructions of citation networks and historical lineages, after-the-fact interpretations of historical records, remembrances of prominent figures in science, and abstracted "social aspects" of science's institutional and administrative organization. 3 These studies do not, so to speak, take the reader into the contemporaneous achievement of science as it unfolds in the daily routines of shop work. It is as though what scientists do qua scientists in their day-to-day practices is presupposed in the literature as an unproblematic working
out of the methodological details of a science which is understood more generally.

Although it is not the task of this volume to systematically confront or refute romantic ("storybook") or formal-rational versions of science in the science studies literature, the observations of scientific work which are reported here are perspicuous for the incongruities they reveal between science as practical action and science as presented in extant science studies. These contrasts are elaborated upon throughout the volume as they are brought into specific relevance in discussions of particular phenomena in the production of laboratory shop work and shop talk. These contrasts are cited here only for the purpose of introducing the proposal that a study of the locally organized details of scientific conduct in a laboratory yields far more than an empirical "filling out" of common understandings about science expressed in the science studies literature.

A few of these contrasts are suggested in the following list of redefined concepts. These terms which are common to literary formulations of "science in general" are "redefined" as a way of bringing the reader into the specific inquiries which follow. They are not presented as operational definitions stipulated in advance of the research and used as theoretical resources. They rely upon the already completed studies reported in the following chapters and should be read as a textual device for orienting readers to what will follow.

1 **Scientific community:** in this study the community of science is identified in the local scene of its technical production in shop work. Such communities as professional associations, academic disciplines or sub-disciplines, and "invisible colleges" are treated here as "exterior" to the laboratory community. In light of the amount of attention paid to these exterior communities it is curious that the place where the labor of science is performed has rarely been visited in studies of science and investigated in terms of its productive relation to the material domains of scientific inquiry.

2 **Scientific facts:** in this study scientific facts are viewed as outcomes which are inseparable from the courses of inquiry which produce them. Throughout the following chapters, scientific facts are treated as social accomplishments. This contrasts with treatments of facts as either independent natural objects or constructs of a theoretical imagination. Instead, facts are viewed here in the context of the practical activities of discovery and description. These activities of discovery and description among collaborators in lab projects are treated as substantive phenomena for a social inquiry.

3 **Scientific inquiry:** "inquiry" is identified in this study with the tangibility of technical labor and technical talk, in contrast to abstract chains of inference which make up "hypotheses" which are temporally isolated from the ways in which they are tested or "worked out" in the practical domain. Instead, technical work and technical talk are described here as occasions for "reasoning" or "planning" in the course of projects of inquiry.

4 **Scientific methods:** methods are described here as particular courses of action with materials at hand, instead of as step-by-step programs. The difference between methods observed in this manner and methods formulated as standardized programs is that routines of conduct are described here complete with their interruptions, errors, repairs, and occasional abandonment. Accordingly, regularities in conduct are formulated, not as stipulative ideals of error free procedure, but as practical constituents of actual instances of the performance of projects in real time. The sequential organizations supplied by schemes of conduct as they are used within occasions of performance as recipes or "instructions" are not disregarded in this approach. However, these formulas are not appropriated as paradigms which programmatically describe each trial of their repetition. Instead each trial is observed as a unique and at times vivid accomplishment (vivid, often in light of unforeseen discoveries and foul-ups arising from the non-compliant character of the materials at hand). That any particular run of an experiment is achieved as a "run" in a corpus of runs is attributed here to the achievement of its singular performance and to assessments of the adequacy of the particular run as an instance within the corpus. The work of assessing the practical adequacy of any this-time accomplishment is observably part of a method's performance.

Despite the contrasts outlined above, these remarks do not make up a general critique of any or all of the existing theoretic divisions within the science studies literature. Particular accounts in that literature are interesting and deeply illuminating in their general discussions of science. There is no intent here to denigrate the achievements which some of the great works on science represent. What the above contrasts point to is a phenomenon which is largely indifferent to the well respected theoretic and analytic approaches to studying science; a phenomenon that is nevertheless orderly and observable when established provisions for "methods" are radically re-examined. Analytic or observational methods used in this research are, therefore, not derived from extant studies in the literature on science. Since that literature does not direct its inquiry to the ordinary settings of scientific shop work, its discus-
sions are little more than suggestive for an attempt to engage that phenomenon as a topic of study. None-the-less, the common and yet distinctive science which is addressed here is an observable and analyzable subject matter, though it is by no means unproblematic as such.

**Ethnomethodological studies of work**

This study’s thematic and methodological initiatives are taken from a specific corpus of ethnomethodological studies of work. Not every study which rides under the banner of ethnomethodology is equally exemplary, however. It is difficult to characterize the corpus of ethnomethodological studies of work under a uniform definition either of a “field” or of a “method,” or of a “unified theory.” In fact, these studies are distinguished by their irreverence to such terms of characterization. They are produced as exemplars though not through any abstraction of a general formula or theoretical commitment which defines ethnomethodology.

Fundamental initiatives are taken from the studies of Harold Garfinkel and his associates. These studies are distinguished by their attempts to situate themselves within the settings of work they describe. Garfinkel’s maxim of the “unique adequacy” of methods locates a concern in these studies to operate from within the competence systems they describe. Accordingly, their descriptions of orderly and socially organized inquiries do not present an opposition between the practices described and the practices which make such description possible. Instead, the studies recognize the prevalence of inquiry as the phenomenon they confront in their entry into the scenes of work’s accomplishment. In presenting accounts of these competences ethnomethodologists variously seek to exhibit those socially located inquiries in such a way as to bring the “reader” into the common settings of practical conduct. Such demonstrations entail more than a stipulation of rules of conduct, or of theoretically informed interpretations of social actions in the particular settings studied.

Ethnomethodologists do not go into their researches methodologically “empty-handed,” as a corpus of prior studies of work is now usable as a basis for projecting further studies. This basis is not a firm and stable outline of analytic rules and techniques, as the shape of its suggestiveness often emerges in-the-course-of or consequent to particular researches. Recent work in ethnomethodology, however, is characteristic in its use of some general strictures involving the “material demonstration” of the practices of inquiry studied.

As a way of imposing restrictions on the use of stipulative conjecture in the detailing of what count as competent inquiries in the scenes of work studied, ethnomethodologists’ studies converge on the use of *records* of those inquiries as both analytic “materials” and exhibits of discoveries. What counts as an adequate record of “naturally occurring” instances of conduct or inquiry is a topic of considerable debate, though studies of work tend to exemplify the use of record formats which in some manner recover the visibility of the work in the setting of its accomplishment.

The use of records as exhibits of work in its setting is not a matter of relying upon a set of “data collection” techniques. For instance, the analyst cannot unrestrainedly rely upon research technologies such as audiotape and videotape as guarantees of an adequate record of work in its “natural” setting. Videotape and audiotape are situated exhibits of actions, and their use in detailing constituent structures of any particular order of work requires an argument for how that work is visible *in that way* as a consequential production of the setting studied. The analytic use of the video record or audio record requires a demonstration of the relevance of bodily movement and talk as a repository of the detailed make-up of the work’s accountability.

In many settings it is the embodiment of speech and gesture which provides work with its visibility for practitioners. Accordingly, videotape and audiotape are used in studies detailing the circumstantial production of such phenomena as “university lectures,” “conversations,” and “courtroom proceedings.” In other studies the immediate presence of speech and gesture is not exhibited as the prime locale for the work analyzed. Instead, the work’s discovery and display are rendered in terms of the competent organization of details in other textual formats. A collection of ethnomethodological studies uses organizational objects such as truck drivers’ log books, analysts’ transcripts of conversation, and blood alcohol readings in investigations of specific work settings. These records are not appropriated as descriptive data on work practices. Instead, they are examined for how they comprise scenic conditions for the analyzability of work within an organizational setting. The analysis of such records is viewed in studies as a practical accomplishment of those persons in the organizational setting whose work involves the design and reading of the documents. That, for instance, a blood alcohol reading is used as a legal document of “drunkenness” or a truck driver’s log book is used in detecting violations of I.C.C. truck driving regulations is treated as an occupationally specific competence. The organizationally competent interpretation of
such records is itself the phenomenon of interest, rather than being implicitly used by the ethnomethodologist as a resource for "finding out" about truck driving or drunken driving. Furthermore, the conditions for an adequate interpretation of such records are sought in these studies within the practical settings where the documents are employed as grounds of inference and action. This contrasts with the use of scientific rules of interpretation as an authority for the competent interpretation of the records. Accordingly, no attempt is made to improve upon the outcomes of the practically managed interpretations of organization members. Instead, ethnomethodologists attempt to delimit the organizationally specific ways in which the records are used.

In other studies written texts such as poems are analyzed. The analysis does not, in these cases, conform to literary conventions of textual interpretation. Instead, the texts are analyzed for how they are employed within occupationally specific competencies of "reading" and "writing." These studies assert that a competent analysis of the texts cannot be isolated from the practical states of affairs which employ the documents as instructions or exemplars of adequately performed work. The studies propose that a strictly "literary" analysis of such texts cannot assure the analyst of a grasp of the organizational practices in conversational turn-taking and sequential design which are analytically visible in varieties of specific work situations. The analyst, in addressing the shop talk of inquiries as conversation, renders the talk visible and recognizable as conversation with its full repertoire of analytic features of questions, answers, methodic ways of insulting, requesting, agreeing, etc. Furthermore, these abstracted "devices" of conversation can be demonstrated as part of shop work through the use of transcripts of tape recorded "materials" from the work setting. The findings analytically demonstrated in this fashion are pervasively relevant to the work in such settings, though not, to borrow Harold Garfinkel's usage, the "just what" of the work. The "just what" of the work specifically eludes any account which subsumes an analysis of shop talk within a general conversational analytic. This is not a fault of conversational analysis, per se, since conversational analysts study those occasions of concerted speaking which are visibly "having a conversation," "asking a question," "doing a greeting," and so forth. However, the application of those findings to work settings cannot fail to generate further analytical specifications which do address the work as it actually appears, but which do not specify how the work is uniquely that work and not some other instance of "conversation."

I speak of this resource as "double-edged," however, since the wholesale application of results from studies of conversation to investigations of specific work situations provides an easy way to produce analytical findings while leaving the specific and substantive character of the work being done in-and-through the conversation un explicated. In elaborating upon the situated use of generic conversational structures the analyst recapitulates sociology's extrinsic interest in work by using a corpus of analytical "structures" from "society in general" (or "ordinary conversation" in this case) to account for specific instances of conduct. Such a practice guarantees an analytical distance from the work's detailed accomplishment as a specific feature of those settings.

Conversation is ubiquitous wherever persons in society perform concerted actions in each other's verbal presence. Because of this fact, studies of conversational structures offer a general relevance to varieties of specific work situations. The analyst, in addressing the shop talk of inquiries as conversation, renders the talk visible and recognizable as conversation with its full repertoire of analytic features of questions, answers, methodic ways of insulting, requesting, agreeing, etc. Furthermore, these abstracted "devices" of conversation can be demonstrated as part of shop work through the use of transcripts of tape recorded "materials" from the work setting. The findings analytically demonstrated in this fashion are pervasively relevant to the work in such settings, though not, to borrow Harold Garfinkel's usage, the "just what" of the work. The "just what" of the work specifically eludes any account which subsumes an analysis of shop talk within a general conversational analytic. This is not a fault of conversational analysis, per se, since conversational analysts study those occasions of concerted speaking which are visibly "having a conversation," "asking a question," "doing a greeting," and so forth. However, the application of those findings to work settings cannot fail to generate further analytical specifications which do address the work as it actually appears, but which do not specify how the work is uniquely that work and not some other instance of "conversation."

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This issue of "unique adequacy" in the use of a "material" record of work is not a question of empirical grounding of a theory. Any of the varieties of records which are available at the scenes of work, or which are produced through the constructive involvement of the analyst within the scene (tape recordings, ethnographic field notes, films, photographs, interviews, questionnaires, etc.) is adequate as "grounds" for particular claims and arguments on what these "data" reveal. What is raised as a problem here is not a matter of the validity or reliability of such
records of work. Instead, the problem is a matter of what counts as a record of work, or as the visibility of the work, in the setting of that work's accomplishment. It further involves issues of what ways of addressing such records reveal the work's observable and reportable detail in a way that would not initially idealize those details through the devices of extrinsic description.

Circumstantial accounts of scientific work
Throughout this volume I attempt to address the shop work of science in the fashion of a material demonstration. I present transcripts of conversations in the lab and address those transcripts analytically. In doing so, this analysis is not exempt from what was said above about the "double-edged" character of conversational analysis. However, in presenting these transcripts I do not analyze them as talk per se; instead, I present them as accounts or readings of another record of data. This other record which is alluded to in this analysis of shop talk is the electron micrographic rendering of substantive brain events which was used as data in a particular lab project.

In my encounter with the shop work in the particular lab setting, I ran into an interesting problem on the issue of what would count as an adequate record for displaying that work. Scientific work in its "natural setting" is encumbered with records of all sorts, all of which give that work their particular version of accountability. Shop work has an embodied visibility to persons manipulating items of apparatus, laboratory specimens, and preparatory chemicals at the work bench. It has the visibility of shop talk: talk which accompanies the work as that work is underway; not talk about the work but talk in the work, talk which is part of the work. It exists in the formats of extrinsic description: "tours" for visitors, lab reports of projects which give coherent accounts and explanations of courses of inquiry. It also appears as numerous "data formats": micrographs, oscillographic readouts, and graphic displays of statistical patterns. These records are not identical to verbal "reports," since the data-documents exist as indefinitely interpretable grounds for speaking. Such records are appropriated to arguments over what they show, and are referenced in the working-out of agreements. In addition, the construction of these data formats exhibits the painstaking work of rendering "the laboratory animal" of "the specimen" into an accountable and analyzable configuration. The data are and are not the things studied per se; they are conditions within which "the things" are addressed in particular instances of shop work.

It is the formats of visibility constituting "data" that are considered here to be the most critical exhibits of the work of science, rather than the bodily visibility of the work, or abstracted sequences of talk taken in isolation from their implicativeness for the actual or projectable formats of the "data". However, the "data" are not of interest here in isolation from the actions that compose and analyze their observable features.

What are considered here to be meaningful exhibits of "the data" are such instances of the data under an argument, the data under a reading, or the data as documents of an announcement or claim, as can be witnessed in actual occasions of lab work. In all such instances the data are appropriated within a particular action by one or more lab members. The data elaborate the action (the "claim," the "reading," etc.), while themselves being elaborated. The data are inseparable from the situations of their usage.

In this volume (especially in Chapters 4 and 7) I make continual, though often conjectural, reference to how specific actions of lab members make use of the data of a project. The data I usually speak of in these references are electron microscopic montages of photographs of brain tissues.

That "what the data show" is a circumstantial accomplishment is very conspicuous in electron microscopy. Electron micrographs of brain tissues are so completely abstracted from familiar appearances that persons who are unfamiliar with brain science or electron microscopy have nothing to say about them other than that they, for example, "look like abstract art," or are "strange looking." This is not so true of some artistic uses of scanning electron microscopy in depicting insects as huge, ultra-realistically appearing monsters. Such photographs are done at relatively low magnification and involve recognizable objects for most lay persons. In electron microscopical research on brain tissues, the constituents of the cells have no familiar physiognomy as such, other than to persons familiar with the interpretive enterprise. In microscopical work at high powers of magnification one departs from the visibly familiar and confronts a specific scientifically discovered system of entities.

This can be demonstrated by examining Robert Hooke's detailings of the structure of insects in Micrographia, in which an exceptionally large flea is drawn with amazing detail. If this is compared to Leeuwenhoek's drawings of "animalcules," and viewed in the context of their startling character to his contemporaries, Hooke's detailings appear relatively unproblematic. Where Hooke's account of the flea is an enlargement of an ordinary, though "small," object, Leeuwenhoek brings his contemporaries into the presence of a new object with his more highly...
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magnified account. The new objects are discontinuous with the ordinary details of visible entities at that time. The "animalcules" were not sought in Leeuwenhoek's researches, but appeared while he was investigating the sub-constituents of water. Leeuwenhoek ran into verificational troubles which Hooke never encountered with his renderings of insects, though aspects of both accounts are now unproblematically situated in common-sense understanding.

The disjunctive "jolt" to lay experience which is reconstructed above in the case of Leeuwenhoek's accounts can briefly manifest in a naive encounter with electron micrographs of brain tissue (a copy of such a micrograph is displayed in Figure 1.1). Under the guidance of a set of instructions one quickly gains a familiarity with the visibility of the rendering, and the organization of entities emerges in a way that is no longer shockingly disjunctive or dully senseless.

These photographs are operated upon within the lab as phenomena which are accountably present in inquiries. Furthermore, the initially strange character of these photographs to the un instructed reading locates the accountable (reportable-observable, "instructed") reading of the document as a specifically scientific achievement. It is this that is taken as the phenomenon here: accounts of objects in relation to such records.

How, then, is a record like a micrograph a record of work, of relevance to a sociological account, instead of being a picture of real-world entities of interest to biologists? It is argued in Chapter 4 that a particular reading of microscopic records on some occasions in the lab work addressed the record's artifactuality, and that such circumstantial analyses warranted an approach to such displays as accounts of work. That argument will not be elaborated upon any further at this point, other than by saying that in the analysis of artifact accounts the phenomenon of study will be records-under-analysis; rather than, for example, "interpersonal interaction" or the "text" of the microscopic photographic layout.

In this volume (particularly in Chapter 7) transcripts of shop talk are analyzed in terms of some conversational "structures" used in agreements and disagreements. However, this analysis provides for the referential contexts of the lab members' remarks by citing particulars in the electron microscopic rendering of brain tissues. This work of "contexting" the particular conversational fragments implicates those fragments as something other than "usual" conversational discourse, though I am not very satisfied with the analytic stipulation of "contexts" as a solution to the unique adequacy problem. The trouble with this treatment is that ethnographic particulars are stipulated as aids to a credible hearing of the talk without giving alternative access to members' work

Key

| a = astrocyte protoplasm |
| d = dendrite |
| d.g. = degenerating axon terminal |
| mic. = microglia protoplasm |
| mit. = mitochondrion |

Figure 1.1 Electron micrograph of brain tissue (approx. 22,000 X)
with the electron microscopic documents at the scene of their talk. This credibility is enhanced by my relative competence at analyzing electron-micrographs for their ways of exhibiting accountable entities and artifacts. However, what is being raised here is not a question of whether the stipulations which are supplied in the analysis are "accurate"; instead, it is an issue of how this competence is exhibited as an unexplained aid for hearing sensible shop talk. A more adequate way of exhibiting how the bodily work of members elaborates an electron microscopic "environment" in particular instances of shop work has not yet been fully worked out at this point in the research.

The body of this volume is divided into two major parts. Part I, "Ethnographic accounts of shop work," consists of two chapters on the local visibility of laboratory shop work which are preceded by an ethnographic account of the "background" details of the lab setting.

Chapter 2, "The lab setting," presents an "ethnographic context" for the discussions and exhibits in the chapters which follow. This chapter briefly treats such matters as the layout of the lab facility, its personnel, and projects of study. The account characterizes the lab's naturalistic understanding of a particular research project on the regenerative capacities of brain tissues. Selected features of the lab's account of this phenomenon, "axon sprouting," are given to familiarize the reader with referential aspects of laboratory shop talk. Contrasts are drawn between this preliminary fashion on how projects are generated from within the lab setting and the way they are exhibited in discussions of laboratory tours and research reports. These settings are characterized by the way they exhibit lab work in a descriptive and recipient-designed format. In contrast, "talking science" is negatively characterized as "strange" to a non-practitioner's comprehension. Features of that "strangeness" are then detailed by relating the referential clarity of shop talk to the specifically scientific settings of conduct it elaborates.

Chapter 3, "Projects and the temporalization of lab inquiry," focuses on the organization of laboratory actions into projects of study. On the basis of field-noted observations taken during the course of a particular inquiry a number of issues are addressed in preliminary fashion on how projects are generated from within lab members' day-to-day work. Contrasts are drawn between this in situ organization and formulations of projects as step-by-step enactments of methods schemes. The phenomenon of "troubles" is treated as an identifying feature of the local accomplishment of tasks in the situated details of shop work.

Chapter 4, "An archeology of artifact," explicitly appropriates research "troubles," and "artifacts," as a way of entry into the social accountability of laboratory productions. Instances are discussed where members' accounts of artifacts provide readings of naturalistic data which definitely or indefinitely invoke a generative context of practical circumstances in lab work. These inquiries into the practical accomplishment of objects of study are thereby identified as reflexive achievements within ordinary situations of laboratory work. An ordered progression of artifact accounts is then presented in exhibiting instances of those inquiries as they were observed during lab projects. These exhibits are ordered in terms of how they demonstrate a circumstantial analysis of laboratory work.

Part II, "Agreement in laboratory shop talk," presents a series of chapters on "achieved agreement" in laboratory shop talk. Chapter 5 opens the discussion of "Laboratory shop talk" by contrasting "literary accounts" of science with talk which is accountably present in scientific work. The science studies literature is characterized in terms of its treatment of science as a written, general, and "monumental" phenomenon. A contrast between "talk about science" and "talking science" distinguishes two distinct modes of discourse within scientific enterprises. "Talk about science" is exemplified in discussions of laboratory tours and research reports. These settings are characterized by the way they exhibit lab work in a descriptive and recipient-designed format. In contrast, "talking science" is negatively characterized as "strange" to a non-practitioner's comprehension. Features of that "strangeness" are then detailed by relating the referential clarity of shop talk to the specifically scientific settings of conduct it elaborates.

Chapter 6, "Two notions of agreement," opens with a discussion of "achieved agreement" by contrasting that notion with a version of agreement which is prevalent in social science accounts. This latter version of agreement, "implicit agreement," is characterized as an equivalence relation between isolated statements and activities which is postulated in social science treatments. "Achieved agreement" is distinguished from "implicit agreement" on the basis of its being a relation between accounts and activities which is asserted in immediate situations of members' conduct. While studies in the sociology of science rely predominantly on stipulations of "implicit agreement" in their analyses of scientific accounts, this study explicates scientific shop talk with a focus on "achieved agreement."

Chapter 7, "Objects and objections: modifications of accounts of objects in laboratory shop talk," continues the treatment of "achieved agreement" with a demonstration of a particular analytic practice which is relevant to agreement in conversation. This practice, "modifications of object accounts," is exhibited with the use of transcripts from ordinary conversations. Several "devices" are isolated in the analysis of these transcripts, and are used in describing ways in which parties in conversation reassert initial accounts of objects while systematically adapting those
accounts to interactive situations. A line-by-line analysis of several transcripts of talk recorded in the lab setting is then presented. These conversations are analyzed in terms of their ad hoc referential use of an environment of lab practices and ultrastructural cellular objects. Detailed attention is paid to how the accounts of lab members and the modifications those accounts undergo in disagreement sequences make use of the phenomena in the lab’s axon sprouting inquiry.

Chapter 8 concludes the volume with a discussion of several problems in the relationship of a social science inquiry to natural science practices. These problems are not addressed through a stipulation of general criteria of similarity and difference. Instead, the research experience formulated in earlier chapters is drawn upon in order to raise the issue of how adequate access to the social order of the lab is inseparable from a competence the technical practices used by lab researchers.

Notes
1 I discuss particular texts in the science studies literature in a more extensive fashion in later chapters (especially Chapters 4, 5, 6, and 8). Since I am not using that literature, or any part thereof, as a corpus of precedents which authorize the treatment here, I prefer to introduce particular texts into the discussion as I go along in later chapters. In doing so I use these texts to illuminate specific issues which arise in my treatment of laboratory shop work and shop talk. Some of these texts (particularly those by Holton, Feyerabend, Kuhn, and Polanyi which are cited in later chapters) make programmatic statements which I cite in regard to specific issues at hand. My “agreement” with these texts does not vitiate the point that the science studies literature does not exemplify the sort of approach to scientific work that I embark upon here. Whether I agree or disagree with the passages in any of these texts, I observe that they address scientific work in ways other than by situating their inquiries within an analytically topologized setting of practical actions in scientific shop work. I do not wish to deny the relevance of historical studies or philosophical speculations for our knowledge about “science in general,” but I maintain that such treatments do not motivate the approach taken here.
2 As indicated in the preface to this volume, the above remarks about the science studies literature pertain mostly to American studies before 1978. A number of recent studies were published, or came to my attention, only after the body of the manuscript was written, so that their impact on the study of science is not fully appreciated in the text as it stands. More extensive discussion of how this study and other ethnomethodological studies of scientific work compare to recent ethnographies and studies of scientific discourse is presented in Lynch (1982a) and Lynch, Livingston and Garfinkel (1983).

Despite the fact that “laboratory studies” have now become an established part of the sociology of science, there are major differences between the collection of ethnomethodological studies in Studies of Work in the Discovering Sciences (Garfinkel, ed., in press), and the ethnographies and analytic studies of scientific discourse. Ethnographies by Latour and Woolgar (1979), Knorr-Cetina (1981), Edge and Mulkay (1976), Collins (1975), Zelenza and Restivo (1982), Whitley (1978), Law and Williams (1982), and McKegney (1979) show heterogeneous commitments to on-site investigations of scientific reasoning and practice. Some of the studies, particularly those by Knorr-Cetina, Latour and Woolgar, Collins, and Mulkay express a degree of affiliation to ethnomethodology’s programmatic concerns that is absent in the others. Resonating concerns are evident in the way all of these studies investigate specific cases of scientific reasoning and practice. They differ remarkably from generic and hypothetical accounts of Science or The Scientist. However, none of the ethnographies compare to the ethnomethodological studies of work in the depth of their interest in the embodied and technical production of scientific practices. Instead, the studies critically distance themselves from technical competences while adhering to social science and interpretive models of scientific activity. Such models are disengaged from the work they analyse, since their use and understandability is reflexive to social science competences which bear little resemblance to the requirements for mastering the details of commonplace laboratory tasks. Concomitant with such “critical” distance is the absence in the ethnographies of thorough examinations of the temporal production of competent laboratory practices. These studies of “actual cases” thereby miss the phenomenon which ethnomethodological studies treat as the matter of primary interest; the mastery of technical discourse and practice.

Analytic studies of scientific discourse by Morrison (1979, 1981), O’Neill (1981), Woolgar (1976; 1980), Gilbert (1976), Mulkay and Gilbert (1982), Anderson (1978), Bazerman (1981), Callon, Courtial, and Turner (1981), Bastide (1983), Shapin (1985), Gusfield (1976), and Knorr and Knorr (1978) are refreshingly suggestive in their approach to scientific writing as something other than objective descriptions of methods and findings. These studies have opened up the topic of the rhetoric of objective discourse and its local uses in science. Morrison’s detailed studies and O’Neill’s more programmatic inquiries are noteworthy for the way they suggest how the coherence of disciplinary specific reading and writing is embodied in the material specifics of a text. This emphasis on the situated and disciplinary production of reading aligns Morrison’s (1979) analysis of a biochemical demonstration with the ethnomethodological studies of work. However, none of the studies...
of discourse in this collection closely tie scientific discourse to its laboratory site, and their analytical use of written and oral discourse bears a problematic relation to the natural science inquiries productive of the discourse. Their relevance to the study of laboratory shop work is limited by their use of written formats and/or interview materials, though some of their findings allow for a contrast to be made between laboratory vernacular (Senior, 1958), and formal writing and speech.

I have given short shift to the empirical studies in the Mertonian tradition such as Gaston's field study of high energy physicists (1973), Zuckerman's interviews with Nobel Laureates (1977), and the many survey-analytic and sociometric studies of scientific communities. These studies are explicit and rigorous in their separation of "technical" aspects of science from the "social" aspects they take up for study. The studies thereby fail to address the disciplinary tasks and troubles that make up daily life in the laboratory, and which are by no means exempted from their constitutive relation to the social order of science.

A number of programmatic pieces outline alternatives to the Mertonian program, and include the disciplinary contents of natural science belief within the scope of sociological analysis. Works in the Edinburgh school (Bloor, 1976; Barnes, 1974; among many others) provided initiatives for many of the ethnographies mentioned above. However, the programmatic writings do not by themselves deliver "cases" of scientific action other than in the conventional fashion of reconstructing historical materials. This is important, insofar as the study of concurrent details of lab investigation opens up a hopelessly situated and embodied kind of work which is missed when science is addressed programmatically or through written residues. Ethnomethodological studies do more than fill out the empirical details of a critical theory of science.

3 Comprehensive discussions of the science studies literature are found in Mitroff and Kilmann (1977), and Merton and Gaston (1978). I do not include the many critiques of social science methods, studies in the "sociology of sociology," or specialized methodology manuals into the collection of science studies. The collection is defined by those studies which topicalize science as a phenomenon of theoretic interest. I do not include studies of social science, since it is a matter of some contention whether or how the social sciences stand as sciences. Accordingly, studies of social science inquiry are complicated by their involvement in issues of assessing whether social sciences are or should be sciences in the first place.

An exception to the above is Garfinkel's (1967) study of a particular sociological inquiry into a psychiatric clinic, where Garfinkel treated as a topic of particular interest how researchers achieved accountable "codings" of clinic file contents through the use of unformulated practices of inquiry. This study is exemplary in the way it sheds light on researchers' practices at the scene of the inquiry which were observable through an investigation of the course of the actual shop work. This approach distinguishes the study from principled critiques of social science methodology which present reasoned arguments on the character of social science practices in the absence of investigations of detailed cases of such practices. Also see Cicourel (1974), in this regard.

4 See Barnes and Dolby (1970); Mitroff (1974); and Mulkay (1976), for such critiques.

5 Cf., Crane (1972); Price and Beaver (1966); and Mullins (1968).

6 Speaking of the science studies literature as a uniform body of works glosses over the varieties of internal disputes and divisions which are produced in those studies. In the sociology of science, the Mertonian program is the "standard" which is confronted in alternative theoretic conceptions of science. For the sake of the introductory argument here, however, I treat both the "standard" textual treatments and their disputants within the science literature as a coherent literature. Regardless of whether I support one or another of these theoretical "positions" in the literature, I maintain that their manner of argument does not exemplify the empirical approach to shop work taken here.

7 I note that many of the authors of science studies texts are or were practising natural scientists. Accordingly, it might seem rather presumptuous of me to be taking issue with those texts on the grounds that they do not provide access to practical settings of scientific inquiry. However, I do not claim that these persons do not "know about" the science they expound upon. These persons are no doubt much more practised in the arts of doing science than I. What I am addressing, however, is not how much is "known" about doing science by its practitioners. Instead, I inquire about how a detailed observation and analysis of those practices in their course might recover features of their social and practical organization which may or may not be "known" in the fashion of practitioners' formulas and remembrances. I intend to pay particular attention to these "known" practices as topics of inquiry.

8 Garfinkel distinguishes between "studies of naturally organized ordinary activities," and "studies about ethnomethodology" in drawing attention to the exemplary character of the former collection of studies for further studies of the in situ identifying detail of ordinary activities. The latter collection of studies are identified by their use of "ethnomethodology" as a topic for reasoned discussion on issues such as, how ethnomethodology compares with prevailing approaches in sociology, what criteria can be specified to locate ethnomethodology's theoretic commitments and what academic or philosophic ancestries are attributable to ethnomethodology. These remarks rely upon a reading of Garfinkel: "Materials for two bibliographies: (I) Candidates for a corpus of studies of naturally organized ordinary activities, and (II) Studies about ethnomethodology," unpublished ms, circulated to a research seminar at UCLA, Winter, 1976.

9 To my understanding, ethnomethodologists do not claim to be...
“theory-free” in their studies, as if the world of their interest were disclosed in a manner purified of prior theoretic prejudice. The eschewal of theoretic commentary in many ethnomethodological writings asserts an alternative relation of the given inquiry to any initial theoretic commitments. Instead of presenting a theoretical line as if it governed the subsequent path of the given study of a substantive topic, the eschewal of theory allows for initial commitments to be modified in the hermeneutic experience of inquiry in an ongoing fashion. This is to say that ethnomethodologists provide for “general statements of theory” to be contingent statements rather than organizing principles in an inquiry, where such general statements are produced in the fashion of temporally situated conjectures which locate “resources” on the basis of the current exigencies of the inquiry.

If, for instance, the above paragraph is recognizable as a theoretic stance “following from” phenomenologists’ initiatives on the non-authoritative character of texts independent of the hermeneutical situation (see Palmer, 1969), I encounter an interesting situation. If, indeed, I were to follow such an initiative, and investigate the phenomenologists’ texts from within the topicalized and temporally relative course of inquiry, the texts would no longer stand as authoritative theoretic statements. Instead, they would be appropriated as contingent expressions within any current state of the inquiry. The phenomenologists would thereby be revived as “living ancestors” in any next reading of their texts, “ancestors” whose statements we influence through the ways we violate previous readings of their statements in order that those statements may live in the present. What the texts say can no longer be treated as authority or grounds for how they are made relevant for this ongoing inquiry; instead, the inquiry is equi-primordial as a “theory” of the text. Theoretical ancestry is thereby relevant, but not as stable grounds for development. Accordingly, I have no way to address these texts other than as a topic for a study of reading. Where reading involves the setting within an inquiry where the text becomes appropriate. Such a topic would constitute a distinct inquiry from the initiative taken up in a study of any “naturally organized ordinary activity.” Or I should say, such a topic would be one candidate topic from within the domain of naturally organized ordinary activities.

A collection of these studies will be available in, Garfinkel, (ed.), Studies of Work, a volume in Garfinkel (forthcoming).

This is to say that the corpus of studies is exemplary without dictating a given methodological schema for inquiries into any particular organization of activities. Cf. Henry Beck, (1972) for a discussion of the notion of an “occasioned corpus.”

See Bellman and Jules-Rosette (1977). The authors make specific analytic use of the visibility of scenes within the conditions of videotape. Cameras were given to native informants, whose use of the camera in filming scenes in their community life was made topical in the analysis of the resulting records. This study of “looking” with the videotape differs from a reliance on videotaping as an unexplicated “data collecting” technology, since the analytic use of the videotape camera was topicalized as part of the “cultural work” studied.

13 See Garfinkel and Burns (1979).
14 Here I include the many studies by Sacks, Schegloff, Jefferson, Pomerantz, and their students.
15 See Pollner (1970, 1974). Other studies involving the use of tape recorded materials are Wilkins (1983), Brannigan (1979), and Lynch (1982b).
16 See Weinstein (1975). Weinstein’s research makes a case for the accountable work of truck driving as an achievement of a reading of self-reported log books. This analysis does not appropriate “log books” as a description of the embodied details of driving a truck. Instead it raises the issue of how the log book is used within the institutionalized inquiries of trucking as the accountable presence of “driving” for the practical purposes of those inquiries. Weinstein’s research raises the further question of how the visibility of “driving” in a “reading” of the log book is to be analytically produced, since that visibility is not something that “leaps off the page” by virtue of the document’s being “looked at.” Instead, the page of the log book exhibits “trucking” within the varieties of situated hermeneutics which make up the actual inquiries within the “institution.”
17 Pack (1975).
18 See MacAndrew (1977), and MacAndrew and Edgerton (1969).
20 Livingston (1976a).
21 Morrison (1976).
22 The studies with which I am most familiar, and which include in the corpus of conversational studies are those by Sacks, Schegloff, Jefferson, Pomerantz, Goodwin, and Terasaki. The numerous studies by these persons which bear upon my research are referenced throughout later chapters.
23 See Atkinson and Drew (1976); and Heap (1978).
24 That conversational analytic studies are spoken of here as of “double-edged” applicability to specific work settings is not intended as a criticism of conversational analysis. The corpus of studies by Sacks et al. was not undertaken with an analytic interest in specialized settings of work other than in the instances of those activities which are accountable as conversations. It is the use of the analytic findings of these studies of conversation as resources for inquiries into work settings without regard to the “special” and circumstantial work involved in analyzing “conversation” by participants in those settings, that I raise as the critical issue.
25 H. Garfinkel, Lectures at Sociology Department, UCLA (1973–77).
26 Hooke (1961).
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27 See Dobell (1958).

28 In speaking of object-accounts as “reflexive achievements” I allude to the way in which, in a particular setting, an account does the work of “description” for the practical purposes of the parties in that setting. Artifact accounts are used as exhibits of that reflexivity since such accounts explicate otherwise “hidden” relationships between the lab inquiry and its objects of study. Artifact accounts are observable as clear cases of accounts which arise as constituent parts of a setting, insofar as they mark that relationship as a problematic matter. This use of the term, “reflexivity,” relies heavily on the writings and lectures of Harold Garfinkel. See Garfinkel (1967), pp. 7-9.

Part I

Ethnographic accounts of shop work
2 The lab setting

Introduction

In this volume my discussions and exhibits of laboratory shop talk and shop work involve the technical details of neuro-anatomical study in a particular research setting. I analyze sequences of action in a neuro-anatomical project, accounts of artifacts in technical work, and accounts of objects in laboratory shop talk. These taped recorded and field-noted materials will be presented in the body of the volume, and my discussions will continually refer to them. For reasons that are elaborated upon throughout the study I have chosen to examine these instances of conduct in addressing topics in the social organization of scientific work. Although the technical details of actual conduct in the laboratory setting are analyzable as social phenomena, their analysis entails some difficulties in presenting them as materials to a social science readership.

I have chosen not to gloss these technical details of in situ conduct in the laboratory under classificatory headings which are primarily meaningful within social science disciplines. I maintain instead that the discovery and display of the social organization of actions in the laboratory requires continual reference to substantive biological matters. To put this very plainly, it is my requirement that the analyst "know what's going on" in the setting prior-to and simultaneous-with the analysis of social or interactional actions in that setting. "Social" conduct and "scientific" conduct do not neatly discriminate themselves in the setting of their production.

In order for the reader to understand how this analysis proceeds from the conversational and ethnographic materials to an account of their interior production, some familiarity with the details of brain science will be necessary. As a way of supplying a rudimen-
tary education on these substantive concerns I will present a brief account in this chapter of the lab’s investigation of “axon sprouting.” This account will go into only those aspects of the lab’s investigations which bear upon the particular ethnographic and conversational materials which I shall present in the study. I will not attempt to give a comprehensive version of an entire field of study. Instead, I will present such details of the lab’s personnel make-up, facilities, research topics, and methodical approach to those topics as are pertinent to the particular instances of laboratory conduct which will be discussed in later chapters.

I present these matters as a series of rather plain “just-so” stories. This presentational format does not recapitulate the fashion in which the matters were apprehended in my visits to the lab, nor does it give access to how these background details were circumstantial to particular occasions of scientific talk and action. Instead, they are organized into this format sheerly for the sake of ready comprehension, and as an initial gloss which will allow the reader to proceed further into subsequent discussions. These descriptions of anatomical and physiological phenomena are by no means intended to be complete or accurate in terms of how a professional biologist might present them. My general ignorance of neuro-anatomy and physiology is compounded by my intention to supply only those details of the technical work and its phenomena of study as are sufficient for a reading of this volume. The study is not an introduction to brain science, in any traditional sense of an introductory text. I only hope that the reader will get a provisional appreciation of the inquiry in the laboratory studied here.¹

The laboratory

The setting investigated is a university-based psychobiology laboratory operated under the supervision of a professor in the biological sciences. This professor has acquired a considerable reputation for innovative studies of the animal hippocampus, and has published over one hundred articles in the brain-science literature, though he is quite young.

The lab’s membership varied considerably in makeup and number from time to time, ranging from ten to twenty full-time post-doctoral, graduate, and undergraduate research associates and assistants. In addition, the lab director collaborated with other professors in the department on various research projects. The lab was also frequented by numbers of undergraduate students taking independent studies as well as by friends of lab members who dropped in for visits. Persons worked in the lab during most times of the day, and all days of the week.

Researchers and research assistants worked with a variable degree of autonomy on their projects; sometimes alone, and other times in collaboration with other lab members. Numerous projects were simultaneously pursued in the lab, and they did not always “fit” together into an overall coherent topical design. For the most part, research in the lab concentrated on studies of in vivo and in vitro animal brain tissues with a topical focus on neurophysiology or brain plasticity. Laboratory rats were used in most projects, and the area of the brain which was studied was the hippocampus. Very few studies of a behavioral nature were pursued in the lab, as the research mostly involved experiments and observations of intercellular and intracellular processes.

The laboratory site was an environment for a variety of distinct technical specialities. Each speciality involved particular instruments and facilities, and these were distributed in an organized fashion throughout the various rooms of the lab. One set of facilities involved equipment for conducting electrophysiological studies of the brain, using oscillographic machines and equipment for the work of implanting ultrathin electrodes within cell layers and cell bodies in the brain. Another set of facilities involved equipment for cell and tissue culture, where extracted pieces of brain tissue were kept in a carefully controlled environment and monitored for cellular activity. Other facilities were devoted to light microscopic and electron microscopic analyses of brain sections.

Lab members tended to specialize in one or more of the available technical approaches utilized in the lab. Most members took a hand at specialities other than those entailed in their current projects, though for the most part they developed expertise at microscopic analysis, tissue culture, or electrophysiology. The arrangements of specialities were quite flexible over time, and over several months they shifted quite dramatically. Because the lab director had decided that the more promising discoveries in the lab’s subsequent researches would involve biochemical assays of brain constituents, the physical “plant” of the lab and the specialities of its members were considerably reorganized in order to incorporate biochemistry into the lab’s repertoire. New items of equipment were purchased and moved into the lab, such as a large scintillation counter, a mini-computer, and various smaller items of equipment used in the analysis of organic material into biochemically meaningful constituents. Concurrent with this, certain specialities of anatomical study were de-emphasized, were given less space, and their practitioners were re-educated in
biochemistry. The entire set-up was quite flexible in incorporating new projects into the research repertoire.

**Research topics**

Two major lines of investigation were pursued in the lab during the time of my study. One of these involved the analysis of neurotransmitters in various brain systems, and the other involved the investigation of "brain plasticity." Brain plasticity referred to the regenerative capacity of the brain, the functional "ability" of the various brain tissues to "compensate" for damage done to specific brain areas. In some circumstances the brain has been demonstrated massively to re-adapt the functional organization of its tissues to compensate for the destruction effected by a lesion. In addition to these two main project areas, numerous other topics of study were addressed.

The analysis of neurotransmitters required the use of hollow glass electrodes of very small diameter, which were filled with a liquid electrode and implanted into specified brain regions. Other electrodes simultaneously monitored the firing rates and amplitudes of chains of neurons in the vicinity of the glass electrode. Experiments were devised in this context which involved a series of injections of ions from the hollow electrode. These electrodes were constructed with multiple "barrels" which allowed the serial injection of different chemicals into the tissue. Subsequent to each injection, the electro-physiological monitor was observed for any marked fluctuations in firing pattern. When "jumps" in the pattern were consistently associated with a given chemical introduction, this was taken as evidence that that substance acted as a medium of synaptic transfer in the particular neural system.

These neural chains were "traced" through an ingenious method which entailed the injection (again through hollow electrodes) of a particular stain into living brain tissue. This stain was said to be "taken up" by neurons and transported over the length of the neurons' axons to their terminal boutons and then across synapses to dendrites from other cells associated with the original neurons. Subsequent to this "transport" of the injected stain the animal was killed, and the brain dissected and microscopically examined. The stain was consulted as a record of its transport and orientation of the axons, dendrites, and cell bodies in the hippocampus was rendered into a two-dimensional graphic display through microscopic preparatory work. A planar section of the hippocampal tissue could be sliced and arranged so that the orientation of a layer of cell bodies could be displayed as "horizontal," the dendrites arising from those cells as "vertical" and the various "parallel" lamina of axons as measurable zones along the axis of the dendrites.

When arranged in this way the hippocampus was used as a research locale not only in terms of any uniquely interesting anatomical and physiological features of the hippocampus itself, but also in terms of the way its "configuration" of neural constituents facilitated studies of generally significant brain phenomena which could not be graphically accessed in studies of other brain regions.

The hippocampus, a relatively small (in adult rats, each hemisphere of the hippocampus is approximately 2mm. × 1mm. × 1mm.) and anatomically distinct region of the "old" or primitive brain, is located beneath the cerebral hemispheres and towards the mid-brain. The hippocampus is said to be involved in processes of learning and attention, though its functions as yet remain largely mysterious. The hippocampus is anatomically described as a distinctly layered brain structure, with its neurons being arranged into relatively parallel laminations of cell bodies. The axons which flow into the hippocampus from cells in other brain regions and synapse with the dendrites of hippocampal cells are also organized into layers. These axon strata roughly parallel the orientation of the hippocampal cell bodies, and are analytically separated into layers according to their distinct sources in other brain regions (see Figure 2.1). The resultant anatomical configuration is thus analyzed into identifiable layers of cell body types, with their "vertically" coursing "dendritic trees" being intersected by several relatively homogenous layers of axons.

This "laminar" configuration of the hippocampus is contrasted with the anatomy of other brain structures such as the cerebrum, where analytically distinct cell layers are found to receive inputs from varieties of other cells in a seemingly disorganized array across the "vertical" alignment of their dendrites.

In the lab discussed here, this "laminar" configuration was appropriated as a sort of naturally occurring graphic field, suitable for demonstrating comparative events and structures which were less easily available elsewhere in the brain. The observed alignment of the axons, dendrites, and cell bodies in the hippocampus was rendered into a two-dimensional graphic display through microscopic preparatory work. A planar section of the hippocampal tissue could be sliced and arranged so that the orientation of a layer of cell bodies could be displayed as "horizontal," the dendrites arising from those cells as "vertical" and the various "parallel" lamina of axons as measurable zones along the axis of the dendrites.

When arranged in this way the hippocampus was used as a research locale not only in terms of any uniquely interesting anatomical and physiological features of the hippocampus itself, but also in terms of the way its "configuration" of neural constituents facilitated studies of generally significant brain phenomena which could not be graphically accessed in studies of other brain regions (the issue of the general significance of anatomical findings rested on assumptions about the ultrastructural typicality of cell anatomy and physiology regardless of particular anatomical region).
Once the hippocampus was made accessible as a graphic field, it was used in studies in which experimentally induced changes in one isolated axon layer set up the observability of consequent “effects” in the adjacent lamina of axons. A degree of experimental control was made available in this quasi-geometric rendering of the hippocampus which was not facilitated in other brain regions. Rearrangements in the relative organization of adjacent axon lamina in this graphic field were used in demonstrating brain plasticity.

It is accepted in brain science that in adult mammalian brains new neurons are not produced under normal circumstances or in the event of an injury. Once a brain cell dies in an adult brain it is not replaced by a new cell. However, the brain has been shown at times to recover former “functions” which for a time were destroyed by massive damage to brain tissues. This recovery is attributed to reorganizations of existing brain tissues to fulfill compensatory functions. The mechanics of that process are part of what make up the topic of brain plasticity.

A range of studies was pursued in the lab on various aspects of brain plasticity, and was worked into a “picture” of the brain as an organic environment of flexibly organised and mobile constituents. This “can of worms theory of the brain” (as the lab director called it) was contrasted to “hard wired” approaches which model the brain in terms of the fixed electrical constituents of a computer. The emphasis on brain plasticity in the lab was demonstrated by studies of the brain’s autonomous capability for “rewiring” during crisis situations. As part of this focus on the brain’s flexible and organic capabilities, an investigation of a non-neuronal cell type called “glial cells” was developed. These cells, of which there were three types studied (astrocytes (also known as astroglia), oligodendricytes, and the controversial microglia), were described as mobile cells with an as yet unknown relation to the more positionally fixed neurons. Glial cells were not considered to be involved in transmitting nervous impulses, though they were said to “attend” the neurons in various ways.

Astrocytes were said to be involved in the “phagocytosis,” the engulfment and transport of degenerating material following an injury. Astrocytes were electron microscopically displayed as amoeboïd cells which flowed between the “forest” of neural axons and dendrites in the brain by projecting pseudopod like extensions of protoplasm. These cells could be seen in electron micrographs as “flowing around” particles of degenerating material and absorbing these into their protoplasm.

Oligodendricytes were not thematic to the lab’s inquiry, though they were said to be involved in the “manufacture” of myelin, the substance which “insulates” the larger axons in the brain. Oligodendricytes had been electron microscopically depicted in association with concentric sheets of myelin, presumably about to be deposited on the surface of axons.

Microglia were the least understood and most controversial of the glia. Some electron microscopists refused to credit microglia as a separate cell type from oligodendricytes, and there were further controversies over where they came from. In the particular lab’s studies these were not only demonstrably present, but were also assigned a key part (as yet unspecified) in the triggering of a regenerative phenomenon called axon sprouting.

The lab’s studies focused on the glia as elements in a hypothetical sequence of events which were associated with the...
and perhaps other organizational phenomena. Such an unprec­

desire diffusable compounds which trigger regenerative processes

had always been assigned subsidiary functions in comparison with

recovery of brain tissue from the effects of a lesion. Whereas glia

the neurons, it was believed by lab members that the glia may

observed in some detail during my visits to the laboratory.

ported finding of a diffusable compound in the brain had yet to be

"sprouting" of new branches from its axon fibers and the extension

synapses were often in anatomically distinct brain regions than

were said to be capable of forming new branches which "grew"

when a territory of synapses was vacated through the destruction

would functionally adapt to the destruction of an originally distinct

into the vacated region to replace the destroyed synaptic connec­

studies it was observed that when one axon bundle was cut in a

labrador animal, other axons, arising from distinct cellular

partially reoccupied its synaptic territory.

continued controversy in the brain science literature.

how that expansion occurred. Existing demonstrations did not

irrefutably show whether the purported "sprouting" of axons was

an actual extension of new axon fibers or whether it was a more

brain system. In short, the phenomenon was a subject of some

division of events in the sprouting process, it was believed that

phenomenon of "axon sprouting." This phenomenon was charac­

of the axon-inputs into that territory. Axons in adjacent regions

of those branches to form synapses with dendrites from other

sources, "grew" along the "path" of the degenerating fibers and

original cells, a recovery of the original brain functions was

A key topic in the lab's studies of brain plasticity was that of the

Axon sprouting

neurons. The process was thought to occur in crisis situations

of the neural system. The account specified that a "mature" neuron had

A number of projects which were extant in the brain science literature,

terminal boutons (terminal boutons are characterized as the end

axon sprouting which were extant in the brain science literature,

neural systems. There remained some controversy, however, over

phenomenon and addressed matters that presupposed a particular

exceptions to the current controversies about the

aimed at understanding the chemical nature of the "trigger," though the possibility was entertained as one with

isolate the particular chemical factor had yet to be successful.

anatomical demonstration of axon sprouting, and others of which

employed in these studies, such as light and electron microscopy,

microglia were possibly involved in releasing such a compound.

is an electron-microscopic study of

configuration of events in axon sprouting. This study sought to

was an electron-microscopic inquiry into the ultrastructural

level," as well as to discover further sequential features of the

A number of projects which were already

took place during my visits to the laboratory.

studies which utilized the graphic rendering of the hippocampus

sought further breakthroughs in the sequential and histo-chemical

In the lab's investigations of axon sprouting, the written contro­

In the first year of my studies in the lab, many projects were already

such as light and electron microscopy,

processing of the phenomenon. A variety of techniques were

anatomical demonstration of axon sprouting, and others of which

anatomical demonstration of axon sprouting, and others of which

ultrastructural project was an electron-microscopic study of

further demonstrate the phenomenon on an "ultrastructural

 occurred. These addressed a

anatomical demonstration of axon sprouting, and others of which

anatomical demonstration of axon sprouting, and others of which

ultrastructural project was an electron-microscopic study of

ultrastructural project was an electron-microscopic study of
swellings of an axon branch which form a synapse in their proximity to dendritic membranes) forming a greater number of synaptic contacts with dendrites in their vicinity. This was seen to account for functional aspects of plasticity in the absence of any growth of existing axon branches. Another account claimed that a general shrinkage in the neuropil (the tissue “environment” exterior to neuron cell bodies) after the introduction of a brain lesion confounded the demonstration of an expansion of an unaffected layer of axons relative to the shrinking field of degenerating material. The ultrastructure project was regarded as a way of utilizing electron microscopic technology to demonstrate photographically that existing terminal boutons formed an average number of contacts which did not vary after the introduction of a lesion. In addition, it was hoped that “growth cones” of “sprouting” axon branches would be photographed, thereby documenting the occurrence of actual growth of existing axons into new territories.

Although the project addressed the counterclaims which were extant in the literature, the inquiry was not exclusively concerned with those competing accounts. Much of the work of the project involved the analysis of electron micrographs as a way of deepening the lab’s particular account of the event. This analytic work addressed issues that were meaningful in light of the lab’s specific account of axon sprouting in a way which was independent from the issues that were current in the literature on the subject. The greater degree of magnification afforded by electron microscopic technology provided a form of observability that had not been exploited in prior studies by the lab. The possibility was entertained that the ultrastructure project would provide for the visibility of such phenomena as “growth cones” (the tip of an axon in the process of extending itself in the brain tissue), the engulfment of degeneration particles by astroglia, and the mechanics of how new axon terminals form synapses while replacing degenerating terminals. Less defined possibilities concerned the unknown “role” of microglia in the sprouting process, and issues of whether the “triggering” of axon sprouting was related either to a series of membrane contacts in the intercellular environment or to the effects of a diffusible chemical. These hopes, which were expressed by practitioners at various times in their conversations with one another and in their accounts given for my benefit, were not all given strong documentation during the project, though they animated much of the work of designing the project and analyzing its data.

In addition to the above reasons which were given for undertaking the project, rationales for the project became available in the course of the work of examining the micrographic results as they came in. Occasionally these situated discoveries became explicit topics of further investigations.

The design of the ultrastructure project involved the display of comparisons of measurable differences between adjacent layers of axonal inputs into the hippocampus in the dentate gyrus region. These comparisons were arranged to provide an account of an expansion of one axon layer into the region formerly occupied by the other axon layer after that latter region was destroyed by an experimentally induced brain lesion.

In the schematic representation of the dentate gyrus in cross-section (Figure 2.2), three distinct layers of axons are shown as they course in a perpendicular direction to the “vertically” represented granule cell dendrites. The enlargement of a single granule cell shows these three layers as they form synapses on different “levels” of the dendritic tree. The axon lamina are indexed to the anatomical region where their cell bodies are located; these being the entorhinal cortex, the commissural (opposite hemisphere) and associational (same hemisphere) hippocampus, and the septum. The entorhinal and the commissural-associational layers were featured in this project. Briefly stated, the demonstration of axon sprouting consisted of a display that the commissural-associational layer of axons expanded after a lesion of the entorhinal cortex destroyed the axons arising from that region. Measurements of this expansion showed a consistent reoccupancy of the lower 25 per cent of the region of the granule cell dendrites formerly occupied by the entorhinal layer of axons. In prior studies the bulk of this expansion was found to take place within ten or eleven days post the lesion.

The ultrastructure project was an attempt to make such a demonstration with the use of electron micrographs of a standardized locale along the vertical plane of the dendritic tree (when represented two-dimensionally in the diagram). Measurements of the relative dimensions of the entorhinal and commissural-associational layers were taken at different “time points” post the lesion as a way of displaying any expansions of the latter layer over time.

Renderings

The graphic demonstration of the ultrastructure of axon sprouting required more than a mere “observation” of a living animal during its recovery from the effects of a lesion. The requirement of microscopical observation necessitated the rendering of the animal to comply with the conditions of instrumental observation. Obser-
The technical impossibility of observing a continuous organic process on an ultrastructural level of analysis was circumvented in the project through the construction of a display of the process in a temporally ordered series of displays of brain materials from several cohorts of dead animals. That is, ways were made available for displaying the ultrastructural anatomy of any living brain by "sacrificing" the animal and thereby suspending its living processes to provide a display much like a still-photographic account of a scene of continuous action. As a way of showing this temporal order for any simple living brain's regenerative process, a procedure was used in which a series of electron micrographic montages was constructed at different "day" intervals after an entorhinal lesion. The series of documents of the instrumentally rendered brains was constructed from the remains of different animals, where the series was used as a display on behalf of an analytic process held to be common to any of the animals.

The work of the project involved a methodic series of renderings of laboratory rats that "prepared" the animals for electron microscopic visibility. Cohorts of adult animals (Sprague-Dawley rats - a standard breed of white rat of relatively uniform brain weight and dimension) were subjected to a lesion of the entorhinal cortex, and then "sacrificed" after an interval of days or half-days between two and eleven days post the lesion. The "day" interval varied with each cohort, as they were used to document a "point" in the course of axon sprouting. The brains of the animals were removed and preserved in a formaldehyde-like solution which acted to inhibit the breakdown of membranal proteins following the death of the animal. The hippocampus was then removed from the preserved brains and small pieces were cut in cross section. These "thick sections" were placed in shallow dishes of a liquid plastic which was then baked to harden. The plastic discs were then cut with a fine-toothed saw to expose a selected portion of the embedded hippocampus sections. The exposed surface of the hippocampus tissue was mounted on a microtome and ultra-thin sections were cut. These sections were of such tiny dimensions that they were viewed under a dissecting microscope while being picked-up on the strand of a hair on a thin paintbrush-like tool and transported to a small copper grid about the size of the letter "o" on this page. The grid was the electron microscopic equivalent of a microscopic slide. The thin sections were then immersed in a series of liquid compounds for staining, rinsing excess stain, counter-staining, and further rinsing. When the sections were

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**Figure 2.2** Schematic of a single granule cell in the molecular layer of the dentate gyrus of the dorsal hippocampus (enlargement of cell located at (g) in Figure 2.1)

**Key**

- **E** = layer of axons from entorhinal cortex
- **C/A** = layer of axons from commissural/associational hippocampus
- **S** = layer of axons from septum
- **d** = dendrites from granule cell
- **a** = axon of granule cell
- **g** = granule cell body
viewed under the electron microscope, the microscopist attempted to take an overlapping series of photographs in a vertical alignment perpendicular to a layer of granule cell bodies in the dentate gyrus region of the hippocampus.

After the development of the photographic series from a given slide, the photographs were enlarged and mounted on a large cardboard sheet and placed in a "montage" arrangement of overlapping photographs showing a relatively continuous strip of "landscape" across their adjacent borders. These montages were indexed to the animal, to the section from which the photographs were taken, and to the number of "days" that lapsed between the lesion operation and the sacrifice of the animal. These photographs provided the visible configuration of brain ultrastructure that was addressed in the analytical phases of the study.

As each montage was constructed, it was analytically addressed in the following manner: a clear plastic sheet was laid over the surface of the photographs, and a linear scale was drawn over the surface of the sheet running in a vertical direction which paralleled the edge of the columnar montage of photographs (see Figures 2.3 and 2.4). A scale of "microns" (computed with reference to the magnification power of the photographs) was plotted for the drawn-line, where the "zero" point was set at a horizontal line that approximated the alignment of the granule cell body layer (see Figure 2.5). Measurement along this scale was used to estimate linear distance along the "vertical" alignment of granule cell dendrites as they arose from the cell bodies and coursed upward. (The availability of such formulations as "upward," "vertical," "linearity" can be taken as constructive achievement of the rendering practices that displayed "brain tissue" as a relatively two dimensional array of magnified and locationally framed phenomena. For the time being we are exploiting these members' "place formulations" while providing a naturalistic sense of the lab's topical work. Later we shall find ways to treat these formulations as interesting features of shop work and shop talk, after this ethnographic setting is established.)

The plastic sheet was used as an analytic display of topical entities and their relative arrangements over the surface of the montage photographs. Once the sheet was fixed in place over the surface of the montage, the research assistants in the project went about the work of marking instances of "intact" terminal boutons, "degenerating" boutons, astroglia and microglia (see Figure 2.6 and 2.7). Different colored marking pens were used to index the different entities as to type. Red was used for "intacts," blue or black for "degeneration," yellow for astrocytes, and green for microglia. Marks were made primarily by tracing the outer membranal border of an identified structural entity and forming a colored outline that highlighted the visibility of the analytic entity. Not all located entities were circled, some were marked with a dotted line or with an X placed over the phenomenon. These markings noted ambiguities about the phenomenon which were mainly of two sorts. The first ambiguity marked a difficulty in assessing whether a located terminal could be classified as degenerating or intact. Degenerating terminal boutons were "labelled" with a stain to show up as relatively dark in comparison to intact terminals. They showed a similar range of identifying features to the intacts (small vesicles, shape and size, proximity to "post synaptic densities" which were prominent features of synaptic junctures), though they often were relatively blurred through the presumable breakdown of membranal integrity which produced a degraded visibility. Occasionally an identifiable terminal would be discovered which was not markedly darker than intact structures, and which showed but a small degree of degradation of form. (In other words, some terminals were not clearly assessed one way or the other.) These were marked with a dotted outline in the color of their "guessed" entitative type.

A second sort of ambiguity was noted when the research assistant located an entity that "looked like" a terminal, but which was not visibly associated with a "post-synaptic density." Post-synaptic densities were located as very prominently marked membranes which stained very darkly. Their presence was used as a "criterion" for assessing whether a terminal-candidate was visibly associated with a synapse or with a dendrite. However, there was no assurance that in every case the portion of a terminal that appeared in the cross-sectional rendering would display its proximity to a synapse. In those cases of an identified terminal-candidate where the criterional post-synaptic density was not visible and may have been "out of the plane of the photograph" an X was marked.

The marked-montages were further analyzed through counts of the relative density of degenerate versus intact terminal markings (only the "clearly" identified instances were counted) in five micron square sectors of the photographic columns. The grid formed with the construction of the linear scale on the plastic sheet was used to mark off these sectors. This totaling activity was done for all of the acceptable montages produced in the project. Each total was indexed to the location of its sector on the vertical co-ordinate proceeding from the cell body upwards along the dendritic texture (e.g. 70-75 microns from the cell body, 75-80, etc.). The range of the scale that was used in producing
**Key**

- **A** = "early" system of notation for identifying montage:
  - 5 = number of "days" post entorhinal lesion
  - 1 = identification of "animal"

- **B** = scale of "micron equivalents" drawn along margin of column.
  Numbers on the scale represent "vertical" distance from the granule cell bodies

- **C** = high-power electron micrographic montage (24,000 x)

- **D** = low-power electron micrographic montage (2,160 x)

- **E** = hand drawn rectangle marking placement of "high-power" montage in the field of "low-power" montage (see close-up in Figure 2.5)

- **d.a.** = "marked" degenerating myelinated axon (note parallel slash marks which were drawn to "mark" the axon on the clear plastic overlay)

- **d.g.** = "marked" degenerating axon terminal. Black pen was used to trace outlines of degenerating terminals on the plastic overlay. Circle around the terminal marks it as a "countable" instance

- **i** = "marked" intact axon terminal. Red pen was used to trace "intact" terminals. Circle around the terminal marks it as a "countable" instance

- **x** = "marked" degenerating axon, with "X" mark notating that the terminal is not a "countable" instance. Often the "X" was used to mark a case in which the "terminal" candidate was not visibly associated with a "post synaptic density" (see Figure 2.7). In other cases, an "X" marked that the analyst was "uncertain" as to whether the instance was an "intact" or "degenerating" terminal

- **c** = capillary

- **d** = dendrite (presumably arising from granule cell layer)

- **g** = granule cell bodies (nuclei outlined with marking pen)

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**Figure 2.3 "Early" version of micrographic montage**

This montage format was used early in the project. A "low-power" montage was placed alongside the "high-power" montage and was used as a locational reference for the area of the molecular layer "covered" by the strip of highly magnified micrographs. Analysis of axon terminal density and distribution was done on the high-powered montage through the "marking" of intact and degenerating terminals with different colored outlines. These instances were counted for each five micron sector of the montage and the relative densities of the two terminal types was used as an indication of axon sprouting.
Figure 2.4 "Recent" version of micrographic montage
In this format several montages were photographed from a "low-power" areal plane (not shown on the same cardboard backing, unlike in Figure 2.3). Two of these montages from a "2-day" animal were placed on the same 3' x 4' cardboard sheet and a clear plastic overlay was clipped over the montages. Markings were noted as in Figure 2.3. Two instances of "microglia" cells are shown here. This cell type was identified by members of the project and marked with green outlines, despite controversies in the literature over whether "microglia" were a distinct glial cell type. Microglia were associated with the earlier phases of axon sprouting, and their appearance in the "2-day" montage seems to indicate that the microglia have "migrated" into the degenerating region. Degeneration sets in at around the 70-75 micron sector. Although it is difficult to detect on black and white photographs, the "2-day" montage in this figure shows a relatively "clean" breaking point between degenerating and intact regions, while the "5-day" montage in Figure 2.3 shows more overlap between the two regions. Axon sprouting was said to begin at roughly "5 days" and the overlapping region of terminals at that "day" point was taken as an indication of its onset.
**ETHNOGRAPHIC ACCOUNTS OF SHOP WORK**

**THE LAB SETTING**

**Key**

- **A** = "lower" edge of rectangle showing placement of high-power montage in the field of the low-power montage. The scale of micron equivalents begins at about 60 microns above the granule cell bodies.

- **B** = "upper" edge of rectangle, approximately 110 microns above granule cell layer. Note: microns were computed in reference to the magnificational power of the photographs and a metric scale was constructed to mark "micron equivalents" for each montage.

- **a** = astrocyte cell nucleus marked with a blue line drawn around border

- **m** = two "marked" microglia cell nuclei

- **g** = "marked" granule cell nucleus in granule cell layer (nucleolus is dark spot in center of nucleus)

- **c** = capillary cross-section

- **f** = artifactual line presumably created through the folding of the thin tissue section

- **z** = line drawn to interpolate "upper edge" of the granule cell layer for purposes of measuring distances from the cell layer. The line acts as the "zero" point on the vertical scale of microns.

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**Figure 2.5 Enlargement of low-power montage from Figure 2.3 showing placement of high-power montage in the field “above” the granule cell bodies.**

When shooting a column of photographs for the high-power montage, the electron microscopist attempted to produce a "line" of photographs at an orientation roughly perpendicular to the alignment of the "horizontal" strata of the dentate gyrus. In addition, the microscopist attempted to intersect the transition zone between the intact commissural/associational axons and the degenerating entorhinal axons. Figure 2.5 exhibits the subsequent work of locating the high-power montage within the low-power field, and the mapping of a linear axis onto the planar field of the low-power montage to construct a scale of measurements to be used in setting the high-power montage into equivalent areal sectors. The analytic marking of instances of intact and degenerating terminals provided a basis for computing ratios of intact/degenerating terminals for each five micron square sector. When these ratios were plotted in terms of a series of "days" post the lesion, they were consulted to show the sequential changes in terminal density and the distribution which were used to document axon sprouting.
Figure 2.6 Enlargement of photographs from “5-day” montage (Figure 2.3)

This photograph is an enlargement of a micrograph in the “5-day” montage in Figure 2.3. The photograph was taken of an area in the upper right-hand column of the high-powered montage at around the 100–105 micron mark on the scale. The plastic sheet upon which “markings” were drawn was removed for the sake of clear photography in Figures 2.6 and 2.7. Figure 2.6 shows the inferable results of sprouting in the degenerating region of the dentate gyrus molecular layer. Degenerating material is visible throughout the field of the photograph along with extensions of astrocyte protoplasm and intact axon terminals. Note that the degenerating axon in the upper right of the photograph has a “withered” appearance and the concentric rings of its thick myelin sheath appear to be “unravelling” as a visible manifestation of its degenerating condition. The degenerating terminals in the photograph appear relatively blurred and diffuse in their interior organization when compared with the intact axon terminals. Synaptic vesicles (small circular features within the borders of the terminals) appear more distinct in the case of the intact terminals. In the bottom right of the photograph, degenerating material (d.g.) appears to be in the process of being “engulfed” by the extension of astrocytic protoplasm. Adjacent to the degeneration is an intact terminal (i) which appears to be occupying the former site of the degenerating terminal. Other instances in the photograph where intact terminals are visible alongside degenerating terminals were said to be indicating that the intact terminals had “grown into” the degenerating area during the process of axon sprouting. This, of course, was an inferential reading of the photograph which interpreted the “fixed” anatomical rendering to be a document of a “time point” within a more extended and dynamic process.
Figure 2.7 Enlargement of a micrograph showing "intact" neural synapse
This photograph is enlarged to approximately 40,000 X, and clearly shows visible features which were invoked as "criteria" for marking instances of intact axon terminals. "Countable" instances (those which were included in the tallies of terminal distribution and density for statistically documenting the occurrence of axon sprouting) were visible axon terminals which were associated with a "post-synaptic density." This latter anatomical structure was distinguished by the darkly stained (electron dense) membranal "thickening" adjacent to a synaptic gap. Note the distinct separation between the membranes of the axon terminal (i) and the synaptic spine (s). The spine is the crescent-shaped organelle which projects from a dendrite and "receives" the neural impulse. The circular synaptic vesicles which are distinctive of the axon terminal are said to store neurotransmitters. The release of these neurotransmitters affects the receptivity of the dendritic membrane and facilitates the transmission of impulse from the axon to the dendrite. Note that just to the left of the terminal labelled (i) is another, slightly larger, instance of a terminal. This terminal is not shown to be adjacent to a post-synaptic density within the plane of the photograph, and was not "counted" as a terminal, despite the fact that its distinct vesicular appearance allows its ready identification as a terminal. For further discussion of the identifying features of the ultrastructural environment, see Peters et al., (1970).
these totals was from 60 to 130 microns from the estimated cell layer base.

A relatively distinct "transition" zone was visible in the array of red and black markings of terminals on each montage face. The counts quantitatively documented this transition and provided a measurement of where along the distance from the granule cell bodies it occurred. The character of this transition space was seen to vary depending on how many days had elapsed between the lesion and the sacrifice of the animal (from two to eleven days in this study). In "2 to 4 day" animals the transition was relatively clean between the red and black markings, with a break occurring between 70 and 75 microns from the cell layer. This was taken as a document of the distinct layering of the axon strata under normal circumstances, where the degeneration in the entorhinal area clearly distinguished terminals in that zone from the "intact" commissural-associational terminals. Had no lesion been performed, terminals in the two layers would be relatively indistinguishable on an electron microscopic level (unless some other way of selectively distinguishing the one layer from the other were employed as through a specialized stain).

This transition zone where both intact (red) and degenerating (black) markings overlapped was seen to expand after five or six days post the lesion. That is, intact terminals were found in greater frequency in the display at points above the 75 micron mark on the grid. These intact terminals appeared in the midst of the degenerating remnants of the entorhinal afferents, as those phenomena were displayed in the relativity of the markings.

The markings also formulated noticed instances of astroglial and microglial protoplasm, as they appeared within the photograph frames. These phenomena were noted with less care than were the terminals, and were less centrally featured in the written account of the project that eventually was produced. On some montages rather obvious instances of astroglial and microglial protoplasm remained unmarked, though a rough assessment of any marked changes in the frequency, size, and spatial distribution of the glia was made. In the final draft of the research report a few unquantified "noticings" on the increased size of astroglia during the sprouting period, and on differences in the observable protoplasmic make-up of the astrocytes (increase in glycogen granules, numerous filaments) were stated. The topic of glial involvements was much more strongly featured in a variety of other experiments in the lab's research.

Conclusion

The foregoing account of the lab setting and of the ultrastructure project was provided to me during my study through "tours" and instructions given by lab members while introducing me to their work. It is presented here in the format of a report, where an extensive course of work is provided with its "reasonable" basis for the sake of cogent discussion. As such a report about the work of the lab it presents that work as an unproblematic working out of a conceptual problem; e.g. "what are the components of the process of axon sprouting?" A sequence of activities is then presented as a "reasonable" way in which the problem was addressed (cohorts of animals were lesioned, then they were sacrificed, etc.).

This account of the lab setting was provided in this naively reliant way (reliant upon the form in which it was handed to the author) as a kind of testimony to its irresistible character as "first instructions." Such an instructional account makes the work of the lab available as an accountable matter isolated from any more specific instructional setting such as would be given in the course of accomplishing electrophysiological "soundings" of brain tissue. In this latter circumstance the instructions are inseparably part of the detailed actions with the equipment, the anesthetized animal, the flux of oscillographic readings, and the access that members allow each other to the sensibility of the scene. The version of the lab's work in report or "tour" form is a phenomenon in its own right which will be treated as such later in this study. At present, however, the report format is used as a way of bringing the reader into the lab much in the way that it was used in the lab for visitors. An alternative was not taken, which was to present detailed analyses of particular circumstantial occurrences in lab shop work and shop talk, and to use these as a way of bringing the reader into the situated detail of issues in lab research without the use of any prior glossing of the work as a coherent story. I believe that such an approach is possible, and indeed is predominantly a way in which members of a lab find their way through the day, but that it can be confusing to the reader to encounter the work in that fashion without some general preparation. In a sense, it is a matter of courtesy that the above report is given here as a reiteration of the courtesies that were extended to me during my earlier visits to the lab.
3 Projects and the temporalization of lab inquiry

When I speak of scientific shop work, I am referring to an order of practices that exist within a temporal context. That temporal organization was found in this study to be characterized by the temporalization of shop practices into extended projects of inquiry. By the temporalization of practices I mean the production of extended courses of inquiry in lab work through the serial ordering of tasks in the immediacy of an organizational setting.¹

In this chapter I will focus specifically on the phenomenon of “project,” and will consider what a project consists of as a productive analytic² for the work of laboratory science. The analytic notion of a “task” in a project will also be discussed in terms of how a “task” is credited with being a constituent unit of work in a project. This discussion suggests some variously detailed considerations for an account of the temporal character of technical work, especially as it concerns the organization of laboratory research into projects of study.

A “project” will be treated here as a sequential unit in the work of laboratory members which culminates in the writing of a research manuscript, usually for publication under the joint authorship of the various participants in the project. I am adopting “project” as a sequential unit of interest in this discussion on the basis of the observation that members in the lab utilized a practical analytic for producing their work in such units. Projects, as they are formulated here, appeared as oriented-to units for assigning work to members, ordering supplies, preparing equipment, proposing phenomena for investigation, and consulting prior studies for their exemplary value. I cannot, as yet, speculate as to how generally this organization holds in laboratory science, or whether the production of lab work is invariably oriented to the work’s occurrence as a phase of a project. However, within the
locale investigated, project appeared as a prevalent sequential format in the production of lab inquiries, and appeared as an essential feature of the sensibility of lab shop work as a witnessable phenomenon.3

As units of study, projects appeared “bounded” in the work of members by definite beginning and concluding phases. Beginnings were achieved and became visible in the work of “planning” a course of investigation, when technical ways of addressing and displaying those phenomena were “designed.” Conclusion for a project’s sequence of actions took the form of “writing up” results into the format of a research publication.

This analytic provision for the occurrence of projects is distinct from a sense of scientific projects as indefinitely extensive courses of work defined by reference to their respective topical phenomena. Such a relation of work to a topic of study (such as axon sprouting) is an observable feature of extensive courses of lab research, but is a separable matter from the practical organization of specific courses of research into projects. In the case discussed here, projects were topically linked to one another sequentially and simultaneously in varieties of ways that will be briefly discussed later in this chapter. Years of inquiry were devoted to the phenomenon of axon sprouting, incorporating a wide variety of approaches such as electrophysiology, biochemical assay, cell culture, the use of radioactive trace elements, and light and electron microscopy. In some fashion, the succession of such studies showed a “development” where one could retrospectively trace “linkages” between earlier and later studies. How such “linkages” were achieved at the time of their occurrence and for the parties involved in their achievement is an issue which was beyond the scope of this study.

As an analytic unit in the temporalization of the lab work, a project was observed to be a contingent phenomenon which was not assured of completion or continuous production by the mere fact of its having been started. The continuity of a project did not develop out of its initial planning in a way that was insensitive to circumstantial considerations arising over its course. Instead, a project was observed to be a unit which was treated by members as interruptable, or even abandonable as “failed” on occasions along its course. A project’s completion, marked by the written announcement of its findings, was viewed as a “preferred” outcome of members’ technical work, which outcome was contingent upon the adequate performance of that work in its practical circumstances.5

Projects have been characterized here as temporal phenomena in the assembly of laboratory work. The temporal character of the sequentially arranged “tasks” in a project was observed to involve much more than a mere succession of members’ work-related activities in “clock-time,” and was not always visible as the successive acts of any particular lab member. The relatedness of actions in a project’s sequence of actions, although it displayed characteristic forms of succession, was not available in the continuous monitoring of the spectacle of events which were witnessed in the lab.

Early in my observations I found lab work to be problematically available in the immediate visibility of persons performing tasks in the lab vicinity.7 At any given time when a number of persons were active in the laboratory, numerous not obviously related actions could be seen being performed simultaneously by different persons, or in the successive actions of a single person. The “what” that any given task consisted of as a course of action was not available within the spectacle in terms of its presence in an extended course of inquiry. Instead, one could see, for example, a collection of light microscopic slides being stained by being transferred through a series of brightly colored liquids.8 The detailed character of such a process made reference to an extended course of inquiry in the way that the preparation was a rendering of particular microscopic visibilities to accord with the design of an inquiry. For instance, the particular selection of stains in this staining example depended upon which microscopic phenomena were being highlighted by the bonding of the stain molecules. The phenomenon (in one case, myelinated axons, requiring a particular kind of heavy metal stain to be made differently visible), was topical to a particular project of inquiry. To see the details of the staining operation (just those stains on just those slides in an identifiable sequence), that is, to achieve a sensibility of the scene as something other than a spectacle, required the particulars of the operations to be sensible in terms of their “place” within a project.

A tour of the lab environment revealed, at any given time, a wide variety of tasks performed by different practitioners in conjunction with different instrumental apparatuses. Persons could be seen doing “tissue culture,” monitoring live brain cells’ activities, experimenting with neurotransmitter substances in living brains, analyzing electron microscopic slides, preparing light microscopic slides, using “gels” to separate out molecules of different sizes into analyzable “bands.” These simultaneously
performed activities showed little ostensible relation to one another, at least to my relatively naive observation early in my inquiry. In later interactions with lab members, I learned that no single member had detailed access to the total range of activities in the overall research program. The lab director's access to the different projects approached such an overview, though he remarked to me that it was difficult to keep in touch with the many projects, since each project became even more complicated in the specialized and relatively autonomous course of its production by his associates and assistants.

Members other than the lab director were specialized in the way they worked, and were not responsible for the entire range of inquiries that could be seen to be going on around them. Instead, the various specialties were featured in circumscribed projects, and were somewhat less directly related to other such projects that went on. For many of the lab members, the scene took on a specialized visibility, where activities were sometimes visible to members as a lab spectacle and at other times as practically available circumstances. Whether the lab was visible in the one way or the other depended on the specialized access of a practitioner to what he saw.

The lab's research was not organized into a uniform and coherent design where members worked concertedly and progressively toward an identical objective. A plethora of projects, with particular topical concerns, instrumental modalities, and technical specialties specific to each project was characteristic of the lab environment. The variety of projects were not unrelated to one another, nor were they organized into a coherent topical progression. Specific projects did not stand in isolation from any and all others; rather, a variety of relationships between successive and simultaneous projects appeared.

The serial and simultaneous relationships between actions in any single project, and between projects, appeared to be a substantive issue for the inquiry, conceived of as a developing social production. The issue that is raised here in regard to such relationships is the way in which the detailed actions in shop work achieved something on the order of an organized project, that is, how the orderly character of a project emerged as a feature of the temporalizing actions of members.

The temporalizing of methodic actions

Science is perhaps unique as an occupation for the way in which its shop work is made extensively accountable in written report, and examined for its rational, logical and systematically achieved...
how the actions in a project achieve their organization, if that organization is not identical with the enactment of a predefined “ideal” format. Although it can be assumed that sequences of project actions are designed as extended courses of procedure, their production appears to be more than a matter of faithfully executing a plan of action. Even in those instances in which executing a plan may be involved, how this “execution” is achieved in the actual circumstances of the work remains unexplained if one only consults the formulated plan.

The progression of a project from one constituent task to another, when examined in its local production, was found to be more than a matter of “enacting” a methodological scheme. Although actions in a project were produced and formulated in ways that made painstaking reference to the relevance of methodic, rational, and rule-governed procedural norms, the fact of the achievement of a displayed-reported correspondence occurred as a locally managed social production. That topic will be conjecturally explored here under the rubric of the sequential organization of a project’s course, and will be analyzed in terms of the unexplained existence of local management practices at the heart of the projection of a project within the immediate environment of laboratory work.

Sequential features of a project’s course

A project, as it is formulated in the lab “how-to” recipes, methodological accounts, and members’ verbal descriptions, consists of a sequential order of steps or tasks. These steps ordered with respect to one another in a conditionally relevant way, where steps contain features which project the occurrence of future steps, while being produced on the basis of the occurrence of earlier steps. For example, histological procedures are arranged in sequences in which the slides are first immersed into a solution that contains a heavy metal ion which deposits selectively on particular protein structures in the membranes of the cells. Later, the slides are immersed in liquids that wash-out excess precipitates of the heavy metals, so that the configuration of the deposits is preserved. Throughout the organization of tasks we find that an order has been established in which prior renderings make the tissue available in a way that makes later renderings possible.

Looking at an entire sequence of histological procedures, we find that the order of activities consists in setting up the possibility of the visibility of structures within the instrumentally enhanced horizons of the embodied possibilities for seeing, manipulating, and otherwise experiencing an involvement with the materiality of microstructures. In placing these “objective structures” within the grasp of practitioners, an extensive sequence of prior tasks shows an exquisite relatedness between constituent actions which make the material progressively visible in the preferred way and allow further operations to be performed.

In the particular project studied, the “steps” of the procedure were designed and arranged successively to allow an array of ultrastructure to be photographed in such a way as to permit a “geographical” identifiability within the hippocampus, and to allow measurements to be taken along distances between recognizable “landmarks” within that geography. (The use of geographic imagery here is not my construction alone, since practitioners were observed to provide for the brain’s accessibility in terms of an “atlas” of three-dimensional co-ordinates and expectable anatomical locales and configurations within the intervals of those co-ordinates.) Accordingly, the project’s technical aspect consisted in a sequence of rendering practices for transforming a living animal into a specimen for photography. For this to be done, an animal had to be killed, perfused, the brain extracted, “fixed,” mounted on slides, and placed under the electron microscope. Although the resultant photographs by no means resembled the original, naturally conceived “animal,” they were numerically indexed to a particular animal and taken as the relevant visibility of that animal for the sake of the inquiry.

Practical features of a project’s sequence

The above account of a project’s sequence relies upon the formulated integrity of a project’s course to find in the formulation how “steps” or “tasks” were articulated with respect to one another in forming the “complete” sequence. Although such an account of a project’s course has its relevance in lab work as instructions, or as an index of the work for those accomplishing it, it by no means describes the practical availability of a project to one accomplishing it, and it altogether fails to refer to features of the sequence of a project’s performance that are available in situ.

My appreciation of such practical features of a project’s sequence was gained through extensive observations of a developing electron microscopic project concerned with the phenomenon of axon regeneration (or sprouting). Field notes were taken in noting the progression of visible-and-describable features of members’ conduct while they worked in the lab, and conversations with participants allowed me to gain at least a minimal sense of the actions that appeared in the lab environment. In addition,
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assisted in some of the simpler lab tasks, and recorded many hours of practitioners’ shop talk while they were in the midst of doing the work of the project. This experience with the progressing of a project from its onset until the time of its completion in the writing of a research paper are treated in the following remarks on the character of the practical and technical work in the project. My practical involvement in the project was limited, as was the corpus of materials used in warranting these remarks. The described features do not in-and-of-themselves constitute an adequate analysis of the practical availability of a project in its course, but do suggest some issues that would have to be confronted in such an analysis. In any event, the features that are described below exhibit that in many ways a project, as a performed-course of action, shows features of design and continuity that are unavailable in methodological accounts of scientific work, and that these features are by no means trivial aspects of scientific practical reasoning.

I Practical continuity

A particular project was not synonymous with a sequence of members’ activities in “real time.” Although definite sequential relations occurred between successive actions in a project, that succession was not found in the continuous “clock time” temporality of a working day. Several considerations arise from this distinction between a project’s sequential occurrence and the succession of a member’s actions in the course of a “working day.” (A working day is somewhat distinct in lab work, as a member’s presence is not governed by, for example, a “nine-to-five” schedule, but is in large part determined by the requirements for monitoring experimental and observational phenomena which have a temporal organization that is at times indifferent to preferences for work scheduling.) Members commonly would not devote themselves to only one project at a time, as they were responsible for doing “more than one thing at a time.” This was made possible by arranging the shop work in a way that permitted the successive or “simultaneous” production of unrelated project tasks:

1 For instance, a waiting period for the duration of the hardening of the epon-araldehyde plastic used for embedding slabs of brain tissue could be used for performing other tasks in a separate project or in a different phase of the same project, such as staining a batch of slides. The two sets of tasks, arranged as they were as successive actions in a working day, were nonetheless not adjacently arranged in terms of the course of any particular project.

2 A task, insofar as it was featured in more than one project, could be accomplished to produce materials for several different projects. For instance, a worker’s time at the electron microscope could be used to produce photographs from slides prepared for different projects. In an afternoon session at the electron microscope, a practitioner observed collections of slides of different brain regions, which were treated with different stains, viewed at different magnifications, and destined for different courses of subsequent procedures. The period of time spent at the scope could be used to take pictures from the collection of slides, which would then be indexed separately to later appear as data in separate courses of inquiry.

3 Tasks in a project were variously interruptable or postponeable. Some “next” tasks in a given project’s course were left unattended while alternative projects were pursued in the shop work. Depending upon the character of the in-course project, “next” tasks could be put off indefinitely, for a short time, or not at all.

Instances

The following are reconstructed versions of project actions, which were taken from field notes. They are not intended as literal accounts of particular occasions (if such were possible), and are not “data” in the sense of providing examinable materials of unanticipated richness of detail. Instead, they are provided as extensions of this text about the sequential character of a project, and as such are to be read as part of an argument rather than materials used under an analysis from which the argument took its initiatives.

(a) Prepared slides to be photographed for study of the anatomical array of a peroxidase stain’s course through a layer of axons were filed away and left in a drawer for an indefinite period of time until placed in the scope and photographed.

(b) Some photographs of the ultrastructural “region” of the dentate gyrus were left unanalyzed, “on file,” until members “found the time” to work on their analysis.

(c) Embedded chunks of brain tissue were left in storage until subsequently treated as materials for the same or another project. The hippocampus of one animal could be used to produce an almost infinite number of electron microscopic photographs, since the area occupied by a thin section was infinitesimal when compared to the volume of “usable” space in the hippocampus. Once the brain of an animal was extracted, and the hippocampus dissected out, slabs of hippocampus were placed in liquid plastic which was then baked to harden. These slabs could be used to produce many more sections for electron microscopic photography than could be managed within any one project, so that material
was “left over” at this stage of production and could be rendered into slides for different projects at a later date when hippocampal material was required for electron microscopy. An animal thereby became available as varieties of materials to be used in subsequent projects . . . its remains, as embedded slabs, finished and stained slides, photographs of slides, and records of analytic operations performed on the photographs, were on hand in the lab as potential materials for an indefinite variety of subsequent renderings. (An embedded slab could be treated with new sets of stains to reveal different structures than previously. Slides could be re-examined in light of a different anatomical interest. Analyses of photographs could be made again with a different focus.)

(d) A lesioned rat had to be “sacrificed” on schedule to permit its placement in a comparative study of neural regeneration. Once the sequence of a procedure for “sacrificing” was begun, postponements or delays could only occur within very narrow temporal limits.

(e) Slides waiting in a staining solution were removed within a relatively narrow (though variable across practitioners and occasions) time interval. The use of clocks and “timers” monitored the occurrence of actions for transferring slides into and out of solution.

(f) Perfusion (injection of a preservative fluid into the heart and vascular system of a dying animal – utilizing the still pulsing heart to spread the fluid into the brain) was accomplished within very narrow limits of the demise of the animal. The procedure was timed with the animal’s death in such a way that if done shortly after the heart ceased pumping it was said to have been done “too late.”

The sequential ordering of a project’s course (as alluded to in the instances above) shows “places” where the activity can be left off or interrupted, while during other intervals in the project’s course, successive activities were required in rapid temporal succession, sometimes with the use of clock time as a device for organizing the pacing of activities (as in (e)), and at other times with an orientation to “events” being observed (as in (f)).

The precise character of the work in its provisions for segmentation, continuity, and stopping and starting has not been provided at this point in my research. These preliminary observations indicate, however, that projects are constituted in a manner that provides for “places” where a project can be “left hanging,” abandoned, or reorganized after being started, with its materials being re-collected as elements in a different “story.”

Various circumstances were citable in pointing to why a project would be shelved, such as overt failure, alternatives to pursue, or lack of expected funding. In one case the possible abandonment of a project was a serious issue for the lab in light of a repeated failure to verify results of a promising experiment during a series of carefully designed replications. Several months were spent while researchers attempted to repeat the initial result. After each negative result came in the experiment was altered slightly in further attempts to “control” for any circumstantial “variables” which were imaginable as “factors” that may have suppressed the result. Since the list of such possible “factors” was always extendible by taking into account further analytic aspects of the experimental procedures and circumstances, the repeated failures were not definitive grounds for abandoning the project. Instead, the eventual “shelving” of the project involved the assessment of the possible discovery as not worth pursuing any further, in light of practical necessities for maintaining the lab program with routinely successful findings. Closure was not attributable to a disproof of a hypothetical possibility, since it involved a “turning away from” a possibility for further research in favor of more practical alternatives in the lab’s inquiries.

II Overall design of the inquiry

In the work of the lab as a whole a kind of “overall design” was noticeable. The variety of competences that were employed in the lab were mostly involved in studies of the hippocampus and tended to “inform” one another in a short-term historical manner. Accordingly, some kind of coherent progressing of the lab’s various projects and a variety of topical ties between projects could be observed. Not all projects, however, were easily relatable to one another on topical grounds, nor in terms of allocations of personnel or distributions of technical competences. Projects tended to be associated with one another within certain ongoing inquiries such as the study of axon sprouting. Several of these inquiries were going on at the same time and provided a generative context for “next” projects by occasioning “issues,” “unexplored areas,” and “information needs,” that is, the various enterprises in the lab provided a local corpus of studies which provided an orderly, though perhaps not a unitary, basis for further studies. Practitioners sometimes collaborated in different combinations to produce projects within the different topical areas. These topics were by no means closed with respect to one another, nor were they always mutually relevant. Studies of the biochemistry of axon sprouting arose subsequent to, and in relation to, anatomical and physiological studies, and methods from studies of neurophysi-
ology in the synapse were adapted to the study of the physiology of axon sprouting.

I have not had much access to the phenomenon of the overall design of the inquiry. Although a corpus of research reports produced by the lab's studies over several years provides a rough outline of topics of interest and their successive manifestations in published reports, and although lab members were willing to give historical accounts of their current preoccupations in research, those accounts did not allow for strong inferences as to how the lab's program was produced in the immediacy of any contemporaneous situation. Topical progressions could be traced, and orderly structural arrangements were evident in those progressions, but these comprised "after the fact" records of a collective course of action that was differently available when the doing of the research was at issue—i.e. when "plans" were made or changed, projects initiated or "scrapped," and decisions made over the "present course" of the research.

It is unknown at this point in my research whether members of the lab had access to the phenomenon of how the overall program of the lab achieved its direction. For many of the students and more recent members of the lab, the program was mostly available in terms of immediate sets of technical tasks, with a developing horizon of prior studies, and concurrent studies not related to those tasks becoming available at the scene of the work. This was how the lab was available to my inquiry, given the amount of practical involvement and familiarity with the detailed workings of the setting I attained. I can only speculate as to whether something in the order of an overall program was available, and as to how it was available to "veteran" lab members and especially to the lab director. Certainly, the lab director had a more comprehensive overview of the lab's programs than did any other lab members, though he had only limited access to some studies that were pursued fairly autonomously by visiting scientists and post-doctoral students. Some control over lab policy was available to the director, though I cannot speculate as to how comprehensive that control was, and how it manifested over the exigencies of an extended historical course (of, for example, five, or ten years). Although it seemed clear that the topical progression of the lab's research was not a unitary development of a coherent inquiry, I cannot as yet, lay out its organization in any detail.

III Detailed sequences of action in situ
Observations of the course of a project's accomplishment revealed an order to the performance of tasks in a project that was at considerable variance with the version of the project as a sequence of "steps" which appeared in recipe accounts of the "methodology." Although methods prescriptions were used as instructions for doing series of tasks, they did not account for varieties of properties that appeared in the examination of an actual sequence of work in a project's performance. (In the Appendix I have recorded a set of instructions from a methods text which treats the topic of "fixation by vascular perfusion," and I have placed this along with a "behaviorized account" from my field notes of a perfusion procedure witnessed as it was performed on a cohort of animals.\textsuperscript{16} Remarks in this section about the detailed character of a task's performance are examinable in light of a contrast between the accounts in the Appendix.) To point to differences between "methods" accounts and the technical details of the actual performance is not to fault the methods accounts for their insufficient detail, but is to take notice of unformulated features of practical action that are relied upon in the methods account. Some features of the work's detailed performance as it was witnessed in the actual research setting are as follows:

1. Whereas in the written account, a uniform procedure is posited for the renderings performed on each animal, each instance of the "processing" of an animal required work that arose in the circumstances particular to that instance of the performance of the prescribed sequence.

2. In the written account, the comparability of one instance of the performance of a serial order of "steps" with another was implicit in the formulation of a uniform sequence on behalf of the variety of instances of the performance of that "same" sequence.

Within any occasion of performance of the sequence of procedures, the comparability of one instance of the accomplishment of the serial order of steps with another was essentially a problematic matter. The variations from one performance to another were treated as "troubles" which were assessed for whether they rendered any current performance usable in the project. There was a tendency to describe the results as "unpleasant" when troubles occurred, and to assess the usability of the results in light of the troubles.

3. The actual series of actions for any given task was indefinitely more complex than glossed in the methods description. To put this another way, a "task," when analyzed for how it provided a constituent action in a project, became a very problematic matter, in that a "task" as formulated in a methods text became indefinitely complicated when viewed in situ. A "task" appeared to be an achievement which relied upon the situated use of unformulated
Tasks
I have spoken of “tasks” as if they were irreducible procedural elements of projects. A detailed examination of the sequential accomplishment of a project, however, showed that the matter of sequential “units” in a project is highly problematic when the units are considered as segments in actual action sequences, and may be more appropriately discussed as a kind of notational convention in the writing and reading of methods accounts. (I have found, in my formulations of a project in this text, that couching description in terms of “task” notations has been an, as yet, unavoidable resource.) The following remarks suggest some considerations in the sequential analysis of “tasks” in a project.

(a) Certain “tasks,” in some fashion, were treated as transferable across different projects. The lab’s compendium of instruments, supplies, and expertises provided for the recurrence of technical procedures as features of different projects of action. Perfusion technique, embedding technique, sectioning and staining procedures, and fixation methods were all sequences (or formulated accounts of typified sequences) that could be treated as independent from any particular project.

(b) Some tasks were performed with variations that took into account the particular character of a project of inquiry. For instance, staining procedures were designed to elucidate particular structural forms, and could be varied in context to develop a slide to show a particular effect that was intimately fitted to the investigation at hand. Not only could an available staining “technique” be selected to highlight (or “label,” in the terminology of practitioners) a thematic cellular constituent, a particular technique could be worked with in ways that would give it unique potentiality as manifested in the circumstances provided by the particular project.

(c) Some tasks were designed specifically for a given project of inquiry. Pieces of equipment were built especially for a need arising in a new project (such as chambers for keeping brain cell pieces “alive” in a liquid medium, a relatively new development that required a design of equipment consonant with the novel situation), and existing instruments were reconstructed to “fit” particular projects. Electrophysiological equipment required continual rewiring with each experiment, and practitioners were expected to be able to work the equipment to adapt to contingencies of developing experiments. Improvisory work of this sort required more than a “rote” understanding of how to perform tasks, and necessitated a working acquaintance with the machinery, the chemical composition of materials used, and the detailed regional anatologies of brain areas.

(d) Performance of procedures necessitated numerous reparative practices where a procedure would be inserted within an ongoing task to deal with troubles of various sorts. Some “repairs” were rather routine and were inserted on occasions of expectable trouble (one such case was in performing a lesion, where a subcranial artery was often ruptured during the operation. Cotton swabs and a powdered substance to aid the blood clotting were placed within reach of the operating table, and formulated the expectable character of using those materials in the event of the rupture.) Other remedies were not so close at hand, nor were the troubles that occasioned them anticipated in the organization of the task at hand.18

(e) Contingencies of “this animal,” “these persons,” “this particular developing task” provided each instance of performance with its unique character. Notable realizations of that uniqueness often took the form of “troublesome” features of a task, where the task could not be completed simply as a matter of routine, and the results of the achievement became problematically comparable to other instances of the task, and possibly unreliable.

(f) Although for some tasks an extensive rationale could be given of its mechanical, chemical and electrical workings, for other tasks no such rationale was given for the expressed reason that it wasn’t known. In such cases (many of which arose in staining work with heavy metal compounds, which were said to be “capricious” for unknown reasons) practitioners managed the task so as to find ways that worked “often enough.” A body of “superstitious” and “personal preferences” were said to be characteristic of some procedures. A superstition was a retrospective discovery that consisted of an attribution about aspects of equipment and technique (lengths of waiting periods, temperature measures, chemical concentrations of solutions, etc.) to the effect that they were variable or dispensable in ways not previously recognized in a procedure’s construction.

(g) Although different tasks in a sequence for a project did not, by necessity, require their being performed by the same cohort of persons, in the lab studied, members almost invariably “processed” an animal through the entire sequence of renderings. A rationale was given that the organization allowed a practitioner
to have access to the "history" of the procedures upon which the ultimate visibility of the studied phenomenon was seen to rest. (h) Some tasks were said to be available in the reading of methods descriptions. Other tasks, especially those tasks developed uniquely in the lab (such as a procedure for inserting an electrode within the body of a small "interneuron" cell without bursting the cell and thereby removing the possibility of recording its ongoing activity), were said to require a course of embodied instructions in the particular research setting, and could not be adequately formulated for instructional use.

Summary and conclusion

This chapter provides some preliminary remarks on the temporal character of laboratory shop work. It does not yet approach a "material demonstration" of that phenomenon, since it remains to be seen what would count as materials adequate to the display of shop work for the purposes of an interest in the phenomenon of project. It provides instead some loosely organized ethnographic observations which are believed to point to consequential features of a project's accomplishment.

A demonstration of such features (or of other features that would be discovered in a more intensive analysis) might be possible through the use of videotaping action sequences of lab work. For a variety of reasons, however, some of which have been mentioned in this chapter, video analysis might not be entirely appropriate to the phenomenon. Since, as noted previously, the continuity of a project is not a "real-time" continuity, sequential structures would not necessarily be discovered through the witnessing of a continual stream of any person's or set of persons' activities. The analysis would need to supply, through ethnographic or other means, a way of indexing successively displayed actions to their appropriate projects (although it is anticipated that the observable actions might be seen to show continuities and discontinuities of involvement in unitary project sequences).

I have made efforts to reconstruct the work of a project through an analysis of the record-formats used by practitioners to display their work to themselves and each other prior to any official writing up of results into a finished document. Varieties of "in-course" records were used in the electron microscopic project, such as electron microscopic montages, oscillographic readouts and notations, and lab members' notebooks. These records were distinguished by the fact that they recorded events in the course of a project's performance which did not appear in published write-ups of the project. Analysis of these accounts provided a number of insights into the "unedited" course of the project's occurrence, but these were only problematically relatable to properties of the action as it was performed, witnessed, and talked about by members. Although the records produced in the project collected the work into extended and examinable continuities, their analysis led to insights about a "disembodied" sort of work, and it was found that tape recordings of practitioners' shop talk, however fragmentary their involvement in extended sequences of work practices, were preferable as access to the in situ projection of a project's course.

Appendix: Fixation by vascular perfusion

Exhibit 1


Method

In Figure 3.1 the intravenous bottle (1) is filled with Solution C. [Note (M.L.) chemical recipes for solutions, such as "Solution C" were provided previously in the text.]
ETHNOGRAPHIC ACCOUNTS OF SHOP WORK

The intravenous tube (B) attached to it is filled with Solution B so that its drip chamber (2) is partly filled, and the clamp (3) tightened. Tube A is filled with Solution A by syringe at the end where the cannula (4) is later attached after closing the clamp (5). The drip chamber (6) is filled with enough Solution A to allow the clamp (5) to be manipulated so as to fill the cannula (4), and yet leave the chamber (6) about half-full. It is important that the tube A and the cannula (4) be completely filled without air bubbles. The perfusate bottle (1) is hung about 5 ft. above the animal. At perfusion the cannula (4) is inserted into the left ventricle and the root of the aorta, tied in place with a previously placed ligature, the right side of the heart opened widely, and the clamps (3) and (5) released. Following a perfusion of about 15 min. at room temperature, the brain is removed and placed in Solution C, and small pieces of tissue carefully sliced. The tissue is postfixed with buffered 1% osmium tetroxide for 3 hours.

Exhibit 2

Behavioralized account of perfusion procedure (from field notes, 22 April 1975)

In E.M. room 2 (one of the rooms used by the electron microscopists), cage with 5 rats with stitches in heads (from lesion). Rats have numbers written twice on tails.

Tags on cage: (name of lab head/name of student in E.M. project)

Rt. Ent. Lesion (right entorhinal lesion)

4/19/75 in p.m. (date of lesion)

#1, 2, 3 (number for rats in that cage)

Other tag: Gold

B. January 26, 1975

R. March 28, 1975 (biographical data for rats)

Next to cage – seven syringes, a metal box with wood shavings inside, sink, rubber pad, hoses with clamps leading from inverted bottle labeled “conc,” collection of scissors, clamps over to the side, manual open to section on perfusion.

J. put on rubber gloves, grabbed rat by the tail, put into metal box, stuck needle into bottle, 1% sodium nitrite.

Rat 1

grabbed rat by the tail, jabbed in the belly, it squirmed and wriggled free. J. bent down so that forepaws of rat rested on floor, jabbed it several more times.
lets the rat lie for several minutes
takes different pair of rubber gloves, clear plastic
takes jar, pours vial of clear liquid from hood
puts on gloves
takes black felt pen, marks “1” on lower cheek of rat
run 500 ml. through of fluid
takes scissors out from drawer
opens jar
takes clamp, puts on tube near needle, takes out needle
hacks head off with scissors (conventional paper cutting scissors)
puts in jar, closes jar, puts near sink, head is face down in fluid
takes plastic bag, places on floor near waste basket

Next victim
repeatedly jabs it in lower belly
takes carcass of other rat out
untangles tubes (of suspended bottle apparatus)
takes rat out, it moves a bit, J. mutters, “oh shit”
waits awhile
tries cutting it with scissors
it doubles up and kicks, tries to bite
tries again, it kicks
gives it another shot
rubs belly
cuts again, it kicks
waits awhile
cuts - it squirms a bit, but not as much
cuts up belly
it jerks, squeaks a bit
waits after skin cut away
J: “fuckin’ animal that doubles over and dies on you”
cuts up chest
spreads it, pokes around to expose heart
deepeens cut, probes around with scissors
cuts hole in heart
jabs needle in ventricle
hole cut in auricle
checks fore arm of animal - it’s loose
J: “this one’s fucked - it’s worthless”
grabs scalpel, throws it aside, changes angle of needle
cleans off a small pair of scissors
checks arms and head - they’re stiffer
puts “II” mark on lower jaw
J: “it’s still not very stiff”
drains blood off mat

runs water over it
checks head and arms again
takes clamps out and needle, etc.
cuts head off with scalpel
blade slips off
sticks needle directly near back of head
runs fluid on spinal areas briefly
pulls lower jaw to see if it’s stiff
grabs jar, puts head in it
(head) smaller this time
puts plastic sheet over the jar
throws rat into sink
me: “what do you think happened with that guy?”
J: ((mutters)) “I don’t know . . . .”

Rat 3
two shots went without much struggle
waits about a minute or two
between first two and third shot
J: really in foul mood, allergies acting up and he’s not in mood
to explain anything in detail
the third shot - rat really squirmed - fluid ran out of wound
jabbed it again
sets up two scissors - small and medium size - one rib clamps
reached for rat, it squirmed
waits, grabs tail
puts on pad, pokes it, it starts twitching and squirming
towels out blood
clamps artery to left of heart
has trouble poking needle through wall of heart
J: “this is the toughest fuckin’ heart I’ve seen”
lower legs twitching
checks head and forepaw
pokes around with scissors, mutters, “fuck”
cuts off head
pours remains of “conc” bottle into jar, its the same stuff as the
stuff in the hood
replaced fluid with bottle marked, “dilute”

Rat 4
checks needle to see if its (fluid in syringe) running out regularly
- the manual - contains descriptions of procedures and
equipment that aren’t used, like shaving belly (of rat), hair
clipper
gives shot to rat
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takes it out
rubs belly
squeezes it with scissors
probes around with clamp, cuts out breast plate, pokes
[gap in notes – due mainly to redundancy in observed particulars]

Rat 5
First two shots while Rat 4 is still perfusing
beheads Rat 4 – puts head into “improvised” jar
doesn’t cut out breastplate
severs a membrane around heart
checks distance of spray from needle – spray goes out immediately
J: “fuck a duck, where’s that coming from, leak?”
sticks finger on leak
J: “c’mon, stay in there” – needle
cuts side of heart again
fluid running out nose (of rat)
tries cutting side of heart again
leakage around needle
J: “doesn’t look like this guy worked too well”

Remarks on account of perfusion (Exhibits 1 and 2)

In a write-up of the “ultrastructure” project subsequent to performance of the procedure, the procedure was summarized as follows:
“sacrificed under nembutal anesthesia by intracardial perfusion utilizing a mixed aldehyde fixative media.”

The written account acts as a gloss over the exigencies of the several times that the procedure was performed as an accountable instance of perfusion.

The witnessed series of perfusions was observed to be distinguishable into unique attempts. Each unique attempt was distinguished from the others by:
1 Its witnessable temporal presence as a “this time” instance of the action. Even in light of its visible typicality, each instance was witnessed as uniquely “this one.”
2 Its particular circumstantial troubles. Not only was each instance a unique event at the time of its performance, but it took on unique sequences of action by virtue of troubles that arose within the procedure, and which became occasions for remedial attempts.

In the account above, such troubles were encountered in the animal’s attempts to escape (Exhibit 2, Rat 1), the failure of the animal to easily “go under” the anesthetic (Rats 2, and 3), the “toughness” of the heart tissue to puncturing in the procedure (Rat 3), and a leak which sprung in the fluid’s course through the vascular system (Rat 5). The troubles pointed to the circumstantial character of each animal in its operative situation. For instance, troubles were inherent in the animal’s frantic attempts to avoid compliance to the procedure, and in the arrangements of equipment and their possibilities for falling apart.

The troubles called for remedies, some of which seemed to be rather typical remedies. That is, they were provided for as contingencies that had been encountered before. (In some methodological texts, typical troubles are sometimes formulated in contingent appendices to the idealized description of the procedure. At various points in the sequence described, an “if” prefaced insert can be placed within the formula to deal with a commonly occurring trouble.) “Repairs” often took the form of a repeat of a procedure with some modifications taking account of the developed situation, as for instance, in the above account of Rat 1: “jabbed a rat with hypodermic needle in the belly, it squirmed free; placed the rat near ground so that forepaws scrambled for floor, arching the belly of the rat” (a procedure which prevented the animal from doubling up on itself and biting the needle at its belly). Another instance showed a more drastic modification, where for Rat 2, a test of the flexibility of the forepaws showed that the perfusion had not “taken” completely when injected through the heart. An alternative procedure was performed, where the head was taken off and the perfusion solution was injected directly into the spinal cavity. This repair was possible on the basis of the practitioner’s access to the intentional character of the particular procedure. That is, the injection of the heart was performed as a way of getting the fluid into the vascular system permeating the brain. When the normal procedure did not work, an alternative course was taken, which was to inject the brain directly with the fluid.

Troubles occasioned complaints when the circumstances were not compliant to the procedure, and remedial attempts were tried to enforce that compliance as a practical matter.
3 Despite the particular circumstantial troubles, the work went on. Visible failure of the procedure (as in Rats 2, and 5, where J complains of the failure of the procedure) was not enough to assure abandonment of the animal’s usability. The brains were readied for the next step of the procedure, and treated as if they had been prepared sufficiently well for the purposes of the project. In some cases it turned out later that the materials from an animal, when examined microscopically, showed peculiarities. At those
times, practitioners sometimes invoked a failure in a preparatory operation (such as fixation) as having been responsible for the visible peculiarities (or artifacts). When subsequent analysis of the material showed no such gross problems, the earlier "failures" were no longer retrospectively invoked.

To generalize from the above: in scientific work, variability in the performance and results of any "this time" instance of an accountable identical procedure is treated as trouble. Although these troubles are often dismissed as the inevitable and surmountable variability of results, they are, nonetheless, occasions where the inquiry explicitly recognizes the ongoing character of its accomplishment. Perhaps, I should say, trouble is an occasion where the uniqueness of any "this time" performance of a "method" becomes accountable as such, and is displayed in the work as a specific matter of concern.

In later chapters I will focus on shop talk as it was witnessed in the setting and recorded on audiotape. A recurrent theme in the analysis of that shop talk will concern how the talk stands in relation to an ongoing project's course. It will be claimed on the basis of the analysis that the projection of the project was not simply a matter of fulfilment of an a priori methodological design - an idea that has been suggested in this chapter, though without a strong documentary base. Furthermore, in this examination of shop talk we will be able to consider features with a more detailed involvement in the actual work of producing a project in the collaborative and temporal setting.

Notes

1 The term, "temporalization" has enjoyed widespread usage in the Heideggerian tradition, and has been topical to some rather extended (and often arcane) philosophical treatises (cf. Derrida, 1973). In this chapter I am using the term in a rather concrete manner, and I am, no doubt, unfaithful to the subtleties of its usage in existential philosophies. I use the term (rather than, for example, the "temporality" of lab inquiry) to display my concern for the character of actions as ongoing and developing achievements, rather than as finished sequential products or as typified schemes of action. Although for the most part I address myself to actions that are now visible as "completed" sequences, and I inevitably exploit that finished character in pointing to "features" of projects of shop work, I wish to eventually make reference to the in-course character of a project, as a developing circumstance ... as the unremitting work of project-ing actions and forming-up the identifiable "units" of a sequence.

2 My use of the term, "productive analytic," relies upon Harold Garfinkel's lectures on production accounts (for an exposition addressed to production accounts, see Girton (1974)). What I want to be saying with the term is that practical actions take on an appearance in their organizational settings that is analytic in at least two senses: one is that the "visibilities" of a setting are variously identifiable within that setting in accordance with practitioners' "readings" of the scene - what counts as a storiable event, seeing that things are being done well or poorly, down to the very details of what is "seeably" being done. The second sense involves the doing of the action, where its temporalization is performed in a way that exhibits regularity; that is, identifiable configurations are "repeated," conventional starting and stopping points are established as "bounds" of actions "units." This is to say that "productive analytic" characterizes the witnessability of performance, not in the manner of "perception of an object" alone, but as a being-with the action (either as author or imaginatively as one who understands the detailed course as a practical accomplishment).

3 My preoccupation with sequential formats in the production of shop work is indebted to conversational analysis (see Sacks, Schegloff and Jefferson, 1974). Like conversational analysts, I am concerned with the recovery of sequential structures that are integral to members' work of constructing their activities with an in-situ identifiability. Unlike students of conversation, I do not have, as yet, an established sense of what would count as "materials" for investigating and-displaying sequential structures in the analyzed activity. Compared with accounts of conversational "devices," my remarks on "features" of lab work are conjecturally based and not clearly indexed in a display of the "materials" reflexive to the environment studied.

4 The notion of the "design" of a project is highly problematic. I do not wish to give the impression that the work of a project consisted entirely in the fulfilment of a prior design. Instead, I find "designing" to have been a concrete phase of a project's course, and I leave myself uncommitted to the notion that "designing" refers to a way in which a project achieves its form as compliance activity.

5 My use of "preference" is a rather loose usage, when compared with the analytic sense developed in conversational analysis (see Schegloff, Jefferson and Sacks, 1977; Sacks and Schegloff, 1974). I mean by "preference" to say that abandonment of projects was in many ways treated as a cause for complaint, was formulated as "wasted time," and that one could find many reparative practices orienting to preserving projects from possibilities of failure.

6 Harold Garfinkel reformulates Merleau-Ponty's (1962) original use of the term, "spectacle," to mean, "the visibility of scenes whose structures of coherence and visibility are un witnessed and unwitnessable" (personal communication). I use the term to mark a contrast between the non-practitioner's "mere witnessing" of
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7 The lab's physical plant is a locatable and definite set of rooms and facilities. At the same time, these definite spaces are regarded as overlapping regions or territories for a variety of technical activities. For instance, equipment was distributed by rooms into histology, electron microscopy records and facilities, light microscopic equipment, tissue culture, electrophysiology, and biochemistry. The character of the rooms changed dramatically with shifts in emphasis in the inquiries in the lab. A central area was reconstructed (literally torn apart and rebuilt) as a chemistry lab in preparation for biochemical inquiries into axon sprouting. Different persons could be looked for and found in juxtaposition with the equipment locales, electron microscopy specialists being in their rooms as if the rooms were personal offices. These rooms were not personal offices, however, as they were "equipment" or technical offices which particular persons frequented when engaged in projects requiring the equipment particular to each room.

8 The description of the spectacle of "staining" here is already available as something sensible in a practical way. One could take the naivete of the viewer somewhat further, and have the viewer describe the scene as persons taking glass rectangles, placing them in colored fluids, and so on. The point is that the "scene" provides for endless descriptions, only some of which make a practitioners' analysis relevant to the description.

9 A number of practical microscopy texts are discussed in Lynch (1974b). Those texts, and theoretical accounts which take them to be "descriptive" of scientific practices, make up an immense literature.


11 According to Holton, "As contrasted with his largely unconscious motivations and procedures, the intellectual discipline imposed upon the physical scientist is now quite as rigorously defined as the conventionalized form for research papers. This superposition of discipline and convention upon the results of free creation represents a dualism in the work of the scientist which runs parallel to the dualism in the nature of science itself" (1963, p. 391). Although I am reluctant to ascribe the actual enactment of science to unconscious motivations, I find with Holton that scientists make a standing distinction between the way they do the work in situ and the way they reconstruct the work in published reports or in accounts about that work.

12 Holton (1975, p. 329), distinguishes "the time trajectory of . . . largely private scientific activity (S1)" from the time trajectory of "public" science (S2). He proposes that the historical study of science requires different procedures depending on whether its object is the "world-development" of public scientific knowledge (S2) or the "personal" and "private" situation of the actual research situation (S1). Here I suggest that (S1) as observed in the

lab setting was not necessarily a "personal" or "private" matter, but was visible as part of a social scene of competent practitioners.

13 Robert Merton, whose researches are far removed from the approach taken here, and who has been criticized for his "storybook version" of science reconstructed from the public accounts of scientists (see Barnes and Dolby, 1970), nonetheless noted at one time that studies of science relied almost exclusively on "idealistic" accounts of scientific activity in scientists' writings, and that there was a need for "a synthesis of historical materials and of materials based on first-hand field work" (Merton, 1952).

14 "Conditional relevance" is a formulation developed in conversational analysis to describe the retrospective and prospective orderliness of adjacently placed utterances (see Schegloff, 1972). In speaking of methodological schemes, formulated in written accounts, I am speaking of a predesigned order in which steps mutually elaborate one another, and a prior step contains features that set up the occurrence of later steps. In using the term, "conditional relevance" I only wish to point out the contingent character of the way one step is built upon the prior accomplishment of earlier steps. This usage misses some of the subtleties that are introduced when one considers the improvisatory character of conversations' achievements of sequencing where the "latching" of utterances is not available via any previously existing recipe formulations, and where the definiteness of a prior utterance as it becomes involved in the course of action is not assured from the outset. I am not saying that scientific actions as actually performed in sequence do not show this contingent improvisatory character. Instead, at this point in the discussion I am concerned with features of the methodological formulae that are given in accounts of scientific action. I shall then move on to discuss sequential features that are available only when one examines the actual production of the work.

15 When I speak of "adjacency" in a project I am not referring to actions that are visible as adjacently produced "in time." There is a "one-after-the-other" sense of timing, but not a uniform prescription for adjacency by juxtaposition in succession. It is more a "logical" sort of adjacency, where extended intervals can separate two adjacently placed temporal objects. A project, as seen here, consisted of successive "waves" of renderings on different cohorts of animals. The "waves" of practices were not necessarily accomplished in succession, with one cohort being processed, and then the next, and so on. Instead, the "waves" could overlap in various ways. An early sequential action (such as "sacrificing") could be performed for one cohort of five animals while another cohort was still "in process" at a later sequential "point" (say, preserved in plastic embedded brain fragments). Nonetheless, despite the real-time complications in the orders of processes performed during a project by persons in the project, an indexed-order of the serial processes for each cohort was kept, and the organization of
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these sequences of actions was available as a record-keeping practice.

16 The "behaviorized account" notes the appearance of the procedure witnessed from the standpoint of one unable to perform the procedure and seeing the mere appearance of the action. The account is not recommended as a practitioner's reading of the situation, though it does allow for the noting of certain sequential features of the procedure's "looks" which stand in stark contrast to the sequence of actions as formulated in the methods account.

17 This approach to formal methods of instruction relies heavily upon Harold Garfinkel's lectures and exercises concerning instructions and circumstantial attempts to "follow them" (Lectures at U.C. Irvine, Fall and Winter, 1972-3).

18 Here, as in other places in this text, I have adopted terminology from conversational analysis. It remains to be seen, however, if procedural "repair" sequences operate in the way described for conversation (see Schegloff, Jefferson, and Sacks, 1977). At present my analysis is not nearly extensive enough nor detailed enough to compare with conversational analysis, though it is not my intent to adopt a "theory" of action from conversational analysis and find it adequate to the description of shop work. Instead, I am using some of the analytical features of conversational sequences as a way to begin a sequential analysis of shop work, while reserving the option of developing that subject matter in a way adequate to its unique character.

4 An archeology of artifact

The following chapter is concerned with the phenomenon of artifact in laboratory research. A listing of specific cases of artifact-accounts which were observed in particular laboratory research situations will be examined for how they exhibit the work of laboratory science in the visibility of documentary evidences of neurobiological phenomena. Different varieties of artifacts will be described in relation to their differing treatment in lab accounts, and in terms of their consequence for the projects in which they occurred. Accounts of artifacts were drawn from the following sources: tape recordings of lab researchers' talk with one another during occasions when artifacts were discussed, conversations by the author with lab members on the topic of their practices, written research reports, and instructional manuals in microtechnique.

The term, artifact, has a specific usage in laboratory research which needs to be distinguished from a more general use of the term in other areas of discourse. Research artifacts comprise an indefinite variety of substantive and methodological features which appear in laboratory accounts of naturalistic phenomena, especially when the phenomena depend on specialized techniques or instruments for their observability. Artifacts are described in such accounts as particular "intrusions" or "distortions," in the observability of the "natural" features of the world which derive from the instrumental conditions of their perception. The danger that artifacts are said to present for research is that their presence can go undiscovered and be taken as evidentiary features of purportedly natural phenomena. Numerous examples of such recognitional troubles are cited in natural scientific writings as instructional warnings, historical descriptions of the "errors" of early scientists, and as arguments against the acceptance of
particular announced discoveries by contending researchers. Arguments over the possibility of artifact in particular researches are so prevalent in the sub-disciplinary literatures as to mark a major form in which disagreement is expressed in scientific discourse. For major discoveries, it is very often the case that charges of possible artifactuality in the discoverers' method soon follow in the wake of their announcement. The possibility of artifact is an almost inevitable accompaniment of research which relies upon specialized techniques and machinery for making initially "invisible" theoretic entities visible in documentary formats. Although the problem of artifactuality is mentioned most frequently in relation to microscopic observation, it is equally characteristic of research using radio telescopes, deep-sea cameras, bubble chambers, electrophysiological monitoring machines, and countless other items of laboratory equipment which provide unique access to scientifically postulated events and entities.

This sense of artifact contrasts with the use of the word in the discipline of archeology. Archeologists refer to material objects as artifacts when those objects can be related to conditions of human use. Such objects show constructed features which are analytically related to practical, artistic, or ceremonial circumstances. The discovery of artifacts in archeology sometimes occasions debate over how given material objects are distinguishable from mere stones, pieces of wood or bone, which occur independent of any prior human construction. The general problem raised in such inquiries is the reverse of that in lab science where an in-principle distinguishability of "natural" from "constructed" objects is relied upon despite troubles in formulating criteria that cover problematic cases, but with the difference that the discovery of artifacts in lab research is not cause for celebration. While artifacts are taken as a form of positive evidence in archeology, in lab science they are seen to detract from the evidentiary power of empirical data secured in programs of research. Artifacts in laboratory work are comparable to such things as scratches on a potsherd attributed to the archeologists' work of unearthing the fragment. In such a case the accountably constructive form of the scratch is reflexive to the discovering enterprise.

Artifact accounts exhibit the "unwitting" work of science in descriptions of a field of natural entities. In lab science the manner in which the world of, for example, neural phenomena, is actualized as an instrumental accomplishment remains unspecified in talk of axons, dendrites, glial cells . . . i.e., a world of "natural constituents." Such naturalistic references do not specify how the detailed presence of those entities to description — their concrete visible shapes, textural contrasts, size, extension, i.e. their tangibility — is a technical accomplishment in laboratory work. It is as though the complex of theories, techniques, and instruments which were involved in disclosing such entities become transitorily invisible in the way a clear pane of glass is unnoticed by the gaze that sees through it.

Alternatively, the instrumental complex is formulated as methods . . . a series of procedures to be purposively executed as a means to an observational and experimental result. In methods talk, the "technique" or "instrument" is referenced as a kind of practical object — an instrument with a functional order of parts placed at arm's length and viewed as a material thing with functional implications. The instrument when described in such a fashion is not accountable as an implicit condition for the appearance of natural microscopic things, for example, but as lenses, gears, filters, and measurable distances in planes of material operation. The machinery is explicated while its presence in a perception which interiorizes it is left implicit as "function."

One manner in which the technique/instrument complex becomes available and which contrasts with the above two usages is in accounts of artifacts. In such accounts the instrumentality of laboratory perception is not implicated in the form of "things" nor in accounts of "methods." Instead, the instrumentality of the lab's access to phenomena is revealed to be indefinitely part of the visibility of phenomena. This identification of instrument and "natural" detail is discovered in artifact accounts as a repulsive identification; it occasions attempts to remove the influence of the artifact and to restore the presumptively adequate relationship of lab practices to an assertedly independent order of objects.

In members' accounts of research artifacts, the observable presence of practices in a field of natural phenomena is distinguished as a negative presence. The procedure which will be used in analyzing such accounts in this chapter is distinct from the analytic program of the natural sciences in this respect: there will be no interest here in settling differences between artifacts and "natural" phenomena. Occasions of the appearance of artifact will not be programmatically separated from natural appearances, nor will a practical program be pursued for remedying any indeterminacies in the discernibility of artifact in a naturalistic field of perception. Instead, accounts of artifacts will be presented as exhibits of contexts in which practitioners' accounts yield detailed specifications of the visibility of laboratory practices in a way that is unique to their discovering programs. That is, artifact accounts will be shown to be a particular way in which laboratory work is made visible . . . a way which is embedded in specific topics of ongoing
research projects, and which is reflexive to the settings in which the research is accomplished.

In this chapter artifact accounts are appropriated as disclosures of the "unwitting" work of laboratory science. Occasions on which artifacts were discovered, described, and argued over are treated here as perspicuous moments in the work of lab science. Although artifacts were not encountered as a uniform set of phenomena, and appeared in varieties of accounts, they provided circumstances in which the positive manner of addressing a field of instrumentally revealed phenomena was interrupted, and accounts were given of the reflexive character of the discoveries of a lab upon the setting of practices.

The artifact accounts analyzed in this study treated artifacts as trouble, as moments in the work, where the ordinary transitivity of practices was a confounding issue. Attempts to specify sources of artifacts, as ways of addressing their troublesome character, invoked a temporal order of practices and an indefinite range of circumstantial features of the work environment. Such accounts provided "the work" with an elaborate visibility that was absent in systematic reconstructions of methods and which was unexplained in references to substantive and imagined features of "the brain." Accounts of artifact revealed ways in which a social organization of laboratory practices was present in an ultrastructural exhibiting of neural events and structures. Their in situ analysis disclosed essential ways in which those practices failed to efface themselves in an order of positive microscopic details, and exhibited a kind of "sociological" reading of natural science phenomena. This is to say that artifact accounts, in referring particular observed phenomena, experimental results, experimental non-results, and theoretical implications of those results back to sources in laboratory practices, identified social structures as something interior to the expressed topicality of natural science inquiries. This contrasts to any notion of sociological aspects of natural science inquiry as being extrinsic "influences" of occupational associations, personal histories, group memberships, norms and counter-norms, and "pressures" from an environing society.

At least on those occasions when accounts of natural phenomena are found by members to show artifactuality, there is some warrant for addressing "natural phenomena" as a social accomplishment. This is more than a matter of providing for how lab research provides a general context for members to agree upon what will count as adequately "real" phenomena. It is not a philosophical claim about the essential dependence of postulated objects, observed phenomena, or experimental results upon humanistic conditions of "common training" or "agreement." Instead, members' provisions in artifact accounts for specifics of how discovered phenomena are reflexive to laboratory practices and "circumstances" are not accounts "in general" of that reflexivity, but are detailed showings of a work environment as part of specific empirical displays of data. These accounts exhibit a researchable domain which is endemic to laboratory inquiry itself and indifferent to analysts' efforts to formulate generic principles of science's relation to its objects of study. Artifact accounts formulate situated relations of work in its environment which lab members invoke in accounting for particular troubles in their research.

Some problems with an "ethnographic" orientation to artifacts became apparent during my examination of particular laboratory accounts. First, it seemed an easy matter to note that the instances of artifacts presented in lab members' accounts did not exhaustively specify all that could count as artifactuality in their research. It seemed as well that an "implicit" horizon of artificial conditions could be provided in analyses of the formats in which data were displayed in lab documents, though this analytic order of artifactuality might never have been explicitly referenced in members' accounts of artifacts. Features of, for example, a microscopic record of neural "ultrastructure" can be analytically isolated as formal qualitative features of the record which give an artificial character to the field of neural entities accessed through a reading of the record. These features could include such matters as the two-dimensionality of a photograph's surface, its linear frame, the black-white textural variations that give outline to the forms of photographed phenomena, provisions for numerical measurement in the orientation of the photographic field, and the sequential arrangements of photographic series to depict a continuous sequence of "events." Such a version of the work's entry into the field of phenomena it studied, however, created an "asymmetry" with the accounts of lab members. For lab members the format of data display was not treated as a problematic matter as were particular discovered artifacts. Instead, the format was found as "useful" access and as adequate representational access, except when implicated in particular "troubles" with the adequacy of the record as access to neural ultrastructure. Where the form of the record may be spoken of as a general background of presupposed renderings of "invisible" phenomena, artifacts were on many occasions discovered as features of a laboratory's account of those phenomena that were not previously anticipated. Artifacts were discoveries in the sense that they were not often implicated in any general and formal features of microscopic analysis but emerged
as situated occurrences in the work, which had not previously been anticipated. This will be given more substance in the discussion of "situated artifacts" and "negative artifacts" later in this chapter. For the present it is sufficient to point out that instances of artifact discoveries were constitutive for what counted as a possible "influence" on an experimental or observational result, and that a general analysis of laboratory data formats in isolation from such situated discoveries led to an entirely different version of the conditions of instrumental access to neural ultrastructures.

It also became apparent that artifact was a pervasive issue which was not adequately treated in substantive terms. Artifact accounts were not limited to reports of observations of specific, thing-like phenomena which appeared in, for example, a microscopic photograph. Instead, the most interesting (and problematic) artifacts were not definite "things," but were "possibilities" that arose as part of specific (and often controversial) accounts. As possibilities they were not, as yet, specific features of any microscopic scene, but were tied to readings of the scene. Such possibilities were often mentioned as absences in an observation rather than definite constructive presences (spots, blotches, blurs in a photograph which can be seen as "intrusions"). The failure of an expected phenomenon to appear was of interest here for the way in which the absence could be formulated under different conditions as an artifactual absence or as a "real" absence. Under actual research conditions, such absences were troublesome since they were not necessarily definitive of any real worldly absences, but could be taken as "failures" in the technical ways of making a phenomenon appear.

Artifacts were featured in situations of indeterminacy. It will be claimed in this chapter that such indeterminacies were an essential part of the work of laboratory science, that they were irremediable in an ultimate sense, and that they could arise at any time. However, the clear procession of work in a laboratory project to definite conclusions in the face of such actual and possible indeterminacies was not foreclosed. The social and practical achievement of project continuity was conspicuous when considered in light of the innumerable ways in which things could always go awry.

Artifact accounts will be presented here in an "archeology" of artifacts. This "archeology" is not strictly an historical enterprise in the sense of being a reasoned effort to reconstruct institutions on the basis of material constructions which exhibit the transient actions which produced them. Instead, the focus will be on the phenomenon of artifacts in laboratory work as they were invoked in lab members' accounts. In lab work, artifact accounts were embedded in the settings of their production as part of the temporal course of laboratory projects. They did not occur as separate analyses of the residues of an alien culture's activities separated by a temporal gulf.

Artifact accounts did, however, have their "historical" relevances insofar as they consisted of members' attempts to isolate and sometimes specify the conditions of their own work, as that work was productive of visible displays of biological phenomena. It seems peculiar, at first, to state that members became present to the conditions of their work (its products, errors, i.e., its visibility as an accountable phenomenon) through the devices of retrospective discovery. In supporting such a claim it is necessary to refer to conditions of the invisibility or unwittingness of work to its producers at the time of its occurrence. Discoveries of artifacts make implicit reference to conditions in which members "did not know what they were doing" in some specifiable detail.

The phenomenon of error is accountably tied to such conditions, as in some cases errors were retrospectively discovered through analysis of a lab record. A practitioner who discovered such errors was often identical with the one who was assigned responsibility for an error.

There may be no reason to suppose that examinations of a large corpus of conversational materials from a research laboratory would lead on to a formulation of "artifact accounts" as a distinctly marked structural phenomenon in shop talk. Indeed, it may be the case that the interest shown here in such accounts derives from a prior recognition of a philosophical distinction between artifact and "natural" objectivities, which is then projected on behalf of lab practitioners as their standing problem of interpretation. It may be that an extensive analysis of shop talk will eventually subsume "artifact accounts" under a variety of headings, such as "assessments," and "arguments," which formulate local occurrences in conversation. Indeed, as reported in this volume, the author's initial interest in "artifact accounts" led to a study of agreement in shop talk (presented in later chapters), since artifact accounts were observed to occur largely as part of agreement and disagreement sequences in lab discourse (and at times in written discourse). Issues of determinability for cases of artifacts were local to occasions of members' shop talk and did not seem to be resolved in reference to formal criteria. That is, attempts to isolate criteria in reference to the particular forms that artifacts exhibited in comparison to those of accepted natural structures failed to account for the range of observed cases. This was especially true for those instances discussed under the headings of "distortions" and "negative artifacts."
ETHNOGRAPHIC ACCOUNTS OF SHOP WORK

criteria were suggested as, for example, geometric approximation in shape as opposed to “organic” variability, but this failed to capture any but the more unproblematic sorts of artifacts (such as the frame of a microscopic field, folds in tissue, scratches from a microtome processing).

In many cases, the use of the term artifact to characterize an aspect of the visibility of a display was bound up in controversy. Whether or not something was indeed countable as an instance of artifact had yet to be settled in the field of brain science in some cases, and in others was an argued matter among members of the local work group of the lab. Several examples of accounts which were involved in such controversies are used here for their interest as cases of artifact accounts regardless of the extent to which those accounts were established as unambiguous matters. (Some selection, of course, was practised in not presenting witnessed occasions of “jokes,” naively imagined possibilities of artifact, or “strange” accounts by non-credited contenders to scientific competence. Such accounts have some heuristic value in the way they show limits in the social accountability of a natural world, though they were not pursued here.)

An archeology of artifact

Varieties of artifacts became available in practitioners’ accounts of neural events and structures. They were pointed out as part of the instructions given to novices (including myself), as arguments over what particular microscopic photographs exhibited, as complaints and assessments of “materials” prepared in shop work, and as issues addressed in written accounts.

These accounts differed considerably in terms of their consequentiality for an inquiry, their recognizability, their inferred origins, and their remediability. If anything can be said to characterize these accounts in general it would be that artifacts were treated by members as troubles - whether or not those troubles were agreed to be consequential to the adequacy or “validity” of the displays in which they were found. Artifacts were located in complaints as complainable features of materials in hand. When discovered, they were not celebrated as the accomplishments of an inquiry, but were instead found as “mistakes,” “errors,” “unfortunate developments,” “problems,” “hassles,” “misleading appearances,” or “equivocal interpretations.” They were attributed to procedural origins in shop work with a variable degree of specificity, and, more importantly, their presence in accounts occasioned talk on whether the data in which they appeared were practically adequate in light of the identification of artifacts as features of those data.

Artifact accounts were not strictly classifiable into any strongly analytical ordering of “types.” References to artifacts occurred as part of situations in which practitioners discussed the immediate issues at hand in the visibility of a project’s concurrent course. These artifact accounts appeared within the circumstances of a particular project, and as part of situations in which very specific matters were at issue for the parties involved in the setting. In these situations, artifacts were thematized, not as instances of a general collection of artifacts, but in terms of their involvement in an interpretive relationship of parties to a particular display of neural evidences. Accounts of artifacts were provided as part of instructions, arguments for or against the adequacy of particular items of “data,” and projections of further work in light of a current finding.

The varieties of work done with artifact accounts in distinct situations of inquiry were only touched upon in the instances accessible to the author. From a collection of observations that were recorded in field notes, recovered from tape recordings of shop talk, identified through “practised” readings of electron micrographs by the author, and taken from instructional and written accounts, a few general rubrics were constructed for exhibiting ways in which artifacts were identified with, and by, a discovering program. These will be listed here in an increasing order of the “troublesomeness” that attended the discovery and display of the observed instances of artifacts.

In presenting this list of artifacts, I wish to emphasize that the ordering is a construction which relies upon an unusual attention to the phenomenon. This is not to say that lab members were “inattentive” to artifact, but that lab work was directed to the discovery and systematic ordering of artifact only insofar as that analysis was involved in specific situations of inquiry which were addressed to the accountability of “positive” structures. Artifacts were not collected and analyzed in lab research, but “fell out” as occasioned troubles in visibility or interpretation, and as the display of “error” or “mistakes” in the technical work of preparing a specimen. Different occasions of the production of these accounts showed some analytic regularities, though these regularities were not thematic to the aims of the lab inquiry. In this “archaeological” account, artifacts are accorded a positive status that is specifically absent in work that is designed and performed to avoid them, or in accounts which use them as grounds for discrediting the positive claims of rival researches. The ordering of artifacts into the identified rubrics listed below relied upon
the author's exterior preoccupation with lab research, and the
arrangement was possible on the basis of a relative indifference to
the lab's necessity to build accounts of significant natural-analytic
phenomena. This indifference was not a matter of not caring
about the lab's practical successes and failures, nor was it a mini-
mization of the importance of the practical injunction for the lab
to "get results." It was, rather, a possibility for apprehending
hermeneutic features of the lab's work without the necessity to
turn them into an account of the lab's practical victories. This is
to say that an "ontological" interest in artifacts as phenomena
in their own right was particularly "strange" to the concerns of
doing lab research, and marks a difference between this account
of work and the work's analytic provisions in written reports.

What follows is an ordered list of artifact accounts. The
members of the list are placed under three main headings, which
mark their different observability in lab research, their different
work in locating procedural origins for particular artifacts, and
the differential consequences of the accounts for the lab projects
in which they occurred.

"Positive artifacts"

Some artifacts were accountable in textual and situated instruc-
tions as typical and substantive examples of artifacts. These arti-
facts were located as particulars or aspects of the visibility of
ultrastructural phenomena as they appeared under instrumental-
ized conditions. In practical light microscopy textbooks the topic
of artifact is exemplified with typical cases such as bubbles on a
slide, fringes around edges of entities related to spherical and
chromatic aberration in microscopic lenses, precipitated globs of
stain, and the visibility of a student's eyelash in the field of the
microscopic scene. These common examples are usable for
instructional purposes precisely because of their commonality,
ready availability to instructed looking, particularity as "things"
in an observed field, relatively formal properties as distinguishably
non-natural features of a scene, and demonstrable genetic relation
to specific standardized instrumental practices. This is not to say
that some of these phenomena were not at one time problematic
and arguable matters in the history of microscopic research, but
that their current availability in a lab student's program is that of
demonstrably separable "subjective" matters in research-
visibility.

The typicality of these artifacts relies upon a standardization of
lab work in terms of (in microscopy) preparatory techniques for
constructing visible displays of initially unobservable phenomena.

The varieties of technical rendering practices such as staining,
sectioning, embedding, and the looking procedures with the
microscope, along with the utilization of standard equipment
across different occasions of inquiry has provided a reliable lore
of practical grounds for rendering accounts of artifacts. With the
wide adoption of instruments and practices as standard modes of
inquiry, numerous artifactual possibilities are found to accompany
the use and in some cases the design of the technical methods.

This situation of "standard" identifiability is not available for
newly fashioned techniques of inquiry, nor for what is most spec-
ifically identified with the discovering program of a particular
project. On those occasions the possibility of artifact is most
problematic, when "ambiguous," "equivocal," or "incompatible"
accounts of discovered phenomena are seen to arise in relation to
the particularity of the developed technique. It is the identifiability
of artifact with technical innovation, improvisation, and unique-
ness as featured in "cutting edge" discovery work that provides for
the common occurrence of literary controversies over announced
discoveries. A theme in such controversies often centers on
whether the discoveries can be attributed to artifactual causes
rather than to the visible regularities of independent "natural"
phenomena.

What I am calling "positive" artifacts are distinguishable from
the more problematic artifactual possibilities that arise as part of
sustained work in the doing of science. These positive artifacts can
be characterized in the following manner:
1 They have a thing-like substantiality, visibility, and availability
to accounts. As such they are described as particulars in the
revealed microscopical scene which interfere with the transitivity
of the practical access to "natural structures."
2 They are "common" and regular in their appearance as the
everyday troubles that inhabit the work of doing technical prepar-
ations of displays.
3 Once they are located as artifacts they are unproblematically
separated from the adequately constructed features of a display
format (such as electron micrograph arrangements).
4 The discovery of these artifacts initiates accounts directed not
always to their "source" in the practical work, but more import-
antly to the immediate issue of whether the material should be
used in light of their accountable presence.

A variety of substantive visibles in an electron microscopic
account of axon sprouting came under this characterization of
"positive" artifacts. Several instances of these are listed here
which arose in lab accounts of electron microscopic photographs.
The artifacts were discoverable in the midst of micrographically
They were characterized as alien constructions arising from the practical and technical rendering of brain materials. (In saying that they were “constructions,” I point to their discovered-sense as the documentary presence of “unwitting” work, not to be celebrated as an achievement, but to be attributed to the “responsibility” of the technique as an error.)

Instances:
1 “Staining artifact” – this artifact was visible as an opaque dark patch on a micrograph. It appeared to overlap the arrays of axonal and dendritic membranes and interiors, showed no membranal bordering, and revealed no depth of interior detail under stronger powers of magnification. The artifact was accountable as a precipitation of a heavy metal ion from the staining solution (sometimes, it was said, as a result of researcher having exhaled his breath in close proximity to the stain) in such a way that did not label any interior contexture of protein membranes or granules. It was accounted for as a kind of spillage which obscured the interior configuration of ultrastructural detail. It was not seen to affect the surrounding terrain, since it only prevented visible access to the area it covered.
2 “Knife marks” – these were available under analysis as linear streaks in the electron microscopic scene, or as blank spots (“holes”) in the planar display of ultrastructure. A knife mark appeared as a clear area in the texture of membranes and granules which interrupted accountable neural features, such as the labelled texture of a dendrite. As in a “stain deposit,” the blank interior of a knife mark failed to support detailed analysis. A view of an extended array of ultrastructure showed a “knife mark” as one or more parallel linear streaks which were taken as evidence that the thin section had been scratched by a nick in the knife edge of the microtome as the tissue was sliced. “Holes” were also accounted for as the outcome of an accidental formation of crystals during the embedding procedure, in which the tissue was immersed in a molten plastic and then was allowed to harden. Under some conditions crystallization was said to take place, and the embedding crystals appeared as holes in the electron dense material. “Holes” were distinguished from the clear interior of capillaries and from blank regions related to the degeneration of interior tissues, by means of the documentation of membranal borders and associated cellular structures (in the case of capillaries, pericyte cells were visible as cell types constituting the “wall” of a capillary). “Holes” showed a disjunctive relation to the accountable features of the ultrastructural “landscape,” as they interrupted continuous membranes and did not seem integral to the typified constituents of neural tissue. This is not to say that the totality of that “landscape” was constituted by accountable entity types. Indeed, at some magnifications electron microscopic renderings of brain tissue showed an extra-ordinary array of features, many of which were left unaccountably standing for research directed to the display of particular types of entities or anatomical relations. In the case of knife marks and holes, these artifacts could be recognized as distinct from other unaccountable features which took on the appearance of cell-like constituents, despite the indeterminacy of their specific character as such.
3 “Folds” – these were visibly linear arrangements similar to knife marks in appearance, which were genetically related to the folding of the extremely thin section possibly during the transfer of the thin section from the knife blade of the microtome to a copper grid used as a frame for holding the section in place for electron microscopic viewing. Other artifacts which appeared as “positive” elements of the ultrastructural displays and which were taken as artifactual in origin were “grains” attributed to the effects of the embedding procedure, “burn marks” from the electron beam’s intensive passage through a thin sheet of tissue, and various intrusions of dust particles and other forms of “foreign matter.” Although in the description of the above instances I have related each artifact to a relatively definite procedural origin (in staining, embedding, etc.), members’ accounts produced in situations where artifacts were at issue did not always, or even often, take the form of a procedurally causal account. Artifacts were often remarked upon without being related to a “source” in the actions of a preparatory or observational procedure. Instead, artifacts were often referenced with expletives which acted as indefinite complaints about the poor quality of the micrographs.
Occasionally when I asked lab workers for elaborations on their references to artifact they expressed uncertainty about the practical origins of the artifact, and at times gave brief and rather brusque accounts to the effect that they did not care especially about where the thing came from. The matter that seemed more immediate when such artifacts were made explicit was whether the materials in which the artifacts appeared (electron micrographs and other displays of “data”) were usable for the sake of a project’s corpus of data. Accordingly, artifacts were viewed as irritating impediments to the work of examining the micrographs for their display of axon terminal distributions, degenerating versus intact ultrastructural constituents, and glial cell involvements in the degenerating region. In fact, in almost all cases,
the micrographs were used despite the abundance of noticeable artifactuality (where these artifacts were of the “positive” sort – isolatable particulars in the sensibility of the display).

“Lookers” versus “users”

“Positive” artifacts were not always treated as merely incidental troubles in the construction of adequate accounts of brain events and structures. In presentational contexts where a microphotograph was to be displayed, such as in publications or public lectures, these “unproblematic” artifacts took on an entirely different consequence. This situated consequentiality of what otherwise would be treated as incidental artifacts was demonstrated in the following accounts taken from field-noted observations of (1) a light microscopist selecting a slide for photographing an instance of an astrocyte cell for a forthcoming article, and (2) an electron microscopist reviewing material under the scope for its display possibilities:

(1) When reviewing a slide preparation he had made, the microscopist remarked on the beauty of the preparation, but expressed disappointment when he saw what he called some “mung” (a vernacular expletive which in this case might have referenced some dust or stain deposit) on the slide which blemished its presentability for published photographs. I asked him whether the spot made any difference for his purposes of making comparative determinations of cellular regeneration. He said that it made no difference at all. He also said that much of what he did, such as counting glial cells in particular areas, was for the purposes of satisfying what he called “the masses,” who would find such figures important. For his purposes the tedious counts were irrelevant, and he was satisfied that astrocytes did not markedly increase in number after the lesion.

(2) A quote from an electron microscopist while at work: the account was produced for my benefit as a verbalization of what he was doing at the time.

this is beautiful. Unfortunately the field it’s in has a couple of broken membranes. See, and there’s nothing wrong with broken membranes per se, except for like I was telling you about these electron microscopists, they like everything perfect. When we go to publish this stuff, we might as well go in and publish a perfect picture. Well, you know they say, “Well, you got broken membranes there.” Well, so what . . . I mean, you can see these things are all fixed well enough, you can know that you can see nice degeneration packets and they’re opposed by post-synaptic densities and there’s nothing else those things could really be. But when you go to publish this stuff you want an absolutely perfect thing to show these people so they won’t argue with that. They really can’t find fault with this. They never could raise issue with the nature of the study on that basis, they could only . . . I dunno, it’s more a thing of reputation, I suppose. I dunno if reputation is the right word, it’s just that the quality of the work that you turn out people judge on, you know, a really odd basis so you don’t – you want to cover all – all your paths. Sometime in the future there might be an electron microscopy job that comes up, and even though that may not be your main interest, it doesn’t hurt to have a beautiful picture in – in a journal. Published where you say, ‘if you want to see some of my work, go to the Journal of Comparative Neurology in 1975.’ and they’ll look it up and they’ll go “hmm, that’s a really beautiful picture of degeneration” because there’s no way they can attack the uhm, the actual issue that the paper takes.

The documentary use of a photograph in a research article differs considerably from that of a photograph used by lab members as the material visibility of topical events and structures. As an illustration in an article the photograph stands in an exemplary relation to a textual account and serves to display “clear cases” of phenomena that are treated in the account. Illustrations of axon terminals, astrocytes, degenerating axonal material, and synapses appear in such publications not as inspectable grounds to verify or confront the claims of the article, but as instances of what the article discusses. In contrast, photographs which are “used” in lab research, provide materials which members explore, and “work with” in delimiting neural events. The documentary character of such photographs is not that of illustrating an already completed text, but is itself a “text” which is used as discriminable grounds for claims, arguments, measurement, and statistical accounting work by the parties to the lab’s researches. This is not to say that these photographs stand as a priori grounds for inductive treatment since their practically and circumstantially contingent character is precisely what is at issue in discoveries of artifacts. Those discoveries reveal the horizon of analytic and constructive features in the use of photographs as documents of real-worldly microscopic events, and provide for how the adequacy of any photograph as a record of such events is a matter established among the parties to its use.

In those instances in which photographs were presented in
research publications, however, it was not simply the case that they illustrated the naturalistic account made in the papers. They were also available as exhibits of a lab’s practical competence with electron microscopy. On an occasion when a recent publication was passed around to members of the lab, a number of remarks were made about the excellence of the microscopic work exhibited in the illustrative photographs, and at one point the lab’s research director jokingly asked the electron microscopical specialist if he could get pictures “like those.” The extreme concern with finding “perfect” pictures free of exhibits of artifact (however incidental they might be to a paper’s claims) addressed the availability of the photographs to a “practitioner’s reading” that could assess the competence of the lab’s program in the “aesthetics” of its photography.

The display of this care in providing “error-free” documentation of a lab’s technical work in its illustrative records (error-free in terms of a “state of the art” in the practices displayed) arose on occasions when the lab’s data were used for presentations to audiences outside of the lab. On an occasion when I asked for examples of electron micrographic montages to document presentations to sociological audiences, the person who selected the exhibit reviewed his collection of documents to find a “good one,” despite my reassurances that the audience would not be interested in their adequacy as records in the same sense as a biologist would.18

On occasions when photographic montages were reviewed as candidate instances of data in the lab’s corpus, assessments made by members did not concern the matter of whether the data were free of artifactual exhibits of “error” in the same way as the above instances. Artifactuality was often an occasion for complaints in shop talk when photographs were assessed while being used in the research, though the visibility of artifacts did not often lead to the rejection of the data from use in the project. Instead, discovered artifacts became matters for discussion among lab members over whether the material was nonetheless “decent” enough to use as data, as in the following instances:

1. A conversation between an electron microscopist (J) and the lab director (H) while reviewing a recently constructed electron micrographic montage:
   J: I don’t even know if you’d want to look at those, they’re so bad.
   H: They are bad. Well, they’re numbers aren’t they.
   J: They are numbers.
   H: They’re users, but they ain’t Lookers.

2. An assessment made by an electron microscopist while talking with a co-worker in the “ultrastructure” project, after the two of them had discussed the artifactual character of some micrographs that had just been developed:
   J: It just looks like shit.
   (1.1)
   J: Looks like the dog.
   (0.7)
   J: Well, I’ll paste these (mothers) together, what the hell.

In both instances a negative assessment which invoked the indefinite artifactuality of the photographs was followed by assertions to the effect that the materials were nonetheless usable. This was not always the case with laboratory records that were assessed as artifactual. In some cases materials were found to display artifacts that were too confounding or too gross in their visibility to allow their analytic use as data. Material was frequently discarded, sometimes before having been rendered to the point of being made visible in the micrographic format. “Animals” could be aborted at any of the various points along the preparatory process when the up-until-that-moment-adequacy of the material was available to assessment. For instance, sometimes for accountable or unaccountable reasons, a stain would be seen not to have “taken” (on the basis of a “hand to eye” visual inspection and judgment of the slide). In some cases when things were seen to be irreparably “messed up” (a matter available in situ) the procedure was started again with different animals or with unaffected tissues from the same animal. At other times error became available retrospectively through the detection of artifacts which sometimes were traced to definite procedural sources.19
"Distortions"

A number of regularly appearing artifacts were accountable as "distortions" of ultrastructural configurations originating in the preparatory process, or through the optical conditions of microscopic observation. These were characteristically more problematic for analysis since they did not appear as external intrusions upon the "material," but were seen to be figurally continuous with the contexture of membranes and cross-sectional visibilities of "structures." Difficulties arose in cases of systematic "distortions" when a distortion was visible as a "natural" configuration, and unless the systematic procedural origins of the distortion were available in its visibility, it would not be distinguished, and could be taken as the veridical shape of the "things."

Distortions were accountable as ultrastructural objects and arrangements which were visible in a way that "violated" their purported natural structure. In principle this is an extremely thorny issue, since the visibility provided in the electron microscope is essentially a rendering (a "distortion" of sorts) of the original availability of the naturalistic animal. Furthermore, the availability of the microscopic rendering is in some respects all that can be spoken of as visible evidences of what the brain's ultrastructure consists of. Although a congruent account of the natural-theoretic character of the brain is visible in varieties of technical ways, each technical modality, such as electron microscopy, contributes its particular analytical advantages.

How "natural structures" could be found to have been violated by means of the very technical procedures that made them visible in the first place is a very curious matter. If we think of how a two-dimensional surface - such as a geographer's map - is taken as an adequate map despite the systematic distortions residing in the account of the land surface represented in the co-ordinates of the paper, we can see that the account is made practicable through the use of an authoritative "model" of a curved surface which stands on behalf of the represented "natural" surface. By means of this double substitution (of a smoothly curved surface for the "more irregular" natural surface, and then of the flat surface representation for the curved surface representation) formulations can be produced which equate the spatial orderings on the flat surface to their "original" positions on the curved surface. If, however, as happens in some cases of microscopic observability, a single modality of representation is all that is available as access to an invisible phenomenon, its distortedness is not thrown into relief through comparison with any independent authoritative version. Often technological advances in the design of instruments or in the use of different stains, embedding substances, or entire procedures (as in freeze etching) provide grounds of comparison, though no particular account can be taken as wholly authoritative. Each account is said to have its particular advantages, freeze etching gives a three-dimensional rendering, while sectioning with a knife edge allows for a greater detailing of interior organelles.

Although it may be said that, in principle, almost anything that is available as regular anatomical form can be discovered later to be a distortion of some kind, "distortions" were accountable in the lab as particular discovered phenomena which became circumstantially relevant. Practical ways of deciding upon and acting upon "distortions" were very much a part of laboratory situations, though the general problem of how distortion could be discovered against the background of presumable natural structure was not.

A few instances of accounts of distortion will be listed here as a way of showing some situations that involved the determinacy of "distortion" on an instance-by-instance basis in lab research.

Instances

(A) "Typical" distortions: these were provided as recurrently produced artifacts which were tied to the use of a standardized preparatory technique. In this respect they were akin to the "positive" artifacts except that they did not appear as substantive "intrusions," but as modifications of interior properties in a systematic manner.
1 "Stretching" - the thin section created through the slicing of tissue on a microtome was said sometimes to result in the stretching of the section, thereby creating distortions in the shapes and dimensions of the visible configurations.
2 Optical aberrations - spherical and chromatic aberration in light microscopy were said to result in fringes appearing around the borders of observed particles. Historical accounts of microscopic discoveries refer to instances where these fringe-like appearances were taken to be constituents of micro-anatomy, and were called "globules."
3 Fused membranes - a commonly mentioned "criterion" for checking the adequacy of microscopical preparatory work was the "separation between membranes" of contiguous cellular constituents in the neuropil. If in the cross-sectional portrayal of a micrograph, a single line was visible between adjacent constituents (such as axon and dendrite protoplasm in contact along their edges) and this line was not differentiable into two discrete lines, the photograph was described as not fully adequate in its resolution of structures. Accounts of fused membranes related the phenomenon to sources in the embedding procedure, where brain tissue
was preserved from degeneration by the injection of a fluid preservative at the time of the sacrifice of an animal. A chemical breakdown of the membrane proteins relatable to a lack of penetration of the perfusate into the interior of the brain ultrastructure was said to show up in a photographic display as a fused membrane.

4 Broken membranes – in some micrographs a membranal border of an organelle was seen to be “broken” . . . where the continuous line which exhibited the presence of a membrane showed a gap or displacement. These broken membranes were attributed to the work of embedding or fixation where the chemical action of the process of preserving the tissue worked to fracture and displace a membrane.

5 “Blurriness” – a comparison of different micrographs showed a variability in the discreteness of membranal appearance. Relative blurriness of these outlines was related to a variety of procedural sources: sectioning, staining, and fixation. In the micrographic scene a darkening and blurring of texture was sometimes attributed to a section’s having been cut too thickly such that the electron beam was intersected by a depth of electron-dense material, whose variable constitution of membrane alignment at different levels of that depth resolved as a darkening and blurring. “Too-heavy” stain deposits were documented with reference to outlines which appeared “unusually” thick and blurred, presumably resulting from an “excess” of electron-dense metals having deposited on the membranes. Generalized blurriness throughout the tissue was sometimes attributed to a fixative having “insufficiently” penetrated the tissue, such that the tissue was not fully preserved and had begun to degenerate.

(B) Situated distortions: these were non-generic accounts of “distortions” which were problematically part of ongoing inquiries, and which were uniquely tied to that inquiry’s theoretic preconditions, previous discoveries, hopes, and expectations. The instances listed below did not provide for “typical examples” of repeated artifact classes, but were unique occurrences which were interesting precisely because of their lack of easy accountability in terms of repeated and standardized practices and occurrences.

1 “Holes”22 - on an occasion when the lab director met with two student researchers involved in the ultrastructure project, a visible feature of some recently produced electron microscopic montages became topical. The photographs in the montage displayed a curious phenomenon that was not previously seen in other similar electron micrographs. Parties to the discussion argued over what the object could be. One account was given that they were “holes” in the material produced by sectioning or crystallization of the embedding fluid. This was countered with suggestions that they were axon or dendrite profiles that had “swelled” artifactually, and a “black rim” around the structures was cited as support for the claim that they were axons (larger axons are identified as having a distinct myelin sheath bordering them, which appears black in the micrographs). The three parties to the argument did not reach agreement over whether the phenomena were axons, dendrites, or “holes.” A few joking accounts were offered in the midst of the argument that the “animal had been machine gunned,” and that “I think you better get a normal brain.” The discussion left off with indeterminate accounts in which the parties asserted that they were, “in the dark,” and that “you look at things like that and you got to wonder.” The material was subsequently left aside and was not included in the corpus of montages which were analyzed in the project.

The “holes” were not clearly accountable as visible phenomena; their presence was not definitely attributed to either the “structures” of the brain or to the procedures for rendering the material available for analysis. The explanations given in the lab members’ talk with one another (that the “holes” were axons or dendrites that had swelled, or that they were indeed, “holes” in the section) did not mention the possibility that they could be natural structures peculiar to the animal, or that some fortuitous procedural advent had displayed a discoverable constituent of the brain; however, jokes were produced which suggested that the animal was “weird” in some way.23 Parties to the accounts tried possibilities for rendering the holes accountable as typical conformations (however atypically rendered in the photographs), and though they reached no agreement on any of the particular accounts, the question was suspended in lieu of further analysis, which to my knowledge was indefinitely postponed. Practically speaking, the phenomenon was treated as an artifact, though no definite account of its status as such was produced and accepted by the parties to the scene.

2 The microglia in a capillary and the theoretic preconditions of a “distortion” (quoted from fieldnotes):

In Montage 2(1)4 there appears to be a microglia cell within a capillary. I asked Al (not actual name) about it and he showed me a closeup of it. He said it may have been displaced in the fixation and thereby present in the capillary. He said there are cells which enter the brain from the blood – George walked in and asked, “Which ones?” Al didn’t answer by means of giving a specific name, but held that there have been reports of such things, to which George assented.
I then mentioned that some people have claimed that microglia enter from the blood, upon which Al answered abruptly that [name of the lab director] proved that was wrong— and walked into the adjoining room. (I think he may have been referring to the radiography experiment in which thiamadine was found within the brain tissue indicating microglial division).

Three separate pictures were taken of the same glial cell from at least two different sections—the cell as a whole must have been displaced, not just a slice of it; the thing was obviously paid a great deal of attention, as the dismissal was hardly offhand.

In my inspection of the photographs, the “microglia” is detached from the membranal wall of the capillary and there’s some crap seemingly attached to its outer membrane... which might lend plausibility to the claim that it was detached during a preparatory method, floated free during staining and settled in the capillary... or some such explanation. The fixation could have cracked the membrane and left the rest of the cell intact.

The “microglia” within the capillary was an especially interesting case of the appearance of an artifact (in calling it an “artifact” here, I do so only in terms of its availability in Al’s account above. It is not necessarily an artifact in alternative accounts of the photograph). The phenomenon clearly appeared within the borders of the capillary, was surrounded by an intact membrane, showed an elaborate interior structure, and was identifiable as an instance of the cell type, microglia. (Microglia were somewhat controversial phenomena in the brain science literature, as some accounts claimed they were anatomically indistinct from an other glial cell type, oligodendrocytes. In any event, the cell was recognized as the glial cell type which was involved in the system of designatable ultrastructural objects.) In short, nothing about the initial appearance of the object, in terms of its sheer visibility as an entity in the ultrastructural scene, was particularly troublesome. The issue that was involved here, however, was that according to the lab’s account of the structural possibilities of the region, “microglia” were a cell type endogenous to the brain, and did not enter the brain via the blood stream. This was a controversial issue in brain science, as it was alternatively claimed that microglia were akin to leucocytes (white blood cells), and were transported to injured regions of the body as part of the recovery process. Microglia were featured in the lab’s account of axon sprouting, and their involvement was a key link in a chain of hypothetical events which promised to yield significant discoveries. It could be said, accordingly, that the lab members had a “stake” in their account of the microglia in the capillary, though it could also be said, and indeed was said to me when I raised the issue, that numerous experiments and observations in the lab had demonstrated to the members of the lab that microglia were endogenous to the brain tissue, and multiplied therein rather than being transported by the blood stream.

The specific account of how the microglia may have been artificially “dislodged” so that it appeared within the capillary seemed rather flimsy in this instance, though the strong claim for the artifactuality rested not on the visibility of the instance per se but on its presence against the background of an accepted demonstration of the implausibility of the visible instance (that is, the implausibility of the thing as a veridical account of any “real-worldly” arrangement). The “rarity” of the picture was not an adequate demonstration of its impossibility (though, surely, repeated instances of such photographs would have been notable), as one would not expect a relatively rare cellular constituent to be “caught” very often within the plane of a microscopic section while passing from the bloodstream to the brain interior. Its “implausibility” was not an empirical impossibility; instead, it was an isolated case in the face of a more strongly demonstrated alternative account, whose local availability to members of the lab was overwhelmingly part of the apprehension of the microglia as an artifact. The theoretic preconditions which led to the micro-
close-up photographs were made of serially sliced sections, allowed for the inspection of the phenomenon in much greater detail than might otherwise have occurred. In being caught up in such an inspection, I did eventually discover some documentary visibility for the interpretation of the phenomenon as an artifact (the "crap" around the edges), though it is unlikely that it otherwise would have been noticed.

The problematic character of the microglia photograph was not only related to the fact that it contradicted the lab's account of how microglia arose within the brain; it was compounded by the availability of a competing account that was seemingly documented by the photograph. The microglia cell appeared in a way that was not only unaccountable in the lab's version of brain physiology; it could be cited as "evidence" for another account available in the brain science literature. The above account by the lab's electron microscopy specialist transformed the status of the photographic document from being "evidence" for the competing account to being corroborative of the axon sprouting account. In that account the "placement" of the microglia in the capillary became an unwittingly constructed juxtaposition rather than being an arrested "image" of a real-worldly natural environment.

The charge of "shrinkage" was addressed in the ultrastructure project, and was topically treated in the project's write-up. Electron microscopic demonstrations were constructed to show (1) a zone of overlapping intact and degenerating axons which appeared along the previously discrete juncture between the commissural-associational and entorhinal layers, and (2) measurements in microns transposed onto micrographic displays which showed "expansions" in the entorhinal layer after the onset of "sprouting" which were more than a matter of relative differences between the two adjacent axon layers (that is, the measurements gave an account of both expansion and shrinkage in the adjacent layers as separately detectable matters).

Although the charge of shrinkage was addressed in the above ways, it is notable that the charge located a visible account of a process of expansion as being, instead, an artifactual "distortion" brought about through the lesion assay. Regardless of the ultimate acceptability of the account of "shrinkage" in the brain science literature, it for a time presented a rival version of the display of axon sprouting which was addressed in further studies of the phenomenon. The account of "shrinkage" did not suggest that lab members had been negligent, sloppy, or erroneous in their preparation of specimens such as to engender shrinkage in the entorhinal region. The lesion which was considered to be practically responsible for the observed "shrinkage" was a different induced injury, the visible consequences were relied upon as veridical events in a brain's natural regenerative processes. Although the "shrinkage" was engendered by the experimentally induced injury, the visible consequences were relied upon as veridical events in a brain's natural regenerative processes. "Shrinkage" was available in the account as a natural effect of the degeneration of brain tissues, not as an illusory representation of such natural processes.

A notion of "distortion" came into play in the account in relation to the claims made by the axon sprouting studies based on readings of the adequately "natural" representation of microscopic displays of "expansion." The distortion was seen to arise as a kind of "gestalt switch" in the reading of the sequential relativities in width of the adjacent axon layers, where a
"shrinkage" of one layer provided the possibility that the adjacent layer would appear "expanded." In the absence of well fixed standards of quantified and invariant axon layer widths (e.g. commissural-associational layer = 75 microns before expansion, 85 microns post expansion), the gestalt properties of the relative differences were arguable. Considerable work went into subsequent attempts to establish numerical and invariant measures, such as selecting "standard points" along the anatomical course of the adjacent layers to place a linear measure, controlling for magnificational powers and effects of sectioning on widths, fixing a "zero" point for a linear scale at an anatomically "reliable" place in the pre-measured cellular terrain, and developing breeds of rats with standardized brain dimensions in their ultrastructural anatomies. The adequacy of such measurements was contestable on the basis of all the above contingencies for the pre-availability of a standardized terrain that would support the reliability of "microns" as a measure, since the work of measuring linear distances in ultrastructural anatomies was far more problematic than, for example, placing a scale along the diameter of a macroscopic object (though in either case the practical contingencies of doing the measurement are part and parcel of its accountability and accuracy).

As an account of an artifact, the "shrinkage" account implied a condition of difference between the adequate visibility of a display of the dentate gyrus at different "time points" and an equivocal interpretation of just what the display showed. The display, when considered from the standpoint of the "shrinkage" account, became an illusion when it was first separated from and then paired with the axon sprouting account. The shrinkage account made a case for how the display of a discovery, axon sprouting, was a display of a thoroughly "mundane" and "no news" phenomenon. The "illusory" character of the display for the shrinkage account did not reside so much in the fact that shrinkage was artificially engendered through a lesion as it did in the ambiguity of what could be discovered as the brain events documented. The artifactuality was not "substantive" in the sense of being a "positive," object-like addition to the adequately visible micro-anatomy, since the "display itself" was not in question. In another sense, however, the materiality of the display was at issue, since the axon sprouting account showed a material expansion in a way that was beyond question for lab members, but which was contested in the shrinkage account with an alternative "material" argument.

The "artifact" in this case was an essentially problematic matter. It was recognizable only in terms of a particular "line" on the controversy over axon sprouting, and was a matter at issue in the way that the phenomenon of axon sprouting was at issue. Indeed, the presence of the shrinkage account was a way of providing for the at issue character of axon sprouting. Regardless of how the controversy will eventually be resolved (if it ever will), the "artifact" constituted in the shrinkage account was temporally and circumstantially part of that account's relation to the axon sprouting demonstration. In this and in other cases of artifact accounts the "artifact" was relative to a claim or argument as the manner in which the claim gave itself a substantive "neurological" visibility. As such, it was not a "thing" so much as a "thing" within a contending account in an argument (perhaps it should be called an "anti-thing" given the prevalence of artifact accounts which seek to establish the "not that, but this" character of a phenomenon).

The cases presented thus far show a rough progression from those artifacts which were most substantively and typically accounted for to those which were more circumstantial and problematic in their accountability, and thereby less "positive" in their visibility.

The more typical and "positive" artifacts were found to be troublesome for presentations of laboratory displays and were provided for by members as undesirables in the microscopic environment, but they did not have the radically threatening character for the inquiry that the more circumstantially based and particular-account-dependent artifacts had for the lab's discovering enterprise.

The following sections of this chapter will present cases where artifacts were at issue for inquiries, but in ways that were not tied to any definite presence that was accountable as a technical "factor" in the visibility of naturalistic data. Instead, cases will be examined where artifacts were retrospectively discovered or prospectively imagined as (1) ineffectual procedural "superstitions" and (2) possible inhibitors of a yet-to-be-established positive result. This consideration of such "negative" artifacts will lead us to a discussion of how the practical circumstances of an inquiry were present in an indeterminate depth of generative detail for the rendering of a "natural" microscopic scene.

"Negative artifacts"

The following section will discuss artifacts which were accountable not as intrusions, distortions, or particular defects in an observed field, but as absences of results or effects. "Positive" artifacts and distortions were attributed by members to the troublesome effects
of a procedure; what I am calling “negative” artifacts were tied to the absence of expected results and effects of procedures. The former cases were discussed as procedural excesses; the latter were available as procedural inefficacies.

In cases of “negative” artifacts the lack of a positive result for an experiment or observational procedure implicated the adequacy of laboratory work along with an environment of contingencies which were deemed responsible for that result, though in ways that were essentially indeterminate and awaited further work of specification. Two general rubrics that appeared in lab members’ accounts of their practices and which alluded to “negative” artifacts were “superstitions” and “failures.” “Superstition” formulates a discovery of the inefficacious character of particular instances of laboratory work along with an environment of contingencies which were deemed responsible for that result, though in ways that were essentially indeterminate and awaited further work of specification. It was a vernacular characterization by lab members in their accounts of how the stains were troublesome in their practical application.27 When possible, rationales were preferred, since they provided insight into the adequate design of procedures and increased the likelihood of performing standardized and replicable procedures. Nonetheless, “superstitions” were seen as an inevitable part of any particular attempt to embody the performance of these procedures in actual sequences of shop work.

A separate, though related, matter was made available in accounts of “superstitions” having to do with retrospective discoveries of features of instruments and procedures which could be eliminated with no discernible effect on the results. In such cases, the discovered features were attributed to superstitions, insofar as their inclusion as technical constituents was seen to derive from an unprincipled rationalization of performance. Certain inefficacious procedural elements were retrospectively accounted for as having been used because at one time they were associated with positive results. For instance, a “chamber” apparatus that was used in the lab to house viable brain tissue “slabs” in their in vitro condition for periods of up to twenty-four hours was redesigned in the lab to be much “simpler” than a

Superstitions

Members of the lab employed a vernacular term, “superstitions,” as a way of referring to features of shop work that were not “rationally” accounted for in terms of accepted scientific principles. For instance, many of the procedures used in histology (the rendering of tissues preparatory to microscopic observation) employed chemical solutions, latency periods, temperature variables, and sequential operations that were not rationalized with available biochemical principles on the interactions of the specific chemicals and tissues, but which were employed because they “worked.” The prevalence of these “superstitious” elements of histology provided for the construction of tasks in shop work as a kind of empirical craft; sequences of work were referred for their adequacy and their detailed make-up to conditions of practical efficacy rather than to formal principles of why the procedures worked. An account by a lab member on the topic of “superstitions” in shop work, reported in the author’s fieldnotes, follows below:

Joan [not actual name] told me of histological “superstitions”
similar apparatus that was previously used in other labs. The simply designed mechanism was said to possess all of the advantages of the former model, without eliminating any necessary functions of the apparatus. The earlier model was then described as having had “superstitious” features of design which had no practical value. They had been included in the earlier design presumably because their association with necessary features of the apparatus provided the illusion of their efficacy. Analysis of the apparatus into discrete features and the elimination or redesign of some of those features were responsible for such discovered “superstitions” in the shop work.

In accounts which attributed a discovered feature to be “superstitious” in its procedural origins, the demonstrated inefficacy of the feature exhibited a reliance on a kind of “technical intentional-ity” for shop work and shop apparatus. That is, for any feature of the work or its instruments a “tie” between the feature and the enhancement of a result was critical. The discovery of “superstitions” showed that such a “tie” was not always to be found for isolated procedural elements, though its absence was attributed to “error,” “oversight,” “poor design,” or to the contingencies of getting a result.

“Superstitions” were not, of course, as consequential as “failures” for getting a result, since they were seen to be extra features of technical design which were associated with positive results that “really” obtained for other reasons. They did not accountably affect the practical outcomes of procedures other than by requiring unnecessary work, and/or by confusing the rationalization of results. They were said to be consequential in the sense that practitioners managed the contingencies of their work in ways that were not always fully rationalized. In being discovered to be “superstitious” in their origins, inefficacious procedures were dismissed from project-relevance, or subjected to repairs directed to achieving parsimony.

Superstitions were accountable as procedures which were “defective” not because they biased the results of lab projects, but because they were found to have no involvement in the results whatsoever. These accounts of “superstitions” show that procedures were relied upon to be essentially bound to what they made visible, as a condition of their “effectiveness.” Otherwise they were treated as wasted actions, which were not accountable as results, and had no “objective.” The effectiveness of actions was assessed as a matter tied in with a consequential visibility associated with the revelatory power of technical modes of perception. Consequently, any visible field whose availability was secured instrumentally was implicated as a practical accomplishment. The

discussion which follows uses “failures” in lab projects as an occasion to suggest that the accomplishment of perceived visibilities in lab work was available in that work as an active “making-happen” with an examinability independent from the objective or positive results of that work. The achieved-visibility of results will then be shown to be inseparable from the possibility of either “positive” or “negative” artifactuality in lab work.

Inconclusive “failures”

Subsequent to my observations of the “axon sprouting” inquiry I witnessed a project which was eventually abandoned as a “failure” after many months of intensive work on the part of the lab director and some of his associates. Since I did not have an intimate acquaintance with that project, I can recount little of the technical detail of the project’s accomplishment. As I understood it, the project was an attempt to construct an experimental demonstration of a neurophysiological effect of a diffusable hormone on the firing pattern of a specified system of hippocampal neurons. On the basis of prior results and conjectures it was anticipated that the hormone would result in a dramatic increase in the firing amplitude and frequency of the isolated cells, and that this change in the firing pattern would not “decay” over time, unless further chemical introductions were made to counteract the effect of the first hormone. The experimental demonstration of such an effect would have supported arguments on behalf of a biochemical and anatomical sequence of events constitutive of a memory “engram.”

I observed the experimental project late in its course, and was told that the project had originated after a lab assistant observed an “effect” in the firing patterns of electrophysiologically monitored neurons in an excised slab of brain tissue when a specific hormone had been introduced into the media in which the slab was “bathed.” The initial observation of the result occurred almost by happenstance, as the student had been investigating other matters at the time. However, once the effect had been repeated a few times by the same student, the lab director set about building a strongly defensible version of the experiment as a demonstration of what promised to be an immensely significant phenomenon.

Later attempts to obtain the “effect” under a controlled experimental procedure (the original circumstances of the observation were said not adequately to be controlled) failed despite an exhaustive attempt by the lab director to devote his time and facilities to the project. I witnessed one such attempt to replicate the original result by the lab director and two associates. A slab
of brain tissue was kept viable in a chamber apparatus which supplied nutrient media and oxygen to the tissue. An electrode was implanted in one of the hippocampal cell layers, and its activity was monitored on graph paper by a pen hooked up to the electrophysiological apparatus. The pen traced a curve on the paper which graphically rendered the frequency and amplitude of the neural activity. With each repeated monitoring of the cell a relatively horizontal trace could be seen on the paper representing an "unchanged" firing pattern. The hormone was introduced into the media of the tissue, and the parties to the experiment oriented to the graphic display for several minutes thereafter. Each successive reading of the cell showed an "unchanged" pattern, and after a few minutes the parties began complaining about the apparent failure they were witnessing, and speculating over what environmental or technical circumstances could have possibly inhibited the appearance of the result. At one point one of the party grabbed the pen and manually drew a marked "jump" in the pattern of the trace on the graph. This evoked some laughter, as the trace represented the anticipated positive result which was relentlessly absent with each repeated monitoring of the oscillograph apparatus.

This was but one of a long series of unsuccessful attempts to replicate the initial result. What is most interesting about the series of failures for our purposes is that repetitions of the experiment were continued for quite a long time, despite the consistent failures. One could say that the project was sustained as a matter of "belief" in the possibility of a positive outcome, though this would be a massive oversimplification.

When I asked the lab director why he sustained the project in the face of the repeated negative results (which one might presume would falsify the original "discovery" by the lab assistant), he answered that negative results were indeterminate in their import on the plausibility of the project's intended result. Although the negative results stood as failures to corroborate the initial findings of a biochemical-electrophysiological effect, they did not necessarily stand as definitive evidence against the possibility of such an effect. This possibility was kept alive on the basis of the practical circumstantiality of the negative results, because in any experimental demonstration in which no effect obtained, the documentary "evidence" could be viewed as the consequence of an as-yet undetected artifact or uncontrolled contingency of the experimental design.28

Each successive replication of the experiment was not, strictly speaking, a "replication." Instead, circumstantial "variables" were explicitly modified as part of the design of new "controls" each time the experiment was re-performed. Furthermore, the practical accomplishment of each experimental attempt was itself seen to compound an unspecified array of uncontrolled contingencies that inhibited each unique attempt to do the experiment. These "variables" did not exist as a finite or exhaustible list of factors; instead, with each unsuccessful attempt to vary a condition (temperature, oxygen content of the media, concentration of the experimental compound, features of the oscillographic instrument, lighting of the room, etc.), with some effort a "next" possible condition for "failure" could be imagined and tested. Under any stable set of controls it was imaginable that, for example, the oscillograph needle was clogged this time, or some unnoticed error had been committed in the practical operation of an otherwise adequate experimental design. The course of the inquiry took on the character of a search for contingencies that may have operated in the original "positive" observation, but which were not operating in the unsuccessful experiments.

When the attempt was finally abandoned it was not because a list of possible negative influences had been exhausted. Instead, the work was set aside as inconclusive and too "expensive" to pursue further due to the lab's imperative to produce results. Other "lines" of research that were associated with a more immediate possibility for definite success were pursued in lieu of the abandoned project. This is not to say that members did not imagine that further exploration of as yet untested or unthought of circumstantial factors would not have eventually saved the project from failure. The "failure" was formulated by members as a depressing waste of time, where the project was said to have grown too tiresome to continue further despite the significance of its possible results. Ending the project was thus an "existential" decision rather than a capitulation to a definitive disproof.

Making it work

Exercises for students in natural science lab courses are commonly specified in clear-cut ways which leave no question of the availability of a correct answer to any given problem. At the end of an exercise students are often given a "correct answer" with which to check their results, and the degree of correspondence between the student's results and the answer is used in assessing the student's work. Characteristic of such an exercise is that students experience great difficulties in making it work in the way specified in the exercise manual. Anyone who has spent time as an introductory lab student can recall the innumerable ways in which experiments have gone awry, sometimes for no discernible reason. Often
students discover that their answers are quite different from the
correct answer, though the manual and the lab assistant can offer no
explanation as to why the error occurred. In such cases, students
are left with no recourse but to “try again” in hopes that the
experiment will “work” the next time. It is reasonable to assume, within a subscription to the authority
of the experiment’s legitimate formulation, that the ignorance,
carelessness, and/or accidents of students are responsible for such
instances when the experiment fails to “work.” As has been noted
in critiques of the doctrine of falsificationism, the lack of an
adequate replication of the experimental result in students’ work
does not provide an occasion for considering their results as
adequate discoveries which contradict the authority of the formulated “answer.”

In discovery-oriented research, making it work is a far more
fragile process. In such situations the answer is not established in
advance as the standard for checking the accuracy of a result.
Furthermore, one may not be assured that any answer is available,
insofar as the experiment may be ill-conceived and a solution not
possible. Such was the case for the failed experiment discussed
above, where despite an expectation that the experiment would
work, there was no authoritative assurance that it would. When
the experiment failed to work, the question remained: “Did we
do it correctly? Is there anything we could have done that would
have made it work?” Such questions arise in the absence of a
possible authoritative resolution in reference to a standard; they
are resolved in a way that is reflexive to the work itself.

The circumstance of repeated failure brought a host of circum-
stantial “contingencies” into relief for members as they sought to
detect possible inhibitory factors. Each possible inhibitory factor
was discerned as figural against a background of environmental
and practical contingencies. This set of contingencies appeared
indefinite in the sense that it “yielded” to analyses in such a way
as to offer one after another possibility with each attempt to
discern a crucial inhibitory “factor.” This is not to say that every-
thing that imagination could conjure up as a circumstantial matter
was seriously entertained as a factor to investigate further.
Numerous “absurd” possibilities such as practitioners’ clothing
styles, astrological complexes, what members ate for dinner that
day, the color of the laboratory walls, etc. were never seriously
raised. However, some rationale could be given for a wide range
of possibilities each of which faced members with the task of
deciding whether it was worth their efforts systematically to
explore the possibility as an experimental factor.

The vernacular formulation of “making it work” suggests a
contingency of results upon “skilled production.” Performing
successful research in the context of a complicated array of instru-
mental and organic contingencies is no easy matter, and even the
most skilled technicians relegate a considerable number of their
attempts to do experimental and observational work to “failure.”
The fallibility of procedures is such that “failure” carries variable
moral connotations depending upon locally invoked circum-
stances, and in some cases can be attributionally located in a more
or less “objective” complex of circumstances. That is to say, the
work proceeds in circumstances that are seen as, at times, capri-
cious and beyond control, though it is the member’s task to
construct a rationale for actions in terms of “effects” contingent
upon such actions. Consequently, making it work, entails a selec-
tion of those “effects” that can be traced to a “rational” set of
contingencies, and a discarding of “attempts” that are found to
fall short of such “effects.” Making it work is thereby a kind of
skilled work, but one that is never fully under control.

Failure and error
I have spoken of an attributional tie between artifact and “error”
in lab members’ accounts. Visible artifacts are given their origins
via genealogies of “errors” in procedures, “misinterpretations,”
and “illusions,” with varying degrees of definiteness and explicit-
ness. In many cases, however, the appearance of artifact, or
possible artifact, is baffling to members, and not accounted for in
terms of a practitioner’s remembrance of any “mistakes” made
while preparing and working with the “materials.” A procedure
may seem to go perfectly well until an examination of results
shows that a “mistake” may have been made. Accounts of nega-
tive results take on a variety of appearances which are only sugges-
ted in a sketchy manner here. As a way of addressing the topic
of such accounts, I will present two contrasting ways in which
negative results can appear in the course of shop work: “oops,”
and “what went wrong?”

“Oops” is used here as a way of alluding to the concurrence of
an attribution of a mistake with its appearance in the work. “What
went wrong?” formulates an inquiry which seeks to locate a prior
occurrence of “error” in the work on the basis of its subsequent
realization in a documentary display. “Oops” is exemplified in
cases where, for example, a lab worker preparing a solution for
perfusing an animal knocks over the vial of liquid and spills it.
The recognition of the problem is simultaneous-with or follows immediately upon its occurrence. Another example can be given from observations made of electron microscopists' practices. While observing a slide under an electron microscope, a lab member suddenly noticed that the array he was witnessing seemed to melt. When asked what was happening, he replied that the electron beam was burning through the slide section—the section had been left under the beam for too long a time without being shifted.

Instances of “oops” had the feature that the trouble interrupted the ongoing procedure. Sometimes the problem was remedied. An instance of “oops” occurred during a lesion operation on a rat. During the operation a sub-cranial artery was ruptured as the skull was drilled and an opening cut for insertion of the electrode (used for burning out a selected area of the brain by electrical means). Blood gushed out of the artery, filling the incision in the scalp and making further operations in the procedure impossible. The practitioner began showing a great deal of strain, sweated profusely, and worked feverishly to stop the flow of blood before the animal died. He used a coagulant powder and cotton swabs that were on hand (in anticipation of just such an occurrence) and succeeded in clearing the blood sufficiently to proceed with the lesion.

In cases of “oops,” although the “accident” may have been not fully under the control of the practitioner, it was nonetheless taken to be the practitioner’s responsibility (in ways that varied considerably in different cases according to circumstances). Regardless of how responsibility was attributed, however, “oops” showed a relative simultaneity of the appearance of trouble with its discovered origin in the work.

In cases of “what went wrong?” accounts of displayed visibilities, such as electron microscopic montages, attributed error or trouble to antecedent work situations. On the basis of the appearance of electron micrographs, those antecedent situations were glossed in accounts of perfusion, staining, and embedding; in brief “preparing the specimen.” The inquiries into “what went wrong?” did not always achieve their objective of discovering a definite error “source,” as attention was focused on a local and practical history of events circumstantial to the present display.

Many of the artifact accounts mentioned previously in this chapter had the character of “what went wrong?” inquiries. In those cases a documentary display of artifact was addressed in terms of the antecedent conditions of its appearance. Not only were “final documents” incorporated into such an inquiry, but many transitive conditions of the animal during the rendering procedures were also available as the appearance of trouble. For instance, during one study the corpses of a cohort of animals occasioned an inquiry into the “unscheduled” conditions of their demises. The animals had been lesioned and subjected to radiographic treatment, and were scheduled for sacrifice a number of days thereafter. They were discovered in advance of that date as having departed somewhat early in their prospective careers as specimens. The corpses in the cage were seen as exhibits of a prior absence in the sequence of treatments to which the animals had been subjected. That absence was remedially addressed in the next attempt to perform the radiography experiment by inoculating the lesioned animals with penicillin. It was reasoned that the radiation treatment had weakened the animals’ resistance to infection at the site of the lesions. (Incidentally, the account that a precautionary “step” had not been taken in the rendering of the animals was a curious form of “error” in that only after the corpses had documented the antecedent absence of a procedure did its necessity occur. It cannot be said that prior to the documentation that the practitioner had “made an error” since in this case the necessity of the precautionary procedure was not visible at that time.)

A matter that accounts of “error” make available is that although practitioners are variably held accountable as authors or agents of “errors” in the work, error is nonetheless attributed to an independent “environment” as well. Environmental contingencies of an experiment are seen to operate in their own ways and to resist efforts fully to control their influence on results. The contingencies exhibit themselves all along the course of experimental projects as troubles and “absences” in the regular ways of making the experiment work. These contingencies appear at times to be “invisibles” discovered in “archeological” readings which are retrospectively made from the text provided by a microphotograph or other record of data.

We can contrast “oops” with the “what went wrong?” character of the retrospective recognition of non-results. Take, for instance, “the stain didn’t take,” as a particular expression of a trouble. In such a case the procedure was sometimes attributed a kind of responsibility for the “error” aside from the particular member’s actions in accomplishing the procedure (as in “Golgi staining is capricious”). Yet, when such “errors” happen they are viewed as “subjective” factors which inhibit the in-itself character of the object from being shown through the procedure. The practitioner is not in a position of “control” at all times, in the sense of being able to prevent such errors from occurring (and not as a matter of any inevitability of human imperfection). They happen, and
the only control that is possible is a retrospective control: recognizing when something “wrong” has occurred previously, and taking account of it in further actions (letting it be, taking remedial measures, throwing it out). This circumstance allows for the possibility that “errors” can go unrecognized. They are in the world until discovered as a negative horizon of that world, and their discovery is as problematic as is the discovery of any “positive” phenomenon.

In practitioners’ accounts, procedures “themselves” are said to be fallible. This differs from a consideration of the fallibility of any performance of the procedure, where “errors,” “incompetence,” “mistakes,” etc., happen as a matter of personal accountability. Procedures are in some ways encountered as social acquisitions, i.e. as courses of shop work that are passed on without an exhaustive rationalization of exactly how they work in all of their aspects. Many procedures are found to be less than desirable ways of doing things, but as “the best that can be done for the time being.” The same can be said for instruments. They have their flaws, imperfections, and designed incapacities, and these troubles with them are not attributable to practitioners who use them but are limitations associated with the instrument itself (although there are the many stories of amazing accomplishments of scientists with improvised, “primitive,” and inadequately fashioned equipment). These limitations are not “absolute,” but are, for some purposes, available for ascribing fault. A “thing” can have attributed to it the “responsibility” for the “mistakes” of an inquiry, as in “it didn’t work.”

Despite the projectability of error into the instrumental complex (or in some cases, the deceptive character of “the world” observed), the management of the inquiry is held responsible for the errors that are discovered in its research, and the charge that a researcher was unwittingly deceived or deluded carries the seriousness of any charge of technical incompetence in the performance of the work.

Conclusion

The above discussion of “negative artifact” and of the conditions for the attribution of “error” in shop work provided for the work of objective disclosure as more than a matter of “keeping out of the way” of the adequate manifestation of natural phenomena. Instead, for lab members, making a phenomenon happen in lab work was more of an active seeking for the thing or result. Positive artifacts, discussed above in the way they were accountable as substantive intrusions in the face of the visibility of natural phenomena, give us a sense of the work as a passive opening upon the presence of the thing, or as an avoidance of subjective error. Negative artifacts, insofar as they show the work to be a seeking of an elusive object, attest to the work of finding; it is not enough to avoid mistakes, since success requires a management of circumstances so as to bring out an intended result.

Negative artifacts are available as nothings or absences; work which fails to get the thing to appear. They exist as possibilities for “hiding” the thing, though this hiddenness is in some cases essentially problematic in the sense that there is no assurance as to whether anything has been hidden until it makes its first appearance. The hidden object exists as a possibility, as does the artifact which hides its availability, until an as yet untried technical modification documents its presence. Negative artifactuality formulates the conditions for starting over again to access the thing. In contrast, I have spoken of positive artifacts as cases where an analytic visibility is secured to some extent, and where its characteristics are obscured. With a negative artifact, the very possibility of the thing, as a thing, is at issue.

Although no systematic attempt will be made to present an epistemology of the laboratory inquiries which were observed, I suggest that coherent modes of inquiry (seeking a result, “unmotivated” searching an environment of phenomena for discoverable possibilities, strengthening the documentary evidence for a locally established phenomenon, and “checking out” a possibility) exist as circumstantial matters with one or another sort of inquiry emerging in the course of different phases of a project in reaction to specific discursive challenges in a literary or non-literary dialogue and in relation to current results. Accordingly, the different “kinds” of artifact attest to variations in the work’s accomplishment not as a coherently structured inquiry, but as a scenario of available practices.

In addressing artifactuality in this chapter, I have limited my analysis to those cases that arose within lab work (in some cases as typical sorts of artifacts) and which were accountable to members as artifacts. In a previous study, I analyzed the presentational format of electron micrographic montages, and elaborated upon the specific form of visibility that the photographs concretized. Such matters as their two-dimensionality, their magnification variability, and their temporal arrangement as records of events were discussed as artifactual parameters which inhabited the visibility of “natural phenomena” in microscopy to an unknown depth of detail. The presentational modalities were shown to provide the spatio-temporal horizon of the “things” seen in the photographs; not only as the inerutable structures and
processes in a represented biological realm, but as the practical historicity of those phenomena in shop work. In this way any visibility of an initially "invisible" natural phenomenon was detectable as indeterminately constructive. For instance, the phenomenon of "labelling" (a members' term for staining procedures) alludes to how a stain labels a phenomenon in almost a linguistic sense by rendering an initial transparency of tissue into a high-lighted configuration of visibles through selective chemical affinities of the material with the staining compound. "Structure" thereby becomes available as a uniformity of chemical bonds between a staining compound and a "natural" array of membranal structures. A different chemical brings forth a different configuration, the selection of a stain being thereby an analytical matter. The incompletely elaborated chemical properties of a stain and the as yet undiscovered possibilities of chemical structure in the cellular material were seen to create an open indeterminacy of possible appearances.

These characteristics of an instrumental modality were related to the genesis of what practitioners called "artifacts" as practical contingencies in the production of visibility, which at times are indistinguishable from the "things themselves." It was noted that scientists commonly "check" the discoveries of one instrumental complex by employing different forms of analysis (instruments, stains, preparatory techniques) as independent modes of access to the "identical" field. It was noted further that in some sense, despite these "checks," each instrumental modality had its particular irreplaceability which on one hand provided for its unique access to a visible order, and which on the other hand provided conditions for doubting the independence of those uniquely available "structures."

The analysis of presentational modalities, however, obscured a key matter that, for members, was at the heart of the practical consequentiality of artifact. Although it can reasonably be asserted that the entire array of visible and describable features of, for instance, an electron micrograph, is artifactual in the sense that availability of these features is essentially tied to a course of work which appears in-and-as the concrete photograph and its discernible detail, this entirely misses what is most critical in the accountability of such a photograph for laboratory analyses. In lab work, the essential artifactual possibilities exhibited in the constructive horizons of a photograph are incidental and uninteresting, except insofar as they provide circumstantial "troubles" for any ongoing project. Artifacts became present in lab researchers' accounts in ways that were inseparable from the particular phenomena studied, the discursive circumstances of any analysis, and the temporal context in which the "materials" were used in laboratory projects. Stable parameters of the observability of microscopic phenomena were relied upon in that work and were not themselves at issue in the use of the data, nor did members have occasion to elaborate upon what, if anything, such parameters might be. Instead, mentions of artifact occurred in ways that involved the practical adequacy of the materials, not as materials freed from possible contingency or essential ambiguity, but as "usable" or "well done." There was no attempt to "clean" materials from all "contamination" of artifactuality in general, as such an enterprise would have been absurd and would have returned the inquiry to the detached contemplation of a living and macroscopic animal. The "animal" as it existed in the lab was thoroughly artifactual in one sense, and yet its consequentiality as "natural" or "artificial" was relative to specific inquiries. Accordingly, in this chapter I have provided varieties of accounts of artifacts that brought into relief some characteristic features of specific inquiries, and I have not attempted to analytically extend the sense of "artifact" to include the detectably constructive horizons of visible formats.

The discussion of artifacts - whether of the "positive" or "negative" kind - came down to a variety of "essential" indeterminacies. Positive results in lab work were said to be secured relative to the omnirelevant possibility of their being recast into artifacts. Negative results in the cases mentioned were shown to occasion uncertain further efforts to bring forth a possibly "hidden" phenomenon. We have seen how a reliance upon instrumental modes of discovery provided conditions of indeterminateness for achievements with those instruments in the reflexivity of the discoveries upon the practical circumstances of their observability.

These indeterminacies, however, are indeterminate only in this sense: no rule-like guidelines are definitive for, for instance, conducting a search for possible artifactual contingencies of a positive or negative result (or to put it differently, for bringing an inquiry to a close). This is not to say that determinations are not accomplished with massive regularity in scientific work, nor that inquiries are never brought to a close. Quite obviously, they are. However, our concern no longer can be with any general formulation or criterion of how artifacts are described in science in contrast to their non-artifactual grounds. An alternative procedure is to observe how determinations over, for example, "What do we have here?", "Should we use this material, given its specific troubles?", "What should we do next?" are embedded in specific circumstances of shop work and shop talk (not as a general "decision theory," since there are no grounds for assuming offhand that a uniformity of treatment will obtain in any or all situations).
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In subsequent chapters on the topic of agreement, attention will be focused on the work of coming to such determinations in different situations of inquiry.

Notes

1 In two previous papers, a number of historical and instructional examples of artifact “misrecognitions” (“misrecognitions” is placed in quotes as a way of notating its retrospectively constituted character as a charge levied by particular scientists about the erroneous notions of their predecessors) were cited. See Lynch (1974a) and (1974b) for a preliminary discussion of the use of artifact accounts in scientific discourse as ways of discrediting discovery accounts.

The examples cited in these papers came from a variety of sources in the history of technology, in biological research literatures, and in practical microscopy handbooks. Some of the more startling accounts, in terms of their incongruity with what is currently believed in microbiology, were reports and illustrations of microscopic “globules” and minute “homunculi.” S. Bradbury (1967a, p. 164) speaks of a kind of instrumental “deception” that was associated with spherical and chromatic aberration in early microscopic lenses, and which historians invoke in accounting for reports of “globules” in tissue microstructure. Entities were described as globular in shape and were seen as structural constituents of tissues and other substances by some seventeenth and eighteenth-century researchers. These entities were later reformulated as artifacts arising from “fringes” around particles and edges which the aberrant lenses distortedly presented. Another of Bradbury’s accounts (1967b, p. 86) presents a case of an “erroneous” report of minute “homunculi” seen in spermatozoon cells during the eighteenth century. This report was attributed to a projection of a current theoretical “myth” into the blurry and ambiguous visibility of the cell provided by the imprecise optics of the microscopes used at the time. Also see Ritterbush (1972) for an account of globulist theories.

Numerous other accounts can be located which invoke instrumental artifact in conjunction with technical and interpretational error. These accounts are often not as dramatic as the two above cases, though they have in common the location of a positive claim with a technical or instrumental condition, which is attributed “responsibility” for the appearance of the artifact. Controversies in research literatures abound with charges and counter-charges of artifact. This holds not only for “major” discoveries, but for relatively minor ones as well, in all fields of scientific study.

2 Practical attempts to differentiate artifacts from non-constructed objects have occasioned debates in archeology over the applicability of one or another formal criterion as grounds for unambiguously claiming artifactuality for any discovered object. Not surprisingly, no single aspect of shape, substance, texture, i.e., form as such, has emerged as an agreed-to criterion of differentiation. Instead, elements of “context” are invoked in claiming discoveries, as in the following account by Bordaz (1972, p. 8).

It would be logical to postulate that man’s first step in tool making began when he purposely smashed stones and selected those fragments having a useful cutting edge. But it is obvious that this hypothesis is difficult to verify, for only when such stone fragments are found in association with split and broken bones or other evidence of human activity is it possible to distinguish between stones broken by frost, flowing water or other natural action and stones deliberately broken by man.

3 Philosophers have for a long time concerned themselves with the ontology of artifacts. Attempts have been made to formulate criteria of interpretation that distinguish a category of objects as artifactual. Often, these formulations of artifact incorporate a teleological element into the essential characterization of the object. Thus, for example, Jacques Monod (1971, p. 3) characterizes artifacts as “objects endowed with a purpose or project.” Monod provides that the identity of the artifact with a project or purpose is not sheerly a matter of free stipulation, but that the regular and repeated form (the approximativeness of the instance to some formal design) of those objects which are called artifacts show the teleological character of their objectivity. However, according to Monod (p. 7) these formal criteria take in more than those objects which are accountably artifactual in their origins.

In fact, on the basis of structural criteria, macroscopic ones, it is probably impossible to arrive at a definition of the artificial which, while including all “veritable” artifacts, such as the products of human workmanship, would exclude objects so clearly natural as crystalline structures, and indeed, the living beings themselves which we would also like to classify among natural systems.

A more radical version of a philosophical ontology of artifact is found in Heidegger (1962). Rather than defining the artifact by reference to a historically prior creative purpose, Heidegger expresses a relation of “ready to hand” as an representational constituent of artifact. Heidegger eschews the dichotomy between artifact and “natural” objectivity and instead speaks of the artifact or instrument as a condition for the disclosure of the “materials” of a project. As Macomber (1967, p. 40) states on behalf of Heidegger:

Nature first reveals itself not in contrast to the artifact but as that which is useful prior to all processing. We do not first encounter a natural being and then discover that there is something we can do with it, that it fits somehow into our plans and designs. It is only in relation to some possible use or insofar as it fits in with
our designs that we first encounter it at all. . . . In recognizing no more fundamental distinction between things than whether they are natural or artificial, philosophers have falsified immediate experience. There is no clear line to be drawn between the horse and the diesel engine, rock, brick, and cement, the orange and the loaf of bread.

The distinction is thereby formulated as not a matter of origins, but of apprehension in any current encounter. The artifact is then shown to be a feature of the encounter rather than of any independent characteristics of the object. The object's form is not irrelevant to its artifactual exhibition, but it cannot account for artifactuality when taken in isolation from a complex of other objects, relations, projects, i.e., worldly conditions for the disclosure of the object as artifact.

This latter conception is somewhat more akin to this chapter's than is Monod's formulation of a teleonomic principle. However, unlike any of the philosophers who have discussed artifact, I am not so much interested in developing a general formulation of "the artifactual" (as either a thing or a way of grasping the thing) prior to any inquiry into the settings where determinations of artifact are made as an occupational practice. In this study, the ways in which distinctions are made between artifactual and certifiably "real" phenomena are located as part of particular social settings. Elaborations on how that work is done have no compelling relation to principled ways of formulating how "artifact" is determined in an ideal space, or to formulations of a "best" or "most certain" way of characterizing the artifactual.

4 This analogy is suggested in the following passage from Hanson (1958, p. 4):

Consider two microbiologists. They look at a prepared slide; when asked what they see, they may give different answers. One sees in the cell before him a cluster of foreign matter: it is an artifact, a coagulum resulting from inadequate staining techniques. This clot has no more to do with the cell, in vivo, than the scars left on it by the archaeologist's spade have to do with the original shape of some Grecian urn.

5 Michael Polanyi (1966, p. 16) formulates the presence of the tool or instrument in a way that is distinct from its availability as objective "machinery." This "non-mechanistic" version of the instrument emphasizes the instrument-in-hand as something which shows up in the user's perception as a particular mode of sensibility in the scene disclosed through the instrument:

I have described how we learn to feel the end of a tool or probe hitting things outside. We may regard this as the transformation of the tool or probe into a sentient extension of our body, as Samuel Butler has said. . . . Whenever we use certain things for attending from them to other things, in the way in which we always use our own body, these things change their appearance.

They appear to us now in terms of the entities to which we are attending from our body. In this sense we can say that when we make a thing function as the proximal term of tacit knowing, we incorporate it in our body — or extend our body to include it — so that we come to dwell in it.

Polanyi calls this availability of the instrument as a bodily extension, "interiorization." The tacit presence of the instrument in the perceptual field provided via its use in Polanyi's formulation is reminiscent of Heidegger's discussion of "distraction" (see note 3), where the instrument is no longer explicitly available in the fascination with the objects it constitutes. Heidegger speaks of the "broken" instrument as an occasion on which the instrument reappears to its user (or, more accurately, to Dasein, the condition of an occupying awareness) from out of the "invisible" ground of its adequate transitivity. A general characterization of artifact accounts finds them distinct as accounts of objects in the Heideggerian sense of being accounts which make explicit reference to the instrumental complex rather than being couched in a grammar of external entitative apprehensions. Again, however, this general characterization of artifact-accounts glosses over their character as unique and situated events in shop work which bring into play a variety of considerations that are specific to these occasions. For example, what sort of research is being performed, what are the consequences of its findings, who is involved in the assessment of the work, and what manner of record is involved in the observability of the topical phenomena. These latter considerations are not very well characterized in a general account of "perception" or "interpretation," and will be addressed in terms of an ordered recitation of class later in this chapter.

6 Scientists do not necessarily hold a strong programmatic notion of the ultimate separability of the conditions of their inquiry (its "methods," ways of speaking, instruments, and preconditions in training) from the particulars of their discoveries. Especially when spoken to in a relatively casual setting or when speaking to each other in situations in which they are not held accountable for a definitive formulation of the work of Science, scientists make more specific mention of the contingencies of their research findings. At times, written accounts point out that findings are ultimately inseparable from the research setting despite the manner in which they are described in some journalistic versions of science:

Many neuroanatomists have been skeptical of the validity of the two types of synapse on the basis of the fact that the flattening of vesicles has been shown to depend on the osmolarity of the solutions used in preparing the tissue for electron microscopy. . . . But, in a sense, everything the electron microscopist sees is a distortion of the true dynamic living state. The interpretation of electron micrographs, and of any preparations of anatomical specimens, must be made with this
The quotation attempts to blur the distinction between artifact and "natural" appearance by pointing to how any microscopic rendering is necessarily artifactual. Shepherd then gives a post hoc practical rationale for his acceptance of the particular distinction between "kinds" of synapse. Although I agree that the in-itself character of neuroanatomical structure is a construction built from instrumentally apprehended visibilities, it does not provide strong grounds for dismissing any particular case of artifact account, as Shepherd does in the above instance. I have observed numerous cases in which artifact accounts have been responsible for transforming positive claims into "misinterpretations." Although the entire field of observables in microscopy is an artifactual accomplishment, this does not erase the consequentiality of members' discoveries of particular artifacts for what is said on behalf of specific observables, once their artifactuality is strongly demonstrated. The import of this, then, is that members' distinctions between artifact and "natural" objects do not rest on a difference between "practical" and "natural" origins. Instead, artifacts are discovered against a background of adequately transitive constructions (their "adequacy" being at times presumptive, and at other times specifically certified in members' inquiries), and are differentially treated as various kinds of trouble for interpretations of microscopic records. That the distinction may not rest on a programmatic separability of practical conditions of inquiry from the natural phenomena addressed through inquiry does not deny the fact that specific charges of artifact levied against particular discovery claims carry a great deal of weight and can greatly affect the consequentiality of any discovery.

"Trouble" has held a particular fascination for ethnomethodologists since Garfinkel's early work in which he developed the use of "troubles" as an "incongruity procedure" of methodological import in the study of social order. Garfinkel used "troubles" as a discovering procedure by creating situations of disruption and confusion for unsuspecting persons during the course of their participation in commonplace scenes. The bewilderment that "subjects" expressed and their attempts to restore ordinary and "reasonable" circumstances provided a perspicuous visibility to what Garfinkel cited as the background expectancies of a taken-for-granted achievement of social order (see Garfinkel, 1967, Ch. 2, pp. 35-75).

The use of "trouble" in this volume differs from Garfinkel's in being an analytic focus on particular occurrences of research trouble rather than an attempt deliberately to induce trouble for the purpose of transforming the situation into a cogent display of background expectancies. The troubles of which I speak did not often occasion dramatic moments of bewilderment and confusion, as did Garfinkel's exercises, since they arose within ordinary circumstances and were often rather commonly encountered sorts of trouble. Accordingly, it is possible that the trouble described here appeared within the conditions of an implicit common-sense organization and did not make the deeper features of that relied-upon organization visible. What is topical here, however, is not a background configuration of actions or expectancies, but a relation of an account to what it posits as its "background" circumstances. Artifact accounts are of interest here for the way they appear within specific circumstances of troubled visibility, and relate those troubles to antecedent contingencies of scientific shop work and its practical "environment." However, I am not attempting a strictly descriptive characterization of, for example, "kinds" of artifact accounts. I retain some of the methodological interest in artifacts that Garfinkel exemplified with "troubles" in his earlier work, as I have found that artifact accounts exhibit scientists' work of formulating the conditions of their inquiry in a manner that is interactionally situated and which specifies far more than does the recitation of methodological formulae.

8 In an account of the history of the sociology of science, Merton (1978) states that "early" (pre 1950s) treatments of science by sociologists focused mainly on "linkages" between science and the "environing society." This, Merton claims, led to "an absence of a conceptual framework for thinking about the social and cultural structure of science itself." Presumably, this "gap" has been remedied in more recent studies of the institutional, normative, and interactional characteristics of "science itself." However, a close examination of the studies which Merton upholds as examples of studies of the institution of science itself shows these studies to be conspicuously disengaged from actual occasions when scientists work together in doing research. Sociological studies have left the relation of particular scientists to their objects of study untouched in discussions of "sociological aspects" of scientific life. As Karl Popper (1970b, p. 657) noted although for entirely different purposes:

Objectivity is closely bound up with the social aspect of scientific method, with the fact that science and scientific objectivity do not (and cannot) result from the attempts of an individual scientist to be "objective," but from the cooperation of many scientists. Scientific objectivity can be described in the intersubjectivity of scientific method. But this social aspect of science is almost entirely neglected by those who call themselves sociologists of knowledge.

Work that, for example, elaborates the outlines of professional "networks" of scientists, or which focuses on "competition" among research teams often yields interesting and replicable findings about scientists and occupational aspects of their work, but fails to address the substantive matters that scientists identify with their fields of...
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study. However, it is through their work with, for example, the particulars of ultrastructural data on axon regeneration, that scientists interact with each other as scientists (whether in written or oral discourse). That they are competing for jobs, the favor of higher-ups in the organization, or attempting to "scoop" another lab with their latest researches are relevant matters once understood in the way they show up in scientists' "object talk," and not as "variables" cleanly separable from their substantive involvement in the primary field of scientific action and interaction. For a sociologist to isolate relevant social aspects of science without direct and continual reference to the substantive work of a given scientific enterprise is akin to an anthropologist attempting to understand a complex set of organized practices in a culture for which he makes no attempt to learn or understand how members of the culture speak of the practices in their own language. An understanding is reached in such a situation, but whether it would count as in the setting studied (or, perhaps I should say, whether it would be recognizable to members as an abstraction which located as its materials accountable events, witnessable details and known environmental objects) would be irrelevant to the aims of the study and inaccessible to analytic criteria of its "validity."

Although the sociology of science is conspicuous for its lack of any strong ethnographic basis in the particular worldly understandings of specific sciences, it is not alone among sociological studies of occupations in its programmatic irrelevance to the "what" of the occupations it studies. Garfinkel (lectures, UCLA, 1974-7) is responsible for pointing out the ways in which the "details" which so prevalently occupy members engaged in doing occupationally situated work are left out of sociological studies. Not only are they left out, according to Garfinkel, they are not even considered, and perhaps not available as witnessable events within prevailing sociological methods. Garfinkel proposes as a topic of study for ethnemethodological research the missed substantiality that makes up the work of an occupation, and which is absent in sociology's analytic of occupations. This volume is one of a series of studies by Garfinkel and his students which seek to address that topic (cf. Livingston, 1976b). This, of course, does not assure that the present study can claim to have achieved but the most speculative grasp of neuroscientific work within the monstrously difficult strictures of Garfinkel's program.

Agreement is often mentioned in science studies as a condition for the achievement of scientific knowledge. How such agreement obtains in scientific work, however, is described in these studies as an outcome of antecedent training, or professional "socialization" (cf. Thomas Kuhn (1970, p. 192), where he attributes the "sharing" of a paradigm of scientific knowledge to common "exemplars" used in the training of scientists). When I speak of agreement here, however, I am addressing a phenomenon that is more immediately observable as part of specific situations in which members argue over problematic phenomena, make plans over what to do next, and negotiate the adequacy of their data. This approach to agreement differs markedly from a general attribution of agreement to concerted activities and accounts insofar as agreement is displayed as part of getting the work done, as particular achieved-agreements. In situations in which artifacts are possibly present, agreement is involved as an explicit part of the setting when members work to settle the accountability of their data for the practical purposes of the local inquiry. That a more general, tacit, and "underlying" agreement may also be involved in such settings is no doubt likely, though its visibility in the setting appears more problematic and elusive. The notion of agreement as a "sharing" or correspondence of accounts will be critiqued in Chapter 6.

These "presentational" features of a display format in electron microscopy are discussed at length in my paper, "Art and artifact in microscopy" (1976). In that paper I discuss several analytic "parameters" of the microscopic field. These I refer to as "temporal continuance," "spatial continuance," and "perspectival embeddedness." I elaborate upon them in a kind of phenomenology of a micrographic record. These analytic features of the display format of microscopy differ from what will be discussed as artifact in this chapter. In this chapter the discussion is limited to occasion of artifact which became accountable in specific settings of inquiry, and not as general orderings of the work's constructive analytic relation to its phenomena.

At one time I explored the matter of whether artifacts in electron micrographs showed any distinguishing features of shape that would account for their recognizability. Particularly relevant is the issue of "geometricity," where constructed forms (artifacts) are distinguished in an analytic manner from an experiential "nature" by their approximation to the "limit forms" of analytic geometry. (For theoretic accounts of this distinction, see Husserl (1970, pp. 23-59). Merod (1971) also discusses a contrast in the regular and repeated form of constructed objects as compared with "natural" entities. Giedion (1948, pp. 77-129) discusses a contrast between the mechanized operation of actions in early slaughterhouse assembly lines and the "organic" variabilities in the carcasses being processed. Continual technical troubles of design were seen to be inherent in the encounter of the geometrical planes of operation of the mechanism with the more variable configurations of the animals being rendered.) It occurred to me that artifacts in scientific research might be detectable in the relatively geometric form of instrumental "inscriptions" on the organic material. No such geometric criterion was upheld, however, in my examination of cases of accountable artifacts. Some artifacts, such as linear "streaks" related to scratches by the microtome slicing, "folds" in tissue, and "presentational artifacts" such as the linearity of photo edges, the microscopic "field," and the regular clock-time intervals used as "time
points” in temporal reconstructions of events did show a conspicuous approximativeness to geometrically regular form. These features, however, were among the least problematic artifacts, and in the cases of “presentational artifacts” were not accountable in the work as “artifacts” per se, but were analytic constructions which located an order of obvious instrumentalities of access to a more ambiguous ground of visibility.

12 The use of the term “archeology” in this chapter is distinct from that used by Michel Foucault (1972, 1975). In Foucault’s work, archeology is employed as a name for an historical method of analysis, where complexes are identified in the relationships of language and social forms in historically constituted objectivities (such as members’ accounts of “disease” or “madness”). Here, I locate archeology with the work of members, as a way of accounting for artifacts which arise in their work. It is not primarily my mode of analysis in addressing members’ work, but is a way of characterizing that work as it was performed by members in its local setting.

13 By “ontological” here I am not speaking of a philosophical concern with the object-character of artifacts, as in Monod’s account (op. cit.). What is intended here is that artifacts are objects in their own right whose presence in lab inquiry is of interest. They are not viewed as illusory chimeras to be overcome by more careful technical practices, but as objects posited in lab members’ accounts and as “discoveries” in lab work. Their negative significance in the work of science does not deter an interest in observing the varieties of artifact as they appeared in members’ accounts.

14 Remedial treatments of typical artifact possibilities are found, for example, in the following instructional texts for light microscopic work: Hartley (1964); Munoz and Charipper (1943); Martin (1958); Needham (1958); and Schwab (1963). These manuals elaborate upon the possibilities of artifactual trouble in the “artificial” preparation of living tissue for microscopic analysis, and give suggestions on how to avoid them in “careful” work. These instructional texts are used in discussing common artifactual possibilities in the work of microscopy in Lynch (1974b).

15 Although in some cases artifacts appear as substantive things (as, for instance, a blotch of stain on a microscopic slide), they are attributionally located within the “subjective” conditions of observation. That is, they are seen as dependent upon the instrumental conditions of perception. These conditions include a temporal order of preparatory actions, insofar as the actions are made visible as traces on the template of the “in-itself” tissue. We are not committing ourselves to a Cartesian version of lab work in speaking of instrumental actions as distinct attributional grounds from the “tissue” observed. Clearly, the presumptive “ultrastructure” of the tissue is as dependent for its visibility as are artifacts upon the conditions of lab work in its instrumental projects. However, within members’ accounts a separation between conditions of observation and the in-itself character of the tissues is constituted along with attributions of artifact. Prior to any particular case of such an attribution a more indeterminate situation is available, and arguments for particular artifacts or discoveries of particular artifact-possibilities arise from a scene of observables which await and resist the work of distinguishing “objective” from “artifactual” phenomena. The distinction arises as a claim for any particular case of a phenomenon.

16 In speaking of a disjunctive relation between “holes” and their ultrastructural surroundings, I do not mean to be saying that some visible aspect of the micrographic record unequivocally showed artifactuality. The “disjunctive” relation, consisted of an “interruption” of the flowing textures of organelles, the lack of anatomical configurations supporting the “hole’s” presence within the tissue organization, and the absence of anatomical or functional rationales for the phenomenon’s presence in the tissue. However, such “criteria” could, in principle, be overrun by some alternative account of how the “holes” were anatomically integrated into accountable tissue structures. Nevertheless, within the local order of accounts in the lab, the “holes” were undeniably artifactual. Endless analytic rationales could be cited in supporting an account of the phenomenon’s artifactuality, though this did not preclude the possibility of alternative interpretations (especially by one naive to accounts of the phenomenon’s artifactuality). An account of “holes” in the tissue which attributed a disjunctive relation to the visibility of the “holes” would only be credible from an informed acceptance of a complex of accounts, both of the entitative system of brain ultrastructure, and of the available manners in which that ultrastructure could be formulated in lab discourse.

17 Members’ accounts of artifacts did not always include accounts of how the thing may have arisen from sources in the work of preparing tissues for analysis. Often when artifactual features were very obviously present in an array of neural ultrastructure they were used as “reference points” by electron microscopists. For instance, they were used as points of departure and return for high-powered explorations of a microscopic terrain. At other times they were “passed over” as inconsequential flaws in a micrographic field. Their consequentiality was contingent on the circumstances of any particular inquiry or attempt to construct an “aesthetic” display of ultrastructural configurations. These differing consequentialities of artifacts were borne out in the following observations of an electron microscopist at work:

His wayfinding was not unproblematic. He would rely upon the edge of the material as a guide when scanning . . . would track along the edge until he found an appropriate place to move further within the cell border. Then he got lost (especially at high power). With each successive adjustment of magnification the field would reverse polarity, so that he often lost track of the direction of his previous scanning. When he got lost he would lower the
power to 100, and start up again. Not knowing the layout of his expectations I had a hard time knowing how he was orienting in the field. He had a hard time locating mossy fibers that had taken up peroxidase. He didn’t seem to quite know why more didn’t show up. He stated that perhaps the counterstain affected the label, but that that was a guess. He also came across several things that he wondered about out-loud, in such terms as, “what the fuck’s that.” In most cases he’d glance at them and pass on, not bothering to venture a guess at what they were, even when I asked for one. There appeared to be many artifacts—blotches, bubbles, streaks, holes, tears, folds, and specks—which in most instances stood out distinctly from the range of materials and were not difficult to identify as artifacts. How they were artifacts was another story, though in some cases he could account for them (as, e.g., breathing on the preparatory fluid, extra precipitate, folds in the section) in many instances he’d venture no guess. For most slides he complained about the material, though some he labelled as O.K., or good, though the best material had been wrecked by a puncture, apparently from the tweezer. (from fieldnotes)

18 The distinction discussed here between “users” and “lookers” is reminiscent of Reichenbach’s (1938) famous dichotomy between the context of discovery and the context of justification. The distinction between “users” and “lookers” may seem to be a kind of local vernacular version of Reichenbach’s formulation. That formulation has been attacked in science studies by those who, like Feyerabend (1970), claim that justification is already involved in the analytic work of discovery; that discovering work anticipates the reception of its yet-to-be-realized possibilities. In the lab observed in this study the distinction between “users” and “lookers” marked a difference between local conditions of accountability and those in a literary “public”. Although justification to members of the local community seemed to be involved whenever researchers addressed discovered or describable phenomena, conditions of adequate justification (convincing one’s collaborators, assistants, bosses, etc.) involved a different sort of argument than in publicly circulated written discourse. It is, no doubt, true that the work of the lab, from its inception, anticipated the announcement of its findings to a “wider” community. This, however, did not undermine the fact that for members, in their local discourse, a distinction was maintained between conditions of accountability in shop work and those in a literature.

19 “Error” is discussed more extensively under the heading of “Failure and error,” pp. 115–18.

20 Freeze etching is a technique used in electron microscopy by which cellular material is frozen solid and then fractured to expose an inner surface. The fracture is said to show convex and concave areas, ridges, and differences in texture according to variations in the protoplasmic terrain inside of cells. “Shadowing techniques” vaporize a heavy metal substance at an oblique angle to the fracture face to provide a differential pattern of electron dense material analogous to a pattern of shadows on the surface of an uneven landscape at sunset. The technique is used as a way of investigating the inner configuration of membranes and has been used in discoveries of the molecular structure of cell membranes (see Pinto da Silva and Branton, 1970).

21 See note 1 of this chapter for a brief discussion of “globulist” theory as reinterpreted in historical texts.

22 The case of “holes” mentioned here should not be confused with the more typical “holes” artifact discussed under “positive artifacts” in this chapter. This particular case was characterized by members as showing “holes,” but was by no means subsumed under any categorical type of artifact (such as knife marks). It was treated in members’ shop talk as a problematic instance and one which was yet to be related to its sources.

23 It is imaginable that scientists could take something like the “holes” phenomena, treat them as a discovery and try to replicate the methodological steps that led to their appearance. One could, in this case, try to faithfully reproduce the procedure, and to evaluate the effectiveness of the procedure in reference to whether the “holes” reappear in any attempt. In this case, however, and in other cases, lab members did not seriously consider doing so. Why they failed to so do is not answerable in any straightforward way, since it is difficult to supply a principled rationale for why one phenomenon and not another would be pursued further in the absence of any clear validating or discrediting evidences. In the case of the “holes” it was certainly true that prior photographs, light microscopic slides, and other lab records had not shown such phenomena as were abundantly available in the sections from the one particular brain. However, the peculiarity of the phenomenon compared to the more common possibilities did not preclude its being formulated as evidence for a discovery related to an accidental technical innovation, or to an as yet unnoticed ultrastructural constituent. Given the rather hazy appearance of electron microscopic renderings, it was possible that something “new” could have appeared in the problematic photographs, which later examinations might bear out as, after all, a regular phenomenon heretofore having gone unrecognized. The manner in which the phenomenon was treated in members, shop talk (as distorted axons, dendrites, or as holes in the tissue) did not offer any practical rationale for checking the phenomenon out further, either as a possibly new constituent, or as an artifact which could threaten the lab’s accounts of axon sprouting. Compare this phenomenon with the case of the “microglia in the capillary” which is discussed later in this chapter. In the latter case, a phenomenon was discovered as an isolated case, but was explored in some further detail because of the way it seemingly documented an
available interpretation which was contrary to the lab's account of axon sprouting.

24 For some electron microscopists, "microglia" were a "doubtful" cell type. In a study of glial cells in the cat cerebellum, Eager and Eager (1966, p. 553) claim that "cells identifiable as microglia were never observed." The authors suggest that what had been called "microglia" might be indistinguishable on anatomical grounds from oligodendrocytes or "reactive astrocytes." A general photographic text on neuro-anatomical ultrastructure (Peters, Palay, and Webster, 1970, pp. 126-7) describes microglia as an "uncertain" cell type and gives a number of anatomical pros-and-cons for classifying microglia as a distinct cell type.

25 Here I do not wish to suggest that the troubles occasioned by the phenomenon's appearance were temporally removed from the seeing of the phenomenon. In my own observation of the phenomenon, its theoretic import arose simultaneously with my locating it in a displayed micrographic scene. What I am alluding to in speaking of the "initial appearance" of the object is that at first glance it looked like a perfectly well formed microglia cell, and that it was not blurry, broken, smeared, or otherwise indicative of some defect in the material. It was precisely because of its appearance as a "reasonable" instance of a cell, in circumstances where a cell could imaginably occur, that it was troublesome for an account that claimed that such a cell was not supposed to be where it was.

26 The argument used by the lab member is clearly an instance of an ad hoc account. To say this of a case of scientific reasoning is often interpreted as fault finding. It may seem to imply that the scientific practitioner had such a "stake" in an erroneous theory as to ignore and dismiss contrary evidences through the devices of conjuring up an argument to "fit" the peculiar character of the case. An ironic stance is not taken here, however, since I do not consider ad hoc argument to be an avoidable and embarrassing part of scientific research except in cases where it turns out to have been wrong. Instead, I find it to be an essential feature of the way research procedures are accomplished in their specific settings. Feyerabend (1970) takes a radical position in the philosophy of science by suggesting that ad hoc reasoning is not only a necessary feature of the practice of interpreting empirical results under the auspices of a theory, but that it is a positive and essential feature of scientific work wherever that work is accomplished. In a study of research assistants' coding practices, Garfinkel (1967, pp. 18-24) described ad hoc practices as ways in which coders managed to achieve the practical relevance of their coding instructions in each circumstance of their application. His study emphasized "ad hocing" as a practice in the research he analyzed, which extended the sense of "ad hocing" considerably beyond its connotation in philosophy as a questionable relation between an argument and a given theory. Garfinkel's study provided for ad hoc practices as ways in which work gets done in its actual setting, and as ways in which the exigencies of an empirically situated inquiry are managed so as to invoke the relevance of rule-like formulations, while also preserving the sense of the rules as trans-situational guidelines. "Ad hocing" was thereby constitutive of the stable collections of "rules" which purportedly "governed" the practices of an inquiry. In this way, "ad hocing" can be spoken of as anterior to the rules that are made relevant in the inquiry.

In the above case, the account of the microglia was exhibited as an ad hoc argument. Other instances of lab work that are presented in this volume can also be spoken of as "ad hocing." For example, the improvisatory work of doing histological procedures exhibited ad hoc practices in ensuring the efficacy of a method in light of circumstantial developments such as troubles with the ornery character of animals, "unforeseen" mistakes, and indefinitely "messy" material displays. See the discussion of "superstitions" below for an elaboration of this matter.

27 In speaking of a lack of rational accountability in scientists' "superstitious" procedures I am not implying that the procedures were without "sense," or that they were not at times seen to be adequate and effective ways of getting the work done. Instead, I am referring to how accounts of some aspects of shop work, which were formulated in lab scientists' conversations with me, made a distinction between procedures that could be related to physico-chemical principles for explaining how they worked, and procedures which were used (and which often worked) without being clearly set in those terms. This distinction recalls Garfinkel's discussion of "the rational properties of scientific and common-sense activities" (1967, pp. 262-83). Garfinkel lists fourteen "rationalities" as an inventory of activities which count on some occasions of conduct as effective modes of decision making and conduct. Four of these rationalities are described as being peculiar to scientists' idealized norms of conduct, and were not accounted part of non-scientific "rational" conduct. Garfinkel also points out that scientists' "methods" are not exclusively defined in terms of the specific ideals of scientific rational conduct, but that actual scientific work involves all of the fourteen listed "rationalities" as well as the four specifically "scientific" rationalities.

In accounts of their shop work which members of the lab provided in interview situations, they seemed to imply that conduct in shop work which could not be rationalized by reference to formal scientific theory and practice was necessary for getting the work done, though relatively faultable. The common-sense rationality of, for example, repeating a serial order of actions in light of a positive outcome of the order of actions on some prior occasion, was located as grounds for the attribution of "superstition."

28 Carl Hempel (lecture, U.C., Irvine; Spring 1978) mentioned a number of famous cases of scientific inquiry which encountered actual or possible negative artifacts. In one case a later discovery of a phenomenon led to an account of how an artifact delayed its
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documentation. In this case, of the discovery of the planet Pluto, a prediction based on perturbations in the orbits of visible planets led to a systematic search which lasted from 1913–25 until photographic evidence was obtained. Because of the extended time period which intervened before the planet was photographed, the photographic plates that had been taken in each sector of the sky during the study were re-examined. It was determined during this search of the photographs that the photographic emulsion on the very spot where Pluto should have registered on a given plate had not "taken." This artifact was then invoked in accounting for the delay in the photographic documentation of the hypothesized planet.

In another case, that of the famous controversy between Millikan and Ehrenhaft on the oil drop experiment, Ehrenhaft's discrepant measurement of the minimum charge on the oil drop was never resolved through the documentation of any artifact. Ehrenhaft and his students experimentally documented a result which measured one-half of the minimum quantum of charge as measured in Millikan's experiment. Ehrenhaft's results were never replicated outside of the circle of his own students, though the argument was made that the successful performance of the experiment required a expertise that had not yet been duplicated in the "unsuccessful" attempts at replication. The controversy went on for years, with no definitive resolution, and it remained possible that an undetected "negative artifact" had inhibited successful replication of Ehrenhaft's experiment. Millikan's results held, and Ehrenhaft's declined in their relative significance. No documentation of either a substantive artifact in Ehrenhaft's experiment or a negative artifact in the attempts to replicate it ever obtained. Holton (1978, pp. 25–83), gives a somewhat different treatment of the controversy, as he points to different local practices in the selection of "good" results in accounting for the discrepancy between the published results of the rival researches.

29 Experimental "bugs" were observed whenever experiments were performed. These took on different consequences, depending upon whether they appeared as episodic troubles with a particular attempt to do a procedure, or whether they were found to be tied to a routine feature of the procedure's accomplishment. In the prior case, a problem such as a strong "background noise" registering on an oscillograph monitor was seen as a trouble in the particular instance of the performance of a recording; as a more or less isolated fluctuation against a more stable ground of regular visibilities. This is far less consequential for the course of a project than, for example, a fairly constant background noise level of sufficient intensity to prevent particular discoverables from registering on an oscillograph screen. In this case, the noise level, although it could "drown out" a fine order of possibly consequential phenomena, would be less obviously part of any given attempt at doing the experiment than would a momentary upsurge of "noise."

30 These inferences are, in part, based on research I conducted in Fall, 1974. I observed and recorded the interactions between a lab instructor and introductory students while they attempted to follow a course of instructions on microscopic observation. I also was invited into the scene in a rather spontaneous manner by students who sought instructions on how to do their exercises. Student questions were often directed to their difficulty in "seeing" the things the instructor and lab manual described for them. Sometimes, the instructor would look into the student's microscope, place a pointer on the object (a cell wall, chloroplast, etc.) and then instruct the student that, for example, "it's the green thing." At other times the instructor, in looking at the student's preparation, would not himself see the phenomenon, and would then question the student whether he, for example, drew the "specimen" from the appropriate bottle in the lab, or used the proper stain in the proper amount. A common resolution to such problems was to instruct the student to try again, with no definitive problem or "mistake" having emerged in the instructor's interaction with the student.

31 Barnes (1974, p. 40) states that in specific settings of scientific work the conditions under which existing beliefs would be abandoned are never specified, and often they are retained in the face of apparently strong disconfirming evidence. In many cases beliefs initially sustained in this way have eventually become unquestioningly accepted as part of our present knowledge; an instance is the theory of evolution, which at one time seemed incompatible with reliable physical estimates of the age of the sun.

Even defenders of the notion that "crucial tests" are ideally part of the manner in which scientific knowledge is certified make necessary reference to non-specified grounds whereby investigators determine if the results of any experiment count as the materials of a proof or disproof. The mediating presence of the experiment's practical and technical accomplishment is always open to assessments of adequacy or inadequacy, and the acceptance of an experiment as adequately done brings into play such matters as the experimenter's reputation, and the purported adequacy of any equipment used. This is not to say that "crucial tests" have not been accomplished in science, or that in some instances they have not had exceptional influence on subsequent scientific development. It is to say, instead, that such "tests" are embedded in a social and practical circumstance, and that this circumstantiality is far from incidental to the determinateness of the accomplished results.

32 I have used the word "effect" in formulating the intentional outcome of a laboratory experiment. What is meant here by that term is not easily defined, though, perhaps it is easier to specify what an "effect" is not. It is not defined by describing the work of discovery as effacement. It is not available in the image of a scientist "exploring" an unknown terrain. It is not a creation, product, or object of art in the sense of being something fashioned from the
available tools of a craft, nor is it disengaged from the work of constructing, designing, fashioning. It connotes a kind of independent responsiveness; laws and ways of being that are nonetheless practically accessible. It brings the constructive character of the enterprise into relief, though this is a very peculiar kind of construction. It is not "making" per se, but making something happen, making it work. See Knorr (1977, p. 670) for a somewhat related discussion of "making things work."

The instances of scientific activity that come under the purview of "making it work" do not exhaust the range of shop practices and extended projects of inquiries that were observed. Not all occasions of scientific work were directed towards obtaining a more or less definite outcome in advance of its actualization. Although in this discussion I will not specify a clear range of "varieties" of shop practices organized in terms of their relation to a final discovery or effect, preliminary observations showed discovering work to involve at least two temporal organizations:

1. A lab worker, usually while engaged in a definite task (such as shooting electron micrographs, preparing a display slide for an article, performing an experimental attempt) would note something that was initially incidental to the task being performed. One occasion of this was observed while an electron microscopist went through a series of prepared slides to find instances of "labelled" mossy terminals (a specific type of axon terminal which had been stained - or labelled - for the purposes of a particular anatomical study). In the course of this work, he noted that he saw a particular type of vesicle in the neural material that "looked like" a fairly rare phenomenon of some current interest in the field. He shot a photograph of the thing and filed it away. I do not know if anything more came of the photograph, but the incident illustrates a kind of temporal organization in which a thing seen brings into relevance past and future issues with regard to its possible significance as a discorsiderable. At the time, however, it was not specifically an "object" of the ongoing discovering work. Further illustration of this sort of temporality in a discovery can be found in S. W. Woolgar's (1976) discussion of the progression of accounts (and presumptive events) in the discovery of the pulsar phenomenon in radio-astronomy.

2. A second form of temporality is that of a project which seeks and secures an outcome. Not only do "experiments" take on this sort of intentionality, but the work of "observing," "tallying," and "measuring" phenomena have the character inquiry into "finding what you're looking for."

I do not offer the above two extremes as separate sorts of projects, as it may be more often the case that shop work has a considerable variation in the degree to which "seeking" predominates over "looking around." For a related discussion see Hanson, (1967).

3. I am somewhat uncomfortable with the distinction between "oops" and "what went wrong?" on the basis of the relative occurrence of an "error" and its recognition. It seems mostly to be the case that errors are noticed after their occurrence, with expressions of surprise and bewilderment accompanying that notice (in cases where the researcher finds his or her own errors). In the case of "oops" the lapse of time is relatively shorter and the claim at the observational account on the error is perhaps stronger than in "what went wrong?" In both cases, however, the "error itself" is a retrospective attribution, and it has the curious feature of something that may have been avoided if it been "observed" concurrently (perhaps I should say it is hard to imagine a situation of observing one's errors in the immedancy of their occurrence).

34 Pollner's work on "mundane reasoning" (1974, pp. 35-54) provides a number of pertinent examples of error attributions. According to Pollner's analyses of traffic court sessions it was commonly a matter for the judge to decide between discrepant descriptions of the "same" traffic situation which were presented by defendant and arresting officer. Pollner elaborates upon how "solutions" to such "reality disjunctures" were generated from within a set of presuppositions which implicated the stable, known in common, and never doubted conditions of a real-worldly setting of traffic events within which the purported offense occurred. Within the judges' subscription to "mundane reasoning," judgments about the "what really happened" of traffic events accounted for discrepancies between defendant and officer versions by ascribing the discrepancies to "subjective" matters of interpretation or description of the "same real scene." Such attributions as "lies," "distortions," "illusions," "hallucinations," "deceptions," "errors," and the perspectivity of observational positions were made in accounting for such "reality disjunctures." On some occasions Pollner found the judge to be accomplishing the subtle work of making an attribution of "subjective error" without the attribution's connoting a "distrust" or "disrespect" for the veridicality and competence of the source of the discredited account. This was especially true when the arresting policeman's account was decided against. In such cases, judges provided for how the observing officers' account of the traffic event was obscured as a result of obstructions, darkness, or other circumstantial features of the environment. Among other things, the use of "circumstances" in such an attribution of "error" freed the officer of the perjurative connotations that "error" might otherwise have carried.

Such accounts locate an error within a situation that, in part, exempts the error from notions of personal responsibility. Although such "contingencies" are specified in terms of the details of an intersubjective environment, they are implicated as the configuration of an "incomplete" perspective out of which "illusions," "deceptions" and "misinterpretations" arise. Such "errors" are seen to require a particular "worldly" context, such that culpabilities are then allocated in a way that wholly, or partially,
exempts personal authorship from any moral offense. In scientific accounts, such “circumstances” are not merely a rhetorical way of excusing error; they present the trouble of being “uncontrolled” sources of variance in any further attempts to achieve a regular result. The “domestication” of organic brain events in a constructed in vitro “test system” is seen to entail innumerable sources of variability in results. “Contingencies” which effect such variabilities provide innumerable accountable sources of trouble that extend well beyond any personal attribution of “mistake” or “error”. Nevertheless, these troubles implicate the adequacy of practices in a way that is not excused through the recitation of possible contingencies. Such attributions operate in the context of prospects for remedying the confounding “objective” circumstance. They are not exempt from practical address.

35 I am reminded here of Feyerabend’s statement (1970, p. 78):

In these circumstances one can do one of the following two things. One can stop appealing to permanent standards which remain in force throughout history and govern every single period of scientific development and every transition from one period to another. Or one can retain such standards as a verbal ornament, as a memorial to happier times when it was still thought possible to run a complex and catastrophic business like science by a few simple and “rational” rules.

It is only to be added that not only are such “standards” likely to vary from historical period to historical period, but that within the present “period” a consistent rule-governed practice of science is difficult to find in the most local conditions – within a single laboratory, for a particular project, for a working day of a single lab member. This is not to say that such things are without their orderliness.

36 See Lynch (1976).
5 Laboratory shop talk

The present chapter is addressed to the phenomenon of laboratory shop talk. Shop talk will be treated as a condition of the observability of science which exists in sharp contrast to the versions of science in the science studies literature. Characteristics of shop talk which distinguish it, on the one hand, from published research results and, on the other, from "ordinary" conversation will be addressed in a preliminary fashion. In discussing how shop talk is a situated variant of conversation I will address the problem of its competent analyzability. Finally, I will move on to a more specific analysis of agreement as it was observed in tape recorded instances of laboratory shop talk.

Literary versions of science

A relatively casual reading of the science studies literature yields the following observation: the Science that is discussed in such accounts is primarily one that appears within written description. It is further notable that this Science is discussed as a general phenomenon that cuts across several centuries of Western history and is common to numerous disciplines and sub-disciplines of inquiry. In science studies accounts this general historical phenomenon is commonly indexed with the use of cogently portrayed examples of monumental discoveries and achievements of the greatest scientists. These rather unremarkable features of science's accountability - its written availability, general character, and the landmarks of its historicity - make up "literary versions of science." I will briefly discuss that topic as a way of contrasting it to laboratory shop talk.
Written accountability

In speaking of the reliance on the written accountability of science that prevails in science studies, I do not mean to say that such studies only consult written documents in generating "news" about science. The terms, "written accountability," allude to an availability of science that is provided through the devices of "description" - an availability that is produced in the relation of discourse to a self-sufficient and external object. In science studies, science is identified in-and-through its literary by-products in the following ways:

(a) Science studies are written, and thereby rely upon the formulability of their phenomenon - its amenability to textual treatment, its availability as an "object" of an account, and its exhibition of qualities that are congruent with the resources of written language.

(b) Studies of science not only are formulated in written discourse, they also encounter a phenomenon that is prepackaged into written "description." This formulability precedes studies of science and is found as an interior feature of the enterprise of science. Written accounts are produced as routine matters in the "writing up" of results, reports, journal articles, notes, letters, retrospective interviews, lectures, and autobiographies of scientists. Often science studies are authored by scientists who "reflect," in general or particular ways, upon their work background, thereby accomplishing a relatively direct transition from interior conditions of formulation to the conditions of science studies' discourse.

Generality

Science is portrayed as a general phenomenon in science studies. This is to say that science studies depict a science that is trans-situational, and not a matter of, for example, watching scientists at work to see what they are doing in any immediate way. Science studies are replete with systems of explanation, rules of logic and method, principles of theory construction and experimental design, and norms of science which are formulated so as to cover all variants (or some disciplinary sub-unit) of Science. These rules of evidence, norms of science, etc., cover a science which is more extensive than any instance of its performance. Regularities which are attributed to science are not of the manner of empirically situated "facts" observed in the behavior of scientists, but instead describe persistent themes which are most congruent with communally held "versions" of how science is or should be.

Monumentality

One rarely, if ever, finds historical reference to trivial, mediocre, or otherwise "unimportant" scientific achievements. When "poor" science is mentioned, case examples are often selected from historically significant "blunders," hoaxes, or misinterpretations. When positive achievements are mentioned in specifics, the achievements, for example, of Newton, Galileo, Copernicus, Kepler, Einstein and Bohr, take precedence over those of the hordes of researchers whose achievements have lived and died in the mass accumulation of research literatures in the special sciences. A relatively limited corpus of famous discoveries and famous scientists gets an extraordinary amount of attention in the science studies literature. The same cases tend to reappear as materials for reasoned argument regardless of the specific claims or contentions made in any particular argument. These cases provide a common corpus with which historians and philosophers construct their arguments and the specific character of the described case changes with the particular argument it is used to document. One can imagine the cases providing materials for a kind of Wittgensteinian "language game" where arguments, claims, counter-claims, agreements, and other gesticulations take on their vivid substantiability through a discursive working of such cases.

The "game" with the cases appears to be a matter of employing them as: (a) "empirical ideals" - monuments of scientific achievement which best exhibit the finest, most laudable, and most significant potentialities of the activity, e.g. moments of genius, socially significant inventions, discoveries which radically alter common sense understandings, and cures and remedies for all manner of human troubles, and (b) demonstrations: the clear visibility of, for example, deductive or inductive ways of proceeding, use of specific reasoning practices, and presence of a "receptive" or inhibitory social context for a discovery is cogently expressed in the use of well-known and indubitably significant case histories. The character of the cases as known-in-common exemplars among philosophers and historians of science allows for their use as empirical materials which are worked into the various contending arguments on how science is done.

An offshoot of these famous examples in literary accounts of science is that the history of science becomes available as a temporal sequence of great geniuses and their monumental discoveries. These landmark achievements provide a kind of visibility for that history by becoming the "nodes" or "places" at which issues of temporal change and development are seen to
take place. More importantly, the common use of this history of exemplary stories provides a sense of science as something understandable to a lay audience. (By “lay” I do not necessarily mean a “man on the street,” but a non-practitioner in a specific research enterprise that is represented in the history. A physicist can be a lay audience in this sense to the researches of a historically represented discoverer whose theorizing, methods, contemporaneous rationales, technical manners of speaking, and phenomena of interest are embedded in their circumstances to the extent that their writings are inaccessible to a casual reading.) Accordingly, the Science that historians and philosophers describe in their literary accounts is a common science: a science that “we” can know, evaluate, compare our own researches to, form opinions about, describe logics for, etc., without having to engage in any or all of the varieties of technical practices glossed under the unitary heading of science.

Literary versions of science do not, however, exhaust the ways in which scientific discourse and practice are accountable. This comes readily accessible when one attempts to read a specialized research article in an unfamiliar field, or when one observes the actions and talk in a research laboratory. In such circumstances, scientific matters are provided as technical talk and technical competences. To a non-practitioner, the writings and talk of specific researches is difficult to sensibly “grasp,” and unless one is well acquainted with work in the specific area a research article is frustrating to read, boring, and mostly opaque. In contrast to “literary versions” of science, research articles and shop talk are largely “uninteresting” to lay appreciations, and are highly specific in their technical content. One might claim that such technical work and talk is of relatively little interest in comparison to the great discoveries documented in science studies accounts, other than as, for instance, exemplification of “normal science.” However, even monumental findings, when published in their original forums (other than, for example, Scientific American, or other widely read publications), often have a technical and opaque character under a “lay” reading. Regardless of how technical and “uninteresting” this situated science talk and writing might be, it is the discourse in which discoveries arise, are argued for in a consequential community, and are ratified or disputed, perhaps later to be formulated in a more accessible way.

Where literary accounts provide for science in a way that is disengaged from the actual workings of an inquiry, technical science writing and technical shop talk produce accounts which are behavioral features of science itself; they are part of the work of doing science rather than descriptions of science in any general way. The interest here is in how such accounts provide materials for the doing of science – materials which are situated in the ongoing accomplishment of science in a social setting. These accounts are neither a “literature” nor a generalized system of rules and procedures; instead, they make up a competence – a competence which is not particularly tied to being able to talk, per se, but which is required and implicated in the actions of speaking sensibly in a local setting.

This latter involvement of discourse within the enterprise of science will be called “talking science,” as a way of contrasting it from the “talk about science” which constitutes literary accounts. “Talk about science” is not limited to science studies literatures, as it forms a routine part of the work of doing science in a laboratory. Nor is “talk about science” distinct in being a written phenomenon, since “talking science” can be done in writing and “talk about science” can be a manner of speaking in a research laboratory. The following sections will treat “talk about science” as it was ethnographically part of a particular research enterprise – not as a “literary account” but as an accountable part of doing routine laboratory work. “Talking science” will then be addressed in terms of the observability of members’ shop talk. Although I will not pursue the matter here, “talking science” may be analytically part of scientific writing as well as talk . . . though its availability as writing may be observable only to those who are party to the work of reading and writing scientific reports in a specific research field.

B “Talk about science”

1 Tours

The local setting of the laboratory had routine ways of enabling talk about science to be done in a regular and non-disruptive way. These “tours” consisted in spontaneously organized “show and tell” sessions which were conducted by lab members for the benefit of visiting scientists, friends, and prospective students. Tours provided a way in which my inquiry into lab work was accommodated in a routine way, though this format was only available during my earliest visits.

Tours were initially encountered as an ethnographic convenience insofar as they provided a condition of compliance with my interests in observing the work, conducting interviews with lab members, and receiving explanations about the lab’s program. During these early visits a lab assistant would go through his working routine while fielding questions about its details and
extended relevances. Occasionally, during breaks in continuous tasks a member would show me pieces of equipment, exhibit some microscopic slides, give general instructions about the inquiries the lab engaged in, and relate anecdotes on aspects of the lab's social life and on more general scientific issues. During these tours lab members showed a willingness to answer strings of questions which I asked about what they were doing, and often their answers occasioned further questions about the terms used in the answer or about implications the answer touched off. In a certain sense these tours were recipient-directed, as the version of the work that I was given relied in part on the sorts of questions I asked. This became quite apparent later when I observed tours given to visiting scientists in the brain science field, as those visitors asked questions and received explanations on an order of technical detail beyond my rudimentary competence at brain science talk.

Tours had only a temporary relevance for my inquiry, since I was honored with a tour only during the first few of my visits to the lab. After that, my efforts to receive explanations for questions about the work met with much greater difficulty. While hard at work, members often showed impatience with questions about what they were doing, and sometimes asked me to "hold off" until they were finished. In those circumstances the work was organized in a way that did not specifically provide for the relevance of "interviewing," and questions needed to be managed in a way that was sensitive to the temporal organization of shop work. I sought moments for approaching lab members when they could not use the visibly ongoing character of their work-involvement as grounds for avoiding answering. Furthermore, my questions, in order to be answerable, required at least a mocking-up of an instructional context, where I would convey an interest in learning the practical details of the work presumably as a preparation for engaging in it. (Naturally, my identity as a social scientist was managed in such a way as to allow certain rights for asking "peculiar" questions in the absence of any visible practical rationale. This license was rather limited in its relevance, and members showed very little regard for it when intensively engaged within their shop practices and talk with one another.) Structural aspects of "interview" talk — strings of questions and answers, "pursuit" of questions, prevalence of "explanation" — could not easily be insinuated into the lab setting once the relevance of tours had passed.

My ethnographic experience with tours is mentioned here not as a way of expressing relief over how nice it was that scientists accommodated an ethnographic inquiry with a routine indigenous activity. Tours are not recommended here as a convenient way of getting information; instead, they are provided as an organizational feature of the lab which was observed to be a locally occasioned setting for talk about science. The version of science, and more specifically of the particular lab's inquiry, which I received in the tour format was not so much an adequate "description" of science for use as ethnographic "data" as it was a "recipient-designed" account or story in a peculiar interactive format. What is interesting about tours is that they provided a kind of talk which is characterized here as "talk about science," and this talk was in many ways distinct from the talk which accompanied the work of doing science projects. Tours provided a kind of discourse which was certainly of importance for the relation of the lab's members to various visiting agents, competitors, potential students and colleagues. However, members' "shop talk" was managed as a continuous part of the work of doing science, and its relevance to a lab inquiry is not addressed in detail through any tour description. That relevance was demonstrated on the scene of an inquiry and in the details of the unfolding work.

Several instances of tours were observed in addition to those given for my benefit. Tape recordings were made of three of these tours, and audible portions of the tapes were transcribed. Interference from background conversations prevented a clear hearing of two of the tapes, though selected portions of the remaining tape are analyzed in Chapter 7.

Tour conversations showed a preponderance of question-answer sequences, with the visitor asking the questions and receiving answers from one or more lab members. Questions were not always of the "information-seeking" variety, and often took the form of challenges, or requests for accounts of uncertain or controversial aspects of the lab's claims. Available on the tapes were challenges over an expressed interpretation of some micrographic photographs (in which the visitor took issue with the lab members' claim that a particular kind of axon terminal was exhibited), and over the veridical character of microglia. In the latter case a visitor challenged a lab member's use of the term "microglia" in explaining axon sprouting. The visitor mentioned the controversies that reigned in the anatomical literature on whether microglia were a separate cell type. Questions of a "how do you know?" or "how do you determine?" character occurred frequently in tours, and in some circumstances could be heard as challenges, while at other times they seemed to constitute relatively ingenious requests for explanations. These questions were honored with answers which varied in the way they displayed an attempt to formulate an explanation vs a dismissal or undermining of the question. Different visitors were shown courtesies in these answers.
AGREEMENT IN LABORATORY SHOP TALK

depending perhaps on the initial sense of importance the visitor carried for lab members, and more immediately on the manner in which the visitor's questions were asked. Sharp challenges in one instance got an abrupt dismissal, while in another instance a series of challenging questions phrased in an "I don't know, but . . ." fashion received extensive explanations and a backing down from an initial claim.

Although these matters will be discussed in greater detail in Chapter 7, it can be mentioned here that tours showed more explicit manifestations of disagreement than did shop talk amongst lab members. Some tours ran off as defenses of the lab's program in the face of the questions and challenges of a visitor. Visitors' questions and challenges assumed competent access to the lab's researches and thematic phenomena, while lab members sought to defuse the challenges through recitations of a more detailed account of what members counted as fact or strong conjecture. In shop talk, on the other hand, the details of the work were often presupposed or relied upon as visible features of a work-scene, and the talk proceeded with less recourse to explanation about such details, except under certain conditions of trouble. Particulars of newly produced data were sometimes defended in the face of questions by collaborators, but these defenses usually involved detailed matters within the work, rather than recitations of the general commitments of the lab's research program. In sum, conversational displays of "skepticism" seemed more accountably part of tours than they were of talk among working colleagues.

2 Research reports

Written accounts which purport to describe scientific work are by no means limited to the science studies literature, as mentioned before. Science is in large part accountable in writing as an institutionalized feature of academic careers in a bureaucratically ordered array of "disciplines," and in terms of the relations between inquiries and granting agencies. Studies of science have seized upon that availability of writing as a way of grounding general claims in recitations of examples from written accounts and retrospective interviews and recollections.

A more recent interest has developed in some sociological studies of science for using scientific writings as other than unproblematic descriptions of scientific work and natural phenomena. These studies focus on scientific accounts as the phenomena of interest, and attempt to demonstrate ways in which the writing of such accounts: (1) is constructive in the way sequences of procedure are reported in contrast to "actual" sequences of performance; (2) is "rhetorical" in the way a sense of objectivity is conveyed through the use of a distinctive syntax, a choice of terms for thematic entities, and an enlisting of authoritative and socially ascribed-to manners of formulation; and (3) is relativistic in the way the "same" events involved in a discovery can be described differently with different consequences. These studies are suggestive of the possibility that certain formal characteristics may be definitive of scientific textual discourse, and that the identification of Science with, for example, logical rigor, independence of findings from the observational process, "disinterestedness," and reasoned argument, may arise out of a confusion of a set of ideals embodied in the form of written accounts with the governing conditions of actual scientific practice.

A somewhat ironic sense of particular scientific accounts can be achieved through a reading of those accounts as written constructions which seek to present results in such a way as to minimize ambiguities in the accountability of the "world" studied. For instance, a conventional passage from the methods section in a given research report can be analyzed for regular textual features which rhetorically emphasize the "objectivity" of the text's claims and descriptions. Such a reading is relevant in the context of (1) institutional pressures to turn out significant and objective findings as a condition for the survival of an academic career and research program; and more immediately, (2) the inevitably constructive involvement of language in the field of activity it "describes." Take for instance, the following passage from a report of the "ultrastructure study":

Unilateral electrolytic lesions of the entorhinal cortex were performed on adult male Sprague Dawley rats. Following post-operative periods of two (n = 5), four (n = 3), five (n = 4), six (n = 2), seven (n = 2), nine (n = 2) and eleven (n = 3) days, animals were sacrificed under nembutal anesthesia by intracardial perfusion utilizing a mixed aldehyde fixative solution . . .

To begin but the most superficial analysis of the above account, I will point out the following features:

(a) The "missing agent" – procedures are described in a way that makes no reference to an agent producing the work. No personal names (as would be the case in "unilateral electrolytic lesions were performed by Harry, George and Betty . . .") or personal pronouns are used in the formulation of the procedure. Agency is left implicit, and if we read the passage literally we get the sense that a "lesion" or "sacrifice" is accomplished through an
autonomous technology freed of the vicissitudes of human agency.\(^{10}\)

(b) Passive description – the use of the passive voice ("were performed," "were sacrificed") contributes to the sense of the absent, implicit or irrelevant agent in the description.\(^{11}\)

(c) Generalized sequencing – the account gives a single descriptive sequence for the procedures accomplished on twenty-one separate animals. The animals are distinguished from each other in terms of their membership in "day" cohorts (the list of "n's"), the separate groupings of which received distinct treatment. The description of the twenty-one cases in the above passage treats them as a collection governed under the standard of a single set of operations rather than mentioning that each "n" was the outcome of a unique, and in some ways non-identical, sequence of action. This economical format for describing twenty-one separate lesions on twenty-one distinctly marked animals ascribes an identity to each performance of the lesion, without regard to the person performing the lesion or the particular animal lesioned. Evidently, the lab reserves for itself the problem of determining whether such standardization holds for any particular case of a procedure's performance, leaving the adequacy of such determination presupposed in the way the account is written.

This way of exploiting the written text is not conceived here as the most promising way of apprehending scientific writings. Instead, the following alternatives for the analysis of scientific writing are recommended. Although written records have not been extensively analyzed here, some conjectures are suggested from observations of writing's involvement in a research program. When considered as interior features of the various courses of action that make up a laboratory inquiry, written records become available in a different sort of way than when viewed as isolated texts. Instead of analyzing such documents for their grammatical and literary features, or utilizing them as adequate descriptions of instances of science, one can: (1) show how specific instances of writing occur as part of a sequence of actions in a laboratory project, and (2) show how written reports, photographic displays, notes, and recipes are employed by lab members as materials in their performance of lab work.

The first recommendation locates writing as a distinct phase of an inquiry, along with those phases in which shop talk and embodied shop practices are involved. Writing up research reports for publication relies upon the production of an inquiry as a condition for the appearance of a document. The document not only occurs sequentially after the work of performing an ongoing course of work, it also acts referentially as a "closing" of an inquiry; it provides for itself as a retrospective report of some completed and definite course of work. Although from one perspective the work of a project is never completed, from the point of view of a written report, a definite project is constituted with a beginning, an organized course, and a completion. This temporalization is not isolated in the format of a written document, since the observable seriality of the work itself is oriented to its eventual "writing up."

The second recommendation is grounded in the observation that documents (either published research articles or documents constructed by lab members as materials in a project) are integrally a part of the work of doing science, but not merely as theoretic precedent or as sources of ideas and inspiration. For instance, a common sight in the lab studied was a lab member working on a technical task with a stack of research articles, opened to relevant pages, placed at the work area. The papers, particularly their "methods" sections, were consulted in light of the developing work situation, as instructions, remedies, suggestions of what to "look for," and what to "look out for," in the developing result. The "methods" accounts in the research articles were combined in ways unique to the current project. For instance, a chemical concentration for a buffer was taken from one account, a temporal sequence of staining instructions taken from another, and exemplary photographs of a thematic anatomical entity was used from still another article. The detailed ways in which specific instances of shop work employed research articles were not examined in this study. Access to such an order of detail could perhaps only be achieved by a competent practitioner's analysis of videotape records of specific shop tasks. At this point, I can only say that the way research reports are read during lab projects is not by an isolated consultation of a continuous textual account. The "materials" of such a textual account are likely to take on remarkably different kinds of sense, definiteness, and form depending upon the character of the developing task.

The "writing-up" of research was an activity which was never directly observed in this study. Writing was done by the several members of the lab who were involved in the ultrastructure project, though the lab director had control over the writing of the final draft. Writing seemed to be a relatively "secretive" activity which was reserved for those who were credited as good writers. However, there were indications that writing emerged out of consultations among the persons involved in the production of an inquiry, and in some instances may have been an activity which was observable in the way that sequences of conversation are produced as witnessable events.\(^{12}\) Since these sessions were not
observed in this study, the topic of the collaborative organization of writing within the temporal organization of a project cannot be further developed here.

In the matter of how written accounts were involved in the performance of shop tasks, it was notable that some lab activities were described to me as accomplishments which were not amenable to written instruction. That is, in some cases a written methodological formulation was said to be inadequate as an account which would allow a reader to perform the task competently without prior training in the specific lab that wrote the account. This was more than a matter of the instructions being inadequate for someone who was untrained in the specific sub-discipline, as the inadequacy of the written accounts was said to apply to highly skilled practitioners in brain science from other labs. The tasks for which written formulation were said to be inadequately instructive were those which had been developed within the specific lab's researches. One such accomplishment involved the implantation of a tiny-diameter glass electrode into the cell body of a small type of neuron. The electrode was a hollow tube which contained a solution of ions which could be electrically charged during implantation of the electrode in such a way as to enable the ions to flow into the cell and bond with the cell membranes, thereby acting as a stain. Inserting the electrode into the body of the cell in such a way as to avoid bursting the cell was said to be an extremely difficult task which had not been accomplished in other labs. Another accomplishment which was similarly inaccessible to outsiders was the use of media chamber apparatus which kept excised “slabs” of hippocampal tissue electrophysiologically viable for periods of time up to twenty-four hours. The construction and management of the chamber apparatus was claimed as a unique accomplishment by lab members, and no other laboratory was credited with the ability to keep in vitro tissues “alive” for as long a period of time. For such tasks, it was said that no amount of written formulation would assure that other labs would achieve the same results. Often, when such methodological skills were transmitted from one lab to another, it was through the medium of a visiting post-doctoral emissary who would be trained in the skill and could then train colleagues in his original lab. I was told that possibilities for “hoaxing” results resided in the inability of some “test systems” to be competently designed and managed when accessed only through the medium of written accounts. If a lab could claim to possess such a test system, results achieved through its use could, for a time, be similarly inaccessible to replication or “falsification,” and fake results would not be readily detected. Similarly, controversies over discoveries often persist in the literature when the discoverer claims that unsuccessful attempts to replicate the original results by other researchers are due to the inadequate design of the replicatory apparatus.13

Troubles attributed to written accounts did not apply to all laboratory procedures. Some procedures were said to be unproblematically available in written accounts or recipes, such that whatever troubles were involved in performing the method in a particular instance were attributed to incidental circumstances rather than to any irremediable characteristics of the account’s gloss of the actual research.

C “Talking science”

Scientists’ uses of discourse in their work activities are not limited to the so-called descriptive functions of language. Instead, language provides the materials for a comprehensive set of actions which include description as a constituent accomplishment. This comprehensive range of actions, which I will call “talking science,” or “shop talk,” is an integral part of the ordinary work of doing science. As such, it is not to be treated as a casual or leisurely activity as, for example, idle chatter during coffee breaks, or as talk which relieves the boredom of routine tasks which can be done “without thinking.” Although much of the talk that occurs in the lab is of such an “idle” character, “talking science” is talk which is directly part of the collaborative achievement of inquiry. Such formulations as “agreeing/disagreeing,” “requesting,” “noticing,” “proposing,” and “assessing,” allude to some of the particular ways in which discourse accomplishes work. These actions are inseparable from scientific actions when they occur in the context of collaborative laboratory work.

“Talking science” as discussed here is not limited to the use of utterances in a spoken language, since the interactions between scientists utilize such non-vocal materials and formulations as written equations, notes, illustrative or analytic diagrams, electron micrographs, oscillographic data outputs, and styrofoam models of lattice structures. An equation, for instance, can be written or erased as an interactive “move” on an occasion of talking mathematics around a blackboard. As such, the equation has a particular significance within the unfolding social situation which is not fully recovered in examinations of verbal discourse alone.

The importance of “talking science” for the emergence of a scientific discovery is clearly shown in James Watson’s autobiographical account of his and Francis Crick’s discovery of the molecular arrangement of D.N.A.14 Watson describes how the
two authors of the discovery arrived at their model of D.N.A. after countless sessions where they tried out theoretical possibilities on each other, argued out these possibilities, worked together on assembling a wire and plastic model of the molecule, and interactively integrated their separate competences into a coherent account. Although the actual sequences of talk and action which occurred during these sessions are only alluded to in the book, it demonstrates that the discovery was an emergent phenomenon which depended upon the interactions between two persons, neither of whom would have arrived at the discovery alone. Other interactions were involved in the circuitous route to the discovery such as those between persons in different work groups who were attacking the "same" problem. The book provides a lucid scenario of the discovery as a complex social event which markedly contrasts with the description of the discovery as a rational course accounted for. The book invites a more detailed investigation of the interactive setting of scientific work, though in the decade since its publication very little has been done to follow up such implications.

Although sociological studies have largely ignored the collaborative setting of scientific discovery, some suggestions can be found in the literature that implicate shop talk as an interesting and significant phenomenon. Research by Kowarski (1965) on trends in authorship of scientific publications shows that joint authorships have been on the increase in twentieth-century sciences. On some publications large numbers of authors are listed, and some research teams have used institutional titles in lieu of personal names. This indirectly suggests that doing science in modern laboratories entails a collaboration of "specialists." Although the organizational ways in which such collaborations are managed in the laboratory situation are not specified in Kowarski's paper, it is clearly implied that any notion of scientific discovery as a solitary, psychological, and unobservable activity may not be very applicable to the modern situation. Instead, discovery may be appropriately treated as a social product of the actions, talk, and institutional arrangements in a lab. As such, the possibility arises that the making of a discovery may be witnessable and analyzable through a detailed study of its emergence in the talk of lab members.

Garvey and Griffith pay explicit attention to "informal communication" in a specific scientific work setting, and elaborate a list of functional distinctions between "informal" and "formal" scientific communications. They characterize informal communications as being loose, casual, often wrong, not carefully qualified, and based on conjecture. Formal communication, as exhibited in scientific publications and public speeches, is described as showing a more familiar set of "scientific" features such as rational argument, carefully constructed experimental design, and the tempering of argument with empirical observations. Garvey and Griffith rationalize the "loose" character of informal communication by pointing to how it is a necessary prerequisite to more careful discussion and experiment. They provide for how informal conversations allow scientists to try out ideas on one another and receive feedback on their proposals without risking much prior investment of time and facilities. Despite the suggestiveness of their account, it fails to point out how shop talk is more than a preliminary phase of research. As discussed in this chapter, shop talk is involved in all phases of laboratory research: from the initial conjectures about a problem to the detailed working-out of its experimental or observational demonstration. Insofar as all phases of experimentation are collaboratively achieved, the talk among collaborators is a key component of the scientific work in a relentless fashion.

The ethnographic analysis of scientific shop talk proved to be a very difficult matter. Although, as stated previously, shop talk showed familiar features of ordinary conversation, a competent "hearing" of the talk required a comprehension of the lab's specific research into axon sprouting. Because of this, analysis of scientific talk in terms of such conversational "structures" as agreements, assessments, proposals, noticings, and claims failed to account for how shop talk was distinctively part of the lab's inquiries. It was unsatisfying merely to state that shop talk was an "adaptation" of conversation in an unspecified "scientific context" since the critical matter was how that "context" was visibly part of any particular occasion of "talking science."

This problem was compounded by the fact that whatever made laboratory talk distinctly part of scientific inquiries was the least accessible feature of the talk under my analysis. The accessibility of the "technical" talk was limited by the extent to which I had participated in the lab's research program. When the talk of lab members was not specifically attentive to my lack of expertise, I found it impossible to overhear sensibly. My comprehension was considerably aided when I could play back tape-recorded instances of shop talk since I could painstakingly work out understandings of the initially baffling aspects of the talk. Detailed analyses of these particular transcripts are presented in Chapter 7. For those conversations that were overheard without the benefit of tape-recording, the "technical" character of the talk was mostly opaque in its specifics, except when it dealt with the electron microscopic facets of the lab studies that I knew best.
In the remainder of this chapter I will lay out some loosely organized "features" of shop talk's phenomenological "strangeness" from the point of view of a relative novice to lab practices. Although such an analysis is deficient as a way to characterize the competencies which were productive of the talk, it is of some provisional value to point to how the sensibility of the talk was local to its productive situations. These preliminary formulations of shop talk are presented under the following headings: "ethnographic strangeness," "quasi-familiarity as conversation," and "inaexpressibility from work situations." Although these characterizations of scientific shop talk do not adequately specify the phenomenon, they indicate properties of scientific discourse which are unavailable in the above named "literary versions" of science.

1 Ethnographic strangeness

Laboratory shop talk was "ethnographically strange" in the "reticence" of its sensibility to a non-practitioner of the lab's research projects (such as myself when I began my inquiry). Except when presentations of lab work were given in the explanatory format of a "tour," shop talk was produced for recipients who were already familiar with the technical details of the ongoing work. Talk which occurred between researchers while they were engaged in doing projects was not "strange" in the sense that it contained peculiar vocabulary items from the standpoint of my "lay comprehension." It did sometimes include technical terms for anatomical features and laboratory equipment, but examinations of recordings of shop talk showed surprisingly few of these terms, especially in light of my prior hearing of the talk as utterly baffling. In addition, after a short period of time in the lab, I came to recognize such terms as "microglia," "dentate gyrus," "epon araldehyde," and "horseradish peroxidase" as familiar references. Despite this, shop talk remained difficult for me to "decipher" during any "first hearing" of a conversation, prior to any examination of tape-recordings of the same talk.

In reading accounts in the sociology of occupations such as Becker's discussion of the culture of jazz musicians, one can get the picture that members of a "subculture" invent strange vocabularies as a way of preventing access by outsiders to the conversations of "initiates."19 Scientists' shop talk, as it was observed in this study, was not a "jargon" in that sense. Its "opaqueness" to a non-member was more a matter of the way in which speakers used fairly common terms of American English vocabulary implicitly to refer to a distinctive environment which members relied upon as a common set of circumstances for their talk. Accordingly, references to the materials of shop practices and the "place" of those practices within a continuity of the setting's presence needed no further specification or explanation for members than was supplied in pronomial and other implicit forms of reference. The "strangeness" of shop talk was not primarily realized as a matter of esoteric vocabularies, nor was it attributable to any exclusionary intent (as in Becker's account of musicians' ways of providing for the difference between themselves and "squares").20 Although scientific talk and writing may often be unnecessarily "thick" with technical "jargon," my encounters with lab shop talk were problematic in a way that would not have been remedied through any injunction to "speak plainly."

2 Quasi-familiarity as conversation

Shop talk was familiar as American conversation in a work setting. However, that familiarity and the formal features of conversation which could be identified analytically in the talk were insufficient as a basis for an adequate hearing of the talk. By "adequate hearing" I mean an access to conversational utterances which allows participation in the talk by the hearer (participation that is competent in the way it locates appropriate junctures for speaking, addresses current topical talk, and does not require special work by other parties to include one in the conversation). Shop talk did not occur in any special scientific language; it was accessible as ordinary conversation on topics of local relevance to lab members. Indeed, analytic efforts on my part structurally to isolate instances of "shop talk" from ordinary conversation which occurred in the lab setting were frustrated. Although I have characterized shop talk as talk between lab members which occurred concurrently with the work of designing, conducting and reviewing lab experiments, "casual conversation" occurred in such circumstances as well. I have attempted to locate shop talk as talk which was not only produced "alongside" lab activities, but as talk which was integral to those activities. However, making such a distinction in all cases was difficult. Tape-recordings of conversation in the lab setting showed remarkably rapid transitions between hearably "scientific" topical talk and talk about such matters as, for example professional baseball players, current movies and movie stars, and local evangelists. Furthermore, "scientific" talk was often received in a "non-serious" fashion, and thereby transformed into an occasion for "play." In one instance, a request for some capillary tubing was turned into a series of jokes, which were touched off by the peculiar way in which the Australian post-doc pronounced the term "capillary."21 On the other occa-
tions, jokes and playful manners of speaking were achieved within accountable shop talk, as when the lab director asked in a loud falsetto, “IS THIS AXON SPROUTING?” while looking at some micrographs. The remark could be heard as a playful insert in the ongoing work of examining the micrographs, but could equally as well count as a way of bringing up a topic for discussion.\(^{22}\)

Clearly “irrelevant” topical talk which occurred among researchers at work showed a degree of integration into the work despite its “non-scientific” topicality. When, for instance, persons were doing routine tasks, such as slicing brain tissues on a microtome, they often spoke to nearby recipients on clearly extrinsic matters. This was not the case in other circumstances in which the work was explicitly attended to in the topical talk. Because of the variation in the work circumstances that permitted or discouraged such topical inattention to “what the hands were doing,” irrelevant topical talk was integrated with the scientific shop work. The ability to disengage the talk from the work at hand varied from task to task and from one practitioner to another for the same tasks. It was also dependent upon the immediate interactive environment, since practitioners sometimes paused in the midst of ongoing tasks while being “absorbed” in conversation, whereas at other times an extreme attention to the ongoing work was usable as an “excuse” not to talk to, for example, an ethnographer’s request for an explanation. This precluded any notion that “irrelevant” talk was not structurally tied to the present work situation.

There did not seem to be any clear-cut structural ways in which accountable “scientific” talk was “keyed” in lab conversations. In some instances, shifts in voice intonation and pitch accompanied topical shifts to and from work-related matters. It was also the case for some conversations that extrinsic topics tended to touch off other extrinsic topics (as in a long series of stories about different movies, followed by talk about Barbra Streisand in one conversation), and talk addressed to the local work scene also had a certain topical integrity. However, these features were not invariable. Voice shifts, topical boundedness, and other “markings” were not especially definitive of shop talk or “extrinsic talk.”

Talk in the lab setting was variously accessible to my understanding; with some conversations appearing as opaque technical talk, while others concerned matters which were not local to shop work. I could easily engage in conversations on topics such as university politics, popular movies, and sports. Gossip about unknown persons was accessible to me as overheard gossip. At times, however, talk among lab members on non-scientific topics was inaccessible in the way it implicated activities which lab members had done or witnessed in common. Such talk employed pronominal references in ways that made participation in the conversation impossible for me, since I could only vaguely, if at all, “follow” these references in common with other speakers.

For the above reasons, I have not attempted to define a specific criterion for how “shop talk” was a clearly demarcated “kind” of conversation. Instead, my analyses of shop talk are limited to specific instances of conversation which were clearly involved in the work setting as part of that work’s local production. Such talk was not produced during “coffee breaks” or other leisurely moments in the lab, as it consisted of actions within specific situations of shop work. I shall discuss some properties of the instances which were examined, but will not refer to these features as criteria which define a structural demarcation between scientific and “common-sense” discourse. Whether or not such a definitive characterization is possible awaits further inquiry.

Shop talk exhibited a full range of structural features of conversation which have been elaborated in the work of Sacks, Schegloff, Jefferson, Pomerantz, and others.\(^{23}\) It was produced in turn-by-turn talk complete with request, announcement, agreement, disagreement, and repair sequences. Not surprisingly, shop talk was structurally congruent with “ordinary conversation” in its sequencing, “preference” systems, topical progressions, and use of vernacular terminologies for most referential tasks. Indeed, shop talk was ordinary conversation. However, some particularities in the way shop talk came out of the work situations contributed to its ethnographic peculiarity. These particularities are elaborated in the following section.

3 **Inseparability from work situations**

The sensibility of laboratory shop talk was achieved as a situated relationship of the talk in the unfolding details of laboratory projects. The “ethnographic strangeness” of the talk consisted in its being irretrievably part of the detailed and ongoing course of lab work as it was witnessed and produced by parties to that work. One standing feature of shop talk, as it was witnessed in the present study, was that the talk was very frustrating to understand in isolation from a video record of the setting, since many references were made which relied upon the presence of, for example, electron micrographs along with other features of the work’s visible accountability.

This is not to say that the talk was *secondary* to the “silent” visibility of the work environment. It is more accurate to say that shop talk occupied a temporal location within the ongoing
production of an inquiry, for example, the examination of electron micrographs, or the placing of chemicals into solution for a stain preparation. In such environments such utterances as requests for materials, or assessments of outcomes of current work, relied upon the witnessable character of the "things" as an environment which the talk specified. The hearing of a "request" or an "assessment" as an accountable feature of that environment relied upon a hearer's competence with, or sensitivity to, the temporal occurrence of the utterance in an unfolding scene of organized shop practices. How the work provided a context for shop talk (a context which was identical with the talk's sensibility) will be presented here under the following headings: "silences in incipient talk," and "implicit references." Both analytic features of audio-taped "talk" implicate the talk's placement within the conditions of a currently visible work scene and an implicit and local lab history.

(a) Silences in incipient talk
As stated previously, laboratory shop talk showed a full range of "ordinary" conversational features. In one respect, however, instances of shop talk which were examined here were unusual in their systematic turn-taking. Sacks, Schegloff and Jefferson formulated a "no gap, no overlap" rule for turn-transition in ordinary conversation which was said to govern the serial placement of utterances by speakers in conversation. In shop talk, however, "gaps" appeared frequently without being made vivid as "absences" of talk. Numerous, rather lengthy gaps appeared in many instances of shop talk and were not marked by speakers with repeats, repairs, or other remedial treatments. Instead, sequentially relevant talk occurred across lengthy gaps, and isolated utterances occurred within the conditions of continuous silence, as in the following instance:

H: Two en a half, huh
(0.4)
J: Yeah
(3.5)
J: I think there all ss-similar to this
(8.0)
H: ((high pitched)) hnn hnn hnn hnn, hnn hnn hnn
(3.0)
J: ((voiced out breath)) Hhhooooey
(6.0)
H: How many will this give us at two en a half

In the above sequence, both speakers display a continuity in the topical orientation of their successive utterances and produce utterances which are sequentially tied to prior utterances by the same or the other speaker. This achieved-continuity holds even for the voiced outbreath by speaker J, whose utterance is "matched" to the prior non-lexical "humming" sounds of H. These continuities are achieved across lengthy silences while the speakers are examining electron micrographs. The talk refers to the micrographs and their analytic features, and occurs as part of the work of analyzing the micrographs for their relevance as data in a project.

The "silent" work of analyzing the micrographs is implicated in the talk which brings that work into further collaboration, and is not available in the talk as an absence of activity. Instead, the work of looking at the micrographs and marking visible features of the photographs is available as a continuous order of common activity out of which talk by involved parties arises. Under such conditions, silence in talk shows a different significance than, for example, silence in a casual telephone call insofar as talk does not display a primacy over silence as a condition for accountable activity. In conversation, silence is accountable as an absence of talk – as an avoidance of, refusal to, or distraction from talk. In the context of ongoing, mutually produced-and-witnessed work, silence is not necessarily significant as a turning-away from talk. Instead it can be available as a continual and collaborative preoccupation with a silent production. Such situations where talk proceeds from within the conditions of silent preoccupation are identified in conversational analysis as "incipient talk," and are seen to be present in a variety of work settings.24

Incipient talk, a variant of natural conversation, is said to occur in various occupational settings where members engage in tasks which are not exhaustively constituted in members' talking to one another. Examples of this are "manual labor" on assembly lines and clerical paperwork. In the lab setting, incipient talk occurs during varieties of technical tasks in preparing slides, assembling arrays of photographs, or titrating solutions of chemicals. These tasks are not performed in a setting of unremitting silence, and utterances such as requests for materials, situated instructions, and assessments of current work are actions which emerge from conditions of silent productivity. Incipient talk is characterizable as talk which emerges when a silent task provides members with a "legitimate" alternative to the social injunctions involved in turn-by-turn conversation.

The strength of this alternative – of the silent preoccupation with ongoing work – was encountered ethnographically as members' unavailability to my questions and requests for explanation. A repeated source of trouble in my attempts to ask lab
members about what they were doing was the fact they sometimes avoided answering by showing their engrossment in their current work. In response to this trouble I developed a sensitivity to the sequential organization of work and sought out phases of that organization which enabled entry into a situation of talk. Examination of interview tapes revealed that I unwittingly used such devices as "noticings" about a practitioner's current work situation as a way of inviting talk by the worker. In organizing interviews in such a way, I used the devices of incipient talk in the service of an ethnography. Only later, when the tape-recordings were repeatedly examined, did it become apparent that the "science" which was exhibited in the interviews was not so much a referent of elicited talk by the interviewee as it was an implicit facility (or in some cases conspicuously unsuccessful attempts to mock-up such a facility) with the devices and referential matters of talking shop.

It was surmised that the use of the silent visibility of tasks at hand provided members with alternative displays to the injunctive character of turn-by-turn talk while they worked in each other's presence. This phenomenon was analytically detected as a "suspension" of "no gap" constraints on turn-by-turn talk in the occupational setting. A "gap" in recorded talk could not be construed as an absence or interruption of accountable activity, as it would be in a setting where conversation defined the appearance of warranted activity. Instead, talk found its relevance within an order of activities which were not initially characterized by the occurrence of talk.

In shop talk, long pauses between utterances did not seem to work against the orderings of adjacency described for ordinary conversation. Questions received answers and assessments received second-assessments, though interspersed with lengthy silences. The visibility of the work seemed to provide for the relevance of waiting, subsequent to a first utterance, while the materials were worked to a point where a second utterance was produced in relation to the task's temporal provisions. For instance, remarks, assessments, or noticings about a photograph's exhibit of dendritic material provided for the relevance of recipients' looking at the materials to find what was said and what could be said in return. This collaborative use of the work-materials showed-up on tapes as silences between adjacently produced remarks:

J: (See)  
(1.0)  

J: There's- w'll there's vesicles behind there and there's a site somewhat to- there, there  
(1.0)  
J: Maybe not even in that plane, there's vesicles in there,  
(0.6)  
J: An' there's a degenerating thing there ((sniff)) more an' more yeh see that.  
(1.5)  
H: Yeah (nice isn't 't) these fuckers 'r rilly small.  
(right)  
(1.0)  
H: Yeh know that?

In the above fragment, J proceeds with an account of a photographic display, while pausing several times during the account. H responds to the account after another pause and locates his utterance in the topicality of the display. The pauses are relatively long in their clock-time duration but are inferentially available in the excerpt as parties' work of finding particulars in the photographs, as those particulars are conversationally invoked. Silences between adjacent utterances may have other analytic significances as well. For instance, "delays" of various sorts which occurred on tapes may have been a matter of the organization of agreement, where "dispreferred" accounts are produced after some delay. The analyzability of "gaps" in shop talk was thereby problematic in some cases, as it was not always clear if a gap should be "heard" as an incidental feature of the work of examining materials, or as a specific conversational "device." Perhaps it was the case that such ambiguities were exploitable by parties in shop talk as ways of "withholding" or "delaying" agreement- or disagreement-relevant assertions and assessments while using the "cover" of a studious orientation to a current shop task. At present, however, this is a conjectural matter.

Regularly appearing silences were not uniformly characteristic of shop talk. Instead, conversationally paced interaction often emerged from incipient talk, with lengthy silences appearing most often at temporal locales where such conversations were in the process of emerging from or reverting to silence. Incipient talk appeared to be equivocal as a way of beginning a conversation since remarks such as "noticings" (e.g. "Two en a half, huh") could touch off topical talk, or could be left as isolated utterances by hearers in an environment. (These hearers were not necessarily recipients in all cases since "out louds" or "noticings" in incipient talk were not always directed to specific recipients; often they were directed to anyone who might care to respond.)
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(b) Implicit reference

Referential items in shop talk were produced with an implicit regard for the familiar, known-in-common character of the work's features as a day-to-day set of circumstances. Without that implicit basis, references were heard as pronominal, vague, and calling for further elaboration. The fact that the talk could be characterized in such a way was more a matter of the analyst's (in this case, the author's) disengagement from the practical environment than it was of any specific features of the talk as an accountably heard phenomenon.

As stated previously, the "lay incomprehensibility" of shop talk was not primarily a matter of a distinct scientific vocabulary. More consequential was how lab talk arose from within a taken for granted setting of common experiences which were not available to an outside observer. These experiences included not only the historical course of researches that members had accomplished in their work, but also names and accounts of objects, persons, and events which were not directly featured in the context of research. These were referred to in stories, jokes, references to past and upcoming events, and talk of non-present persons. Insofar as references to persons and events were designed for recipients who were already familiar with who and what the speaker might expectedly know, the references contained no obvious clue for a naive overhearer to locate their sense in a presumptively shared order of affairs. The details of involvement were essentially missing in "overheard" accounts of shop talk and could not be supplied episodically. Access to the talk, as conversation in American English, was adequate only to a rather shallow appreciation which offered an insufficient grasp of how a recognizable joke might be funny, or what an appropriate assessment of a reported story might be.

A way in which implicit reference was visible in recorded shop talk was as the occurrence of *pro-terms* in conversation. These include such references as are called "pronouns" (he, it, this, those, etc.) and "*pro-verbs*" (such as "did" or "go" when used in a non-specific context). They also include such items which I shall call "pro-names," because they use a generic name to refer to a specific member of the class of objects it names. (Formulations such as "home," "the car," "the lab," or "the electron microscope" can be accomplished so as to refer to a specific home, car, lab, or microscope without the use of the possessive, proper naming, or other specific references.) My inability to comprehend scientific talk was in large part tied to the manner in which pro-references were used for phenomena which I could not locate in their use. For me they were not "labels" for "things", but invoked a taken-for-granted co-temporaneity that was extrinsic to my understanding.

In a way it is misleading to talk of *pro-terms* as non-specific references. In the way in which they were used they accomplished definite reference and were clearly understood by those in a position to hear them as implications of a practical environment. Their problematic or "vague" character was only present in the observer's disengagement from the productive scene. The use of such referential items in shop talk accomplished definite reference and was clear speaking, regardless of any grammatical contrasts which could be drawn between shop talk and "formal" scientific discourse.

Another class of terms which were used in shop talk can be described as those referential items that were recognizable as common vocabulary items in lay discourse, but which were "strangely" used by lab members. Such terms were identifiable as technical terms, but not through any formal distinction between them and common names in vernacular speech (as would be the case when *Latin* names are employed in scientific naturalism). Terms such as "animals" and "days" occurred in lab discourse sometimes as rather unremarkable references from the standpoint of lay intelligibility, while at other times they were used in such a way as to identify them as particular indices of axon sprouting data. Within technical talk of axon sprouting, these items indexed such things as brain fragments, cellular sections, photographs, and statistical averages of axon terminal "counts." These various renderings of animal brains were ordered in terms of their temporal and genetic conditions of similarity and difference arising from the identity of each laboratory specimen's particular career through a series of lab practices. Ordinary expressions were used in lay discourse as showings of an occupationally specific "world" of anatomical and physiological entities. Utterances from within that condition did not, in and of themselves, reveal the particular character of members' understandings of their work circumstances. Instead, utterances were implicitly part of those circumstances.

Conclusion

The preceding "features" of scientific shop talk pertain to the occupationally specific character of discourse between lab members. The characteristics of "ethnographic strangeness," "quasi-familiarity as conversation," and "inseparability from work situations" of lab shop talk do not distinguish *scientific* shop talk from other varieties of occupational shop talk. Technical talk
which is relatively inaccessible to non-members’ comprehension may be characteristic of varieties of settings where a distinct order of activities is performed in the course of speaking. It is not especially distinctive of scientists’ talk that it is understandable only when placed in the context of specific practical circumstances.

What, then, can be said of laboratory shop talk which distinguishes it as scientific talking?

Although I can offer no positive set of “structures” which defines scientific talk, we can begin to address this question by reciting a list of conceptions which were negated in this ethnographic and analytic study of laboratory shop talk:

1. the identification of a peculiarly scientific vocabulary as a condition for the appearance of scientific talk is not a necessary condition;

2. nor is any particular organization of discourse congruent with a distinctly scientific “logic” of explanation, description, assertion or argument (I will go into this matter more extensively in Chapters 6 and 7 in a discussion of structural aspects of agreement in scientific discourse);

3. nor do any formal grammatical structures appear distinctive of scientific shop talk. The aforementioned characteristics of “incipient talk” and pro-term reference were not distinctive of science as an occupational setting. Although for scientific writing, a particular style or “rhetoric” can be identified, oral scientific discourse does not exhibit such features of grammatical structure as have been elaborated for writing. Shop talk is not produced in a way freed from strong evaluative assessment, phrased in a passive tense without the use of personal pronouns, or removed from immediate circumstances. Occasionally remarks are produced in a way that is strikingly “professional” in the apparent phrasing of speech to sound like the written format of formal scientific discourse. Instances were observed of lab members (especially students speaking to supervisors) displaying “careful speech” in their formulation of their talk as accountably scientific speaking. However, lab members did not use this format often in their talk with one another and managed to deal with consequential matters in their work with extremely colloquial styles of speaking.

An unremitting use of a “written scientific format” in shop talk would very likely appear overly “stuffy” or “pompous” in the context of the widespread use of vernacular speaking practices in shop talk. No doubt some persons in science adopt a speaking style reminiscent of the conservative, objectivistic format of scientific writing, but indications are that such a style is not a necessary or even a preferred manner of speaking in laboratory situations.

Perhaps these observations of what shop talk is not are themselves a “finding” about science. Popular images of “the scientist” in movies, television shows, and writings about science often depict the scientist as coolly objective, detached, unemotive, scrupulous, and “stiff” in comparison to “ordinary folk.” In the present study, members displayed themselves as anything but “the scientist” of popular stereotypes. They did not wear white coats, speak in a low monotone, avoid rash judgmental expressions, or stand aloof from popular culture. To appreciate this point, consider the following conversation which occurred while the lab members were building a chamber for housing in vitro brain tissues:

A: ... I'm running shoo't on capillary tubing ((said in speaker's Australian accent))
H: Capillary?
A: ((Sup-won-) you khh'm w'd ye w la ek te go up see ‘f you c'n git some tyübing lak th f'mee
J: Sure
A: On the way back wawk up
H: ((play on name of lab director in another lab in the building))
J: Wha' hh's from whoom?
H: ((gives name of other lab director))
J: ((repeats name with question intonation))
A: ((play on name))
H: Go'um see ((name)) and see if ye can't ge- an, now whad're y' gon' ask for?
J: Capillary tubing
H: That's it.
A: Wh' its j'st cah plary tubing ... cäplary tubing, yeaw
H: Oh now yer godid.
J: Oh now 'e wans
J: Now he wants capillary tubing.
J: Cähpillary tubing [is
( ): [Make up yer mind.
H: (Pass it off) He's saying teh stop.
D: ((falsetto)) ding do [ng
H: ['Sides, iz neater teh say capillar
M. L.: Oh yeah thas vv' [professional sounding
D: ((falsetto) [Capillary . . . Mary, Mary capillary
H: ((refers to C’s haircut)) Haay thee cleaned up young Grubkoff
D: Yea h
H: Ats young Grubkoff?
Maintenance man: I wish you guys wuddn’ talk bad, ya’ burn my tender earz.
H: Oh yeah right? At’d be the first time.

The maintenance man’s remark in the above fragment is especially ironic given the stereotypic version of “the scientist.”

In any event, I have not positively characterized shop talk in any way that would mark it as essentially scientific in structure. In the next chapter we will explore the achievement of agreement in lab shop talk in an effort to specify whether scientific discourse shows distinguishing features in the way agreements are displayed, disagreements are resolved, and accounts are collaborated in specific neuroscience inquiries.

Notes
1 Representative examples of the kind of account discussed here are previously cited studies by Kuhn, Popper, Hanson, Holton, Hempel, Merton, and Feyerabend. These studies do not begin to exhaust the accounts of science which would support my remarks in this chapter, nor are they selected for any lack they may show under analysis; indeed, their excellence as historical and philosophical accounts of science is what makes them appropriate for our purposes here.
2 Philosophical accounts by Kuhn, Holton, Polanyi, Nagel, and Ziman are not only grounded in the scholarship of their authors, but are informed by the careers of the authors in physics and mathematics. It is rather common for prominent scientists to report “reflections” about science in a generalized and philosophical vein, especially late in their careers. Although such reflections often provide interesting accounts of science, they are markedly distinct from manners of speaking and writing which are part of the work of doing science.
3 Some of the classic cases which appear in science studies are the Copernican formulation of heliocentrism, Newton’s laws of force and motion, Galileo’s tower experiment, Einstein’s theory of relativity, the Michelson-Morley experiment, the development of quantum physics, and Watson and Crick’s discovery of the molecular structure of D.N.A. These cases tend to be used as perspicuous examples of “revolutionary” reconstructions of contemporary thinking and experience. Their use in science studies accounts has afforded an exquisite clarity to the illustration of how key events in scientific history have led to gestalt changes of massive proportion in perceptions of natural events.

The above cases are but a few of those which commonly appear in science studies. Numerous other less well-known cases are cited in detailed historical accounts of science, but these accounts do not enjoy general use as a kind of “currency” in science accounts. The recognition accorded the classic cases is not attributable to historians’ election alone, since historians have appropriated those instances of discovery which have already been marked as prominent within the writings interior to scientific fields – whether that recognition acted contemporaneously or retrospectively. Agassi (1963) remarks that in many cases historians have used the “up-to-date elementary textbook” as a standard for their selection of important facts and important figures in the history of science (see pp. 7–20 on “techniques” of “inductivist historians”). Agassi argues that the reduction of science into a clear and cogent history of exemplary discoveries is a constructive achievement by historians in the context of any current understanding of “natural facts.”

Here I am not saying that the prominence assigned to the exemplary cases is an artifact of historians’ work, nor that it is unjustified. Instead, I am pointing to the extreme attention accorded to a relatively limited set of cases in science studies as being a constituent feature of a distinctive field of discourse, a field that is distinguishable from the kinds of discourse which are part of the specialized sciences themselves.

4 The term “competence” here is akin to Garfinkel and Sacks’s (1970) use of the term “member” as a “mastery of natural language.” The term “competence” glosses the multiple ways in which parties display their work as constituents of interactive discourse (and, relatedly, collaborative work).
5 For example, note the following dialogue with an electron microscopist while he was busy examining some neural materials. The instance begins as the microscopist (J) is explaining a curiously appearing phenomenon that I had just pointed to in the magnified field:

J: Well, those things could be microtubules or vesicles ( )
great stuff think about further hhh uh:

M.L.: (Look et) the crappiness (?) 'n th' material (the èZaurrmess (?)) you w're talking about?
J: It was thi-.ck en it was fw.cked.
M. L.: The what?
J: It's thick, the section.
M. L.: Oh I s- so that you don't get a clear (?)
J: It's just
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J: Its not (h) its not-eh ‘ts I think this stuff was fucked up in theee uh
(0.8)
J: Embedding (?)
(4.0)
M. L.: In what way?
J: Em:: ch ran out, the
(0.5)
J: Plastic ran oudf it en
(1.2)
J: Had to re-embed it.

Note that my utterances are questions which employ the materials of J’s prior remarks as a basis for inquiring further. In my pursuit of a series of questions about the expressed artifactuality (“the crappiness”) of the particular materials being examined I locate J’s prior explanations as being in need of further specification. In this case he supplies some specification under this prodding, though later in my fieldwork (once the “tour” quality of our interactions was no longer relevant) my persistent questioning did not often receive very detailed explanation.

6 In ch. 7, note 21, I further address the issue of agreement in laboratory shop talk as it pertains to Merton’s formulation of the “norm” of organized skepticism in science (Robert K. Merton, “Science and democratic social structure,” Social Theory and Social Structure, 1957, p. 560). For the present I can say that displays of agreement and disagreement in shop talk did not mark a criterional difference between accounts in science and accounts in “ordinary conversation.” In written discourse, and in tours, however, disagreement was often explicitly expressed and was seldom as quickly resolved as in face-to-face situations among working collaborators.

7 See Gilbert (1976). Some of Holton’s statements are pertinent to this issue, as in the following quotation from Holton (1976, p. 18):

The detailed analysis of published scientific contributions generally only reinforces this feeling of the insufficiency of research publications as adequate descriptions of science. Most of the publications are fairly straightforward reconstructions, implying a story of step-by-step progress along fairly logical chains, with simple interplays between experiment, theory, and inherited concepts. Significantly, however, this is not true precisely of some of the most profound and most seminal work.

8 See Gilbert (1976) and Gusfield (1976), for analyses of scientific writings’ uses of stylistic and dramatic features of exposition and argument.

9 See Woolgar (1976). Woolgar presents a variety of discovery accounts of the “same” discovery of pulsars in astronomical research, where different accounts present remarkable variations in their report of “events” in significantly different ways.

10 Feyerabend (1970), in ironically contrasting the “objectivistic” rhetoric of modern scientific writing to the historic writings of Newton and Galileo, presents the following perspicuous instance:

Remember that all these reports are about cold, objective, “inhuman” inanimate nature, they are about stars, prisms, lenses, the moon, and yet these are described [in Newton and Galileo] in a most lively and fascinating manner, communicating to the reader an interest and an excitement which the discoverer felt when first venturing into strange new worlds.

Now compare with this the introduction to a recent book, a best seller even, Human Sexual Response by W. H. Masters and V. E. Johnson (Boston: Little Brown, 1966). I have chosen the book for two reasons. First, because it is of general interest. It removed prejudices which influence not only the members of some professions, but the everyday behavior of a good many apparently “normal” people. Second, because it deals with a subject that is new and without special terminology. Also, it is about man rather than about stones and prisms. So one would expect a beginning even more lively and interesting than that of Galileo, or Kepler, or Newton. What do we read instead? Behold, oh patient reader: “In view of the pervicious gonadal urge in human beings, it is not a little curious that science develops its sole timidity about the pivotal point of the physiology of sex. Perhaps this avoidance . . .” and so on. This is human speech no more. This is the language of the expert.

Note that the subject has completely left the picture. Not “I was very surprised to find” or, since there are two authors, “We were very surprised to find,” but “It is surprising to find” – only not expressed in these simple terms. Note also to what extent irrelevant technical terms intrude and fill the sentences with antediluvian bars, grunts, squeaks, belches. A wall is erected between the writers and their readers not because of some lack of knowledge, not because the writers do not know their readers, but in order to make utterances conform to some curious professional ideal of objectivity. And this ugly, inarticulate, and inhuman idiom turns up everywhere, and takes over the function of the most simple and the most straightforward description.

(footnote, p. 97)

Feyerabend goes on in the above vein of irony by citing passages from Masters and Johnson and rephrasing them in a vernacular mode, without claiming to lose any of the sense of the quoted passages. It is not my intention here to follow Feyerabend in pointing out ironies in the use of objective expressive formats in science writing, as I am satisfied merely to demonstrate the peculiar artfulness of science writing as a distinct form of discursive accomplishment from that displayed in ordinary shop talk.

Incidentally, ironic analyses of science writing in Feyerabend, and also in Gusfield (1976), locate social science reports as cogent
exemplars of a rhetorical use of “objectivity.” Perhaps this use of social science writings, rather than, for example, writings in physics, upholds claims on the rhetorical functions of “objective” formats through the implication of a clear difference between the quality of findings in research and the manner in which those findings are turned into seemingly secure objective results in reportage. The irony seems to turn on a notion of how social science research misuses the objective format of research report, by rewording common sense verities into a technical language. This may be true for many social science writings, though to focus on this point is to miss a more general matter of how the “objective” format is identified with science-writing, however “hard” or “soft” the character of the asserted facts independent of the writing.

11 Gilbert (1976, p. 285) asserts, “The report is written in the passive, so that allusions to the identity of the author do not occur in the body of the account. The effect of these devices is to emphasize the anonymity of the researcher, so that the research becomes “anyone’s research.”

12 The activity of “writing” is amenable to description and analysis as a social activity. Precedent for such an analysis of a similarly “private” activity is found in James Heap’s work on the achievement of collaboratively organized “readings” in grade-school classrooms (see Heap, 1978). Heap found that reading lessons were organized in a turn-taking system of speaking rounds, where each student-speaker verbalized a reading of a text while other students and the teacher listened, and occasionally offered corrections. Writing, or writing sessions among lab members, may similarly involve displays of a current attempt at “a writing” by one speaker-writer while other parties attend to that speaker’s account and collaborate in that account in a socially organized way. Whether or not this is the case for science writings, however, is presently a conjectural matter.

13 My comments on hoaxing and irrepliicability rely heavily on remarks made by Professor Gary Lynch of the Psychobiology Department at U.C., Irvine during conversations with me in Spring, 1975.

14 Watson (1968, passim).

15 Kowarski (1965). See also Price and Beaver (1966); Weinberg (1970); and Barnes and Dolby (1970), (see esp. pp. 21–22, where the authors state, “it has become increasingly clear that discovery is not a thing easily attributable to one man.”)

16 Garvey and Griffith (1971).

17 Ibid., p. 361.

18 Ibid., p. 362.

19 Becker (1963) states:

   The process of self-segregation is evident in certain symbolic expressions, particularly in the use of an occupational slang which readily identifies the man who can use it properly as someone who is not square and as quickly reveals as an outsider the person who uses it incorrectly or not at all. (p. 100)
The noticing produced in line (1) and repeated in lines (3, 5) is in each case produced in a dramatic and playful way. In line (2), J responds in a rather “straight” manner, not recognizing in his response any joking connotation to the prior noticing. The noticing is repeated, with the almost hysterical manner of its production being upgraded in line (6). In (6), J takes up the playful cue and carries it graphically forward. In (13), J’s reference to particulars in the electron micrograph which occasioned H’s mock noticing in (1) brings the talk out of the interlude of play and back to talk on axon sprouting. This is not to say that the compliance of other parties to listening to such “work” talk was guaranteed by J’s reference in (13), but they do not opt to continue their fun any further. The matter of interest here is how the remark in (1) and its subsequent versions in (3, 5) act to touch off a clearly frivolous sequence of actions, though eventually they are redressed in (13) as remarks within the collaborative work of examining the micrographs.

I mention these instances of “playful” conversation not as an exemplification of any dichotomy between “work” and “play,” but as a way of showing how problematic was the identification of the talk with the work of doing science. Some remarks, and actions, which were located within the details of the work underway were nonetheless not accountably part of the work’s course. For instance, while experimenting with the effects of specific diffusible chemicals on the firing patterns of neurons, three lab practitioners were oriented to an apparatus which recorded the amplitude and frequency of firings on a revolving paper graph. A pencil was hooked up to the electrophysiological monitoring device and automatically recorded a pattern on the graph which was interpreted as a neural “output” to an artificially produced electrical stimulus. The experiment was not going well at the time as the neural output readings showed no remarkable variation from its pattern prior to the introduction of the chemical into the cellular media. At one point, one of the parties to the experiment grasped the recording pencil and manually traced a graphic pattern which depicted a dramatic jump in amplitude. The tracing was drawn in such a way as to resemble a pattern which would have been indicative of a successful experiment. This occasioned some laughter from the others, and was recognized as a sort of non-verbal joke. Later the lab director, who was one of the three parties, crossed out the “joke” tracing on the graphic record. This latter action marked the “joke” action was produced marked it as an “insert” into the accountable course of electro-physiological readings, and its later erasure from the record emphasized its differentiation from the “data.”

I will not, however, use the “off the record” character of certain events and actions as a criterion for distinguishing shop work from incidental talk and actions in the lab. Doing so would be a move toward treating such reifications as written reports of experiments as authoritative versions of lab practices. This would leave any consequential work which did not reach “the record” unexamined. I do not want to go so far as to ignore the issue of how the talk and actions of members becomes available as “on the record.” Accordingly, I am inclined to examine members’ actions in the lab as relevant to accountably scientific records regardless of whether the record formulates those actions.

23 Some of the written works in conversational analysis which bear on concerns in the chapter are: Pomerantz (1975); Sacks and Schegloff (1974); Schegloff, E. (1972); and Sacks, Schegloff, and Jefferson (1974).

24 Sacks, Schegloff and Jefferson (1974, pp. 714-15) discuss the phenomenon of how talk in conversations can be either continuous or discontinuous depending on options exercised by speakers when “transition-relevance places” occur. Whether or not a silence in conversation is to be classified as a “gap,” “pause” or “lapse” depends, in part, on the character of prior talk, whether a next speaker has been selected, or whether the prior utterance makes an immediately following utterance relevant (as for a question, request, etc.). The ethnographic considerations that bear upon the identification of a situation as one of “incipient talk” are not discussed in the paper. For my remarks on these latter matters I am indebted to Alene Terasaki for her discussions with me on the subject of incipient talk and reference.


26 In the above transcript, the 1.0 second silence prior to the last utterance by B (“Yeh know that?”), appears somewhat ambiguous in its analytic relation to prior and subsequent talk. On the one hand, given the predominance of a pacing in the talk in which pauses appear between each utterance, and within continuous turns by a same-speaker, the silence can appear to be quite unremarkable. On the other hand, B’s utterance in the final line of the transcript seems to mark the silence as an absence of any expressions of
agreement to the just-prior assertion. Having made a claim about
the size of the axon terminals (that they were "rilly small"), B's
question, "Yeh know that?" requests a confirmation by A, a
confirmation that was not spontaneously produced immediately
subsequent to B's claim. Interpreted in this way, the "silence"
appears as a delay on A's part to address the claim.

27 Much has been made of Garfinkel and Sacks's (1970) use
of "indexical expressions" as a way of characterizing the context-bound
use of expressions in discourse. In my selection of a few sorts of
referential terms that were observed in shop talk, I am not limiting
"indexicality" to a few classes of terms, such as pro-terms. I do not
see "indexicality" to be a feature of a definite collection of linguistic
entities, unlike linguists who have isolated a class of terms as
"indexicals." Following from Garfinkel and Sacks's initiative, we
can see that any usage in discourse is "indexical" insofar as its
interpretability is part of the in situ actions which employ it.
Conversational usages of "technical" terms are as tied to
conversational actions in their circumstances as are pro-terms.
Furthermore, the achieved sense of pro-terms, as constituents of
particular actions is by no means "loose" or "flexible." It is only
under the auspices of a program that examines the terms as isolated
word-tokens that they take on the curious character of being non-
specific references.

28 Social science constructions of "the scientist" often exhibit these
stereotypical features. It is not so much that these characterizations
are wrong in every case, but that they give a narrow version of a
quite heterogeneous phenomenon. Further, it is questionable
whether the occupation should exhibit its uniformities in the
regularity of a "personality type." A characterization of such a
type is presented in Roe (1961). Roe's ideal-typical construction
provides for a scientist with the following characteristics:
independence, tolerance for ambiguity, strong ego, low intensity
personal relations, preoccupation with things and ideas rather than
people, and preference for calculated risks. A somewhat different
image of "the scientist" was elicited by Orth (1965) from managerial
workers whose business entailed contact with industrial researchers.
Scientists were characterized by these persons as "logical,
opinionated, impatient, intense, thorough, meticulous, reserved and
clannish." Furthermore, "they normally regard conformity as a
cardinal sin, and in their efforts to avoid it they often behave in
unexpected ways or become interested in the bizarre and unusual"
(p. 197). No doubt such characterizations can be found to "apply"
to some scientific persons, but the point I am trying to make here
is that whatever "image" is constructed of the "scientist," it is not
likely to be upheld for long as an adequate inroad to any detailed
examination of scientific work in its own environment.

6 Two notions of agreement

The following discussion will explicate two distinct approaches to
the study of agreement in scientific inquiry. One of these
approaches will then be used in locating a phenomenon and develop­
ing an appropriate research policy for the analysis of laboratory
shop talk. The two conceptions of agreement which are outlined
here will be called "implicit agreement" and "achieved agree­
ment" as a way of contrasting them for the sake of argument.
The notion of "implicit agreement" will be explicated by reference
to a recent text by Derek Phillips on the applicability of Wittgen­
stein's philosophy to the study of science.1 Numerous other works
in the science studies literature use a similar version of "agree­
ment," though Phillips's account is singled out for treatment here
because of its explicit use of the term "agreement" in a program­
matic discussion of the sociology of science.2 The notion of
"implicit agreement" will then be programatically compared to a
policy for the study of "achieved agreement" which was developed
from Harvey Sacks's lectures and research notes on the subject
of agreement.3

A Implicit agreement

Time and again, Wittgenstein reminds us that language-games
are based on agreement, that concepts and meanings are
largely social creations. This means that they are created by
people in groups. It is therefore not abstract logic or external
entities existing "out there" which are primary, but concrete
human practices. Thus, language (and consequently science,
art, and religion) is pre-eminently a social phenomenon.

Human actions and human agreement underlie all scientific
and intellectual practices, including philosophy. (Phillips, 1977, p. 137)

Phillips uses a concept of "human agreement" to account for intersubjective consistencies in the work of scientists. The regularity of procedures, the reliability of outcomes, and the stability of observed "facts" are formulated as human accomplishments based on an underlying and general "agreement" on what counts as observed "facts" are formulated as human accomplishments based on an underlying and general "agreement" on what counts as observed "facts". Rather than saying that logical or methodological formulae determine the performance of procedures, or that the objective characteristics in a world of independent facts determine what is observed and described in science, Phillips's reading of Wittgenstein provides for this determinacy as mediated by agreement to do procedure or to do observing in a customary way. The behavioral character of scientific procedures is emphasized, and agreement on how to do such procedures, rather than any rules or formulae in any of themselves, is given primacy in establishing the invariance of such procedures.

In demonstrating how scientific procedures are reflexive to agreement on how to perform those procedures, Phillips borrows Wittgenstein's famous example of "following a rule" in continuing the number series, (2, 4, 6, 8, 10, ...). Though most of us who are trained in the elementary operations of arithmetic would continue the series as (12, 14, 16, 18), Wittgenstein provides for how a trainee could follow the series in alternative, but "natural" ways. Furthermore, these alternatives exhibit a discoverable consistency with the original series, a consistency which is not anticipated in the customary invocation of the rule, "add two." Phillips quotes the following example from Wittgenstein:

Now we get the pupil to continue [the series] beyond 1000 — and he writes 1000, 1004, 1008, 1012.

We say to him: "Look at what you've done!" — He doesn't understand. We say: "You were meant to add two: look how you began the series!" - He answers: "Yes, isn't it right? I thought that was how I was meant to do it." — Or suppose he pointed to the series and said: "But I went on in the same way." — It would now be of no use to say: "But can't you see ...?" — and repeat the old examples and explanations. — In such a case we might say, perhaps: It comes natural to this person to understand our order with our explanations as we should understand the order: "Add 2 up to 1000, 4 up to 2000, 6 up to 3000 and so on."

Such a case would present similarities with one in which a person naturally reacted to the gesture with the hand by looking in the direction of the line from finger-tip to wrist, not from wrist to finger-tip.⁴

The ability to "follow a rule" in this case is not a matter of a rule's having prescribed a single and definite course of action by virtue of the appearance of the original sequence of numbers, (2, 4, 6, 8, 10). A little imagination shows this series to be prescriptive of (or, perhaps, retroscripted by) a variety of alternative paths, only one of which was anticipated in the customary practice of continuing the series.

That only one of the available alternatives intuitively appears to be "the way" to continue the series is related not to the properties of the number series itself in isolation from any occasion of use, but instead to the training of those who put the series into practice. This situation of common practice based on common training is called "agreement" in Phillips's account.

Phillips extends the relevance of his reading of Wittgenstein to cover natural scientific inquiries as well as mathematics. In doing so, he cites the works of philosophers of science such as Kuhn, Polanyi, Hanson, Holton, and Feyerabend in order to relate conditions of facticity to a background of implicit social agreements.⁵

Phillips argues that particular scientific inquiries, complete with their practical achievements, are possible only on the basis of shared circumstances of training and language. This claim is similar to Kuhn's proposal that "shared exemplars" in the working situation set up conditions where scientists come to work on common problems and extrapolate solutions to those problems within the horizon of unformulated assumptions about the nature of the universe which are embodied in the exemplars.⁶ As I understand Kuhn's argument, "exemplars" are provided for as instructional models, up-to-date methods and instruments (which provide a prefigured and standardized way of making phenomena visible and manipulable), technical vocabularies, and problems of widespread interest in a discipline, all of which take on an historically relative current configuration for any scientist entering the field.

The background of understandings, relevant problems to work on, available methods to use, and ways of speaking which Phillips glosses as "agreement" is a curious mixture of specifically scientific matters and common-sense knowledge. Polanyi makes reference to the inextricability of common-sense understanding in scientific work in his discussion of "tacit knowledge" in which he argues that the unremarkable facticities of a world of common objects, within a natural language, provide an environment which science relies upon, operates within, and continually returns to in its interpretations.⁷ Like the "observation languages" discussed in
Feyerabend's matters of tacit understanding are provided for as modern science's pre-reflective inheritance from a language and intuitive understanding which science, somewhat paradoxically, often seeks to correct.

In Phillips's account, *agreement* is invoked in explaining how members in a community of scientists are thrown into the midst of commonalities in a working situation with shared standards, tacit understandings existing as an implicit structural basis for the intersubjective character of observed and reported "facts." A version of "observation" and "fact" in this account counters arguments which attribute an unequivocal character to the "facts" independent of the temporal and social circumstances of their discovery and display. Famous historical cases are used in citing examples of discoveries which initially contradicted "natural" facts of life, and eventually replaced those "immutable facts of nature" with new or revised laws. A gestalt account of "seeing" is provided as a theoretical way of reconciling how incommensurable viewpoints can arise in a common observational situation. This account formulates "seeing" as an active structuring of a field of action in elucidating phenomena which thereafter are identified as "natural" features of an entitative system.

The appeal of Phillips's account of science for social studies is that it invokes a social environment as the generative basis for natural facticities which are discovered and described in natural scientific work. His account does not reduce natural facts to social facts, but instead provides a social basis in training and discourse for the alignments of scientific collectivities organized around those facts. Where positive accounts of science see such alignments as a matter of a mutual recognition of the independent attributes of the object, Phillips's account, without denying those independent attributes or their relevance to an inquiry, asserts a social basis in "agreement" for the mutual recognition of such attributes, since those attributes require discovery from among varieties of possibilities. Accordingly, a world of natural objects is provided with an "aspected-ness" which supports a variety of accounts of its features.

Given the centrality of a notion of "agreement" in Phillips's argument, we will now delve into the matter of just what this "agreement" might consist of as a social phenomenon, and how it sets up a sociological analysis.

In Phillips's account the term "agreement" is used as a reference for a sort of underlying basis or general condition for the situated work of scientific practitioners. Phillips uses such expressions as (from Wittgenstein) "they agree in the language they use," "we agree in our calculations," and "shared standards allow for full agreement among scientists as to what counts as freezing or ice." These expressions employ "agreement" as a term which mediates the congruences of method and result which are observed to be present in scientific work circumstances, by referring those congruences back to underlying conditions of social agreement.

What is striking about Phillips's use of "agreement" in such statements is that we might call members' compliance to their practical circumstances are formulated as matters of agreement. That, for example, a scientist speaks a common language with his contemporaries, undergoes a particular course of training, uses available technologies (the meters, instruments, together with prescribed ways of using them), sees the world from a certain perspective, or works on certain inherited unsolved problems in a discipline, is provided for by Phillips as the scientist's *agreement* with other scientists on these matters. These "agreed to" matters include not only the scientist's actions as scientist, but also cover the totality of his actions as a social being (in Schutz's sense). These actions are formulated as "agreements" to exercise options which do not challenge socially organized forms of life. The manner in which agreement is achieved is attributed by Phillips to members' openness to the rewards and punishments of training (socialization) within the specific occupational enterprise as well as the general society. It is not as though a student is presented with an argument with which he can agree or disagree, but that he learns a way of speaking and acting that constructs his inquiry in a certain well-defined way... a way that changes with the historical advent of new theoretical approaches, new instruments and recently discovered entities (which a historical or sociological overview presumably recognizes in a way unavailable to the "unreflective member"). That such compliance could be formulated as a matter of "agreement" may require a somewhat strained notion of free choice and action, though Phillips will not be contested here on that point.

Where issue will be taken is on the matter of how Phillips's account employs "agreement" as a device for asserting a theoretical correspondence between, for example, individual action and social action. When considered as such a device, "agreement" is achieved as a term in a theoretical statement which asserts an equivalence or correspondence in an analytic relationship. This analytic use of "agreement" is quite prevalent in social science accounts and is by no means unique to Phillips. In such accounts "agreement" is used as a term for "equivalence," "similarity," "contingency," or "correspondence" between elements in an analytically provided objective domain. When applied to a sharing of common attributes among individual members in a social popu-
tion, this "agreement" is more than a matching-up of objective characteristics; it is a correspondence which is attributed to the compliant actions of members as "actors." For Phillips, "agreement" formulates how scientists can share social structures and do so in a volitional manner - in the manner of each scientist achieving attributes which mark his actions or interpretations as similar to those of other scientists. "Agreement" thereby alludes to an achievement of members of a society while simultaneously acting as a term for equivalence in natural-theoretic statements which solve a general problem for a social theorist.15

Phillips's account of agreement confuses agreement as an inter-actional accomplishment with attributed sources of socially structured action. Although "concrete human practices" are said to "underlie" scientific practices, we are left in some doubt as to how "concrete" or observable such practices of agreement might be. What is here at issue is a difference between "agreement" as a noticed similarity in the actions of two or more persons and "agreement" as an action which two or more persons can do together in specific interactive situations. In the first case, I can notice how a collection of persons "agree" in their clothing, speech patterns, social class, etc., though they may never have met each other or been aware of how their separate actions fall into some common category which I invoke. In the second case, I can witness (or can involve myself in) an action whereby parties in a conversation "agree" by explicitly asserting that they do so ... by making an agreement happen in their practices of talking together. However, in the latter case, agreement is exhibited as a "concrete human practice," the former sort of "agreement" is not so much a definite practice of members as it is an interpretation by an analyst as to how congruent social practices are possible.

Practices of agreement are not ignored in Phillips's account, as he, quoting Wittgenstein, makes reference to "expressions of agreement" as particular actions in a training situation: "In training someone to play a language-game: 'I do it, he does it after me; and I influence him by expressions of agreement, rejection, expectation, encouragement. I let him go his way, or hold him back; and so on.'"16

These "expressions" of agreement which are featured in training situations are not, however, to be confused with the "agreement" which Phillips asserts is an outcome of such training. Whereas "expressions" are explicitly part of the interactions in which they occur, implicit agreement "underlies," is prior to, or is somehow behind the specific occasions in which the analyst asserts its relevance. Confusion results when "implicit agreement" is taken to refer to an empirically observable, and therefore stud-
agreement with one another on the relevant matter. (This is not to fault the methodology of survey analysis, but to point to the distinctness with which agreement is achieved in its accounts.)

3 Generality: agreement is produced through analytic operations which work independently of local conditions of members' conduct with one another. Agreement is not generalized on the basis of collections of specific "cases" of agreement asserted in particular conversations, but is provided for as an underlying condition for widespread regularities in members' beliefs and behaviors, as witnessed through a sociologist's analytic apparatus. Agreement is stipulated, not witnessed in the sense reported in, for example, "we came to an agreement yesterday about the divorce settlement."

4 Rules of method formulate the determination of whether two compared objects are to be counted by the analyst as "in agreement." Analytic difficulties arise in such cases, where different respondents to an open-ended interview question give differently worded answers which are believed by the analyst to "say the same thing." An analytic competence is used in finding such statements to be in "agreement" insofar as the statements' agreement with one another is not explicitly part of their production. Analysis becomes a coding problem rather than one of recognizing displays of agreement as they are produced. (Note: written questionnaires with fixed response-sets do not circumvent this feature of the determination of agreement by the analyst, since the analytic coding of responses treats one check-mark as categorically equivalent to another, regardless of what the respondent may have thought or meant in producing it.) The important point here is that whatever the adequacy of the analytic coding procedure, the procedure operates on the basis of a set of relevances which are embedded in the analytic enterprise, and which are not necessarily accountable in any situation where persons display their agreements to one another.

5 The achievement of agreement through the exterior analytic is provided for as a members' accomplishment through such "mechanisms" as socialization, common culture, and social structure. These "mechanisms" are themselves analytically stipulated phenomena whose observability is achieved as a technical accomplishment in professional sociology. Instances of the above procedure for producing agreement in analytic treatments of members' assertions or activities are found in social science researches where "sentence frames" or questionnaire items are designed to produce standardized conditions for the appearance of responses. Response items are extracted from the setting of their occurrence in encounters with interview or questionnaire forms and are compared with responses of other respondents to the same question. Quantitative indices of patterns of similarity and difference arise from such a procedure.

In historical studies of science, a temporally isolated analyst employs a rubric of similarity and consistency in providing documentary exhibits of "controversies," "revolutions," "communities," and "lines of scientific advance." Although a cogent and entirely plausible claim is made in such studies that the conditions of similarity and difference in historical continuity are initially generated within a community of practitioners who design their work with a regard for one another's prior achievements, how this situated design may have been achieved in detail is, by necessity, left unexamined.

B Achieved agreement

The version of agreement which will be used herein for studying situated occurrences of shop talk relies heavily on a reading of Harvey Sacks's notes and lectures concerning the topic of "achieved agreement." Sacks's orientation to agreement, as understood here, proceeded on the basis of finding statements or utterances to be in agreement with one another when that relation was supplied as an interactionally asserted relation (rather than as an analytic relationship of similarity).

Sacks's notion of agreement developed out of the observation that a feature of ordinary language is that it provides speakers with devices which are usable in asserting agreement. A simple case of such a device is the token "yes," which, when placed by a speaker in relation to a prior assertion by another speaker, marks a display of agreement in the conversation. A feature of such a display of agreement is that agreement is achieved as a matter of assertion rather than of any examinable relation of similarity between the agreed-to statement and the token of agreement. A common occurrence in ordinary conversation is that speakers, in one way or another, say they agree with each other; such achievements of agreement are not primarily a matter of extrinsic analytic determination.

Characteristic features of the phenomenon of "achieved agreement" which contrast with those outlined above under the heading of "implicit agreement" are summarized here as follows:

1 Asserted relations: the assertions or activities which achieve agreement are produced with an orientation to one another, rather than being independently compared. Agreement is not provided in any equivalence of one activity with another, but via asserted relations. An utterance such as "yes" or "I agree," is often
AGREEMENT IN LABORATORY SHOP TALK

sufficient for achieving agreement without further display of any underlying condition of the agreement so asserted.  

2 Immanent analytic: the analysis which relates the activities grouped together under “agreement” is not provided via the work of an absent analyst, but is accountably part of the production of an agreement sequence. (Certainly, in conversational analysis the analyst engages in work which is not part of the original production of the materials analyzed. However, the expressed aim of the enterprise is to seek an account of how any agreement was achieved through parties’ ways of relating one utterance to another. This differs considerably from analytic programs which do not find it a problem that the terms of “agreement” for relating similar activities together are not explicitly featured in the activities studied.)

3 Agreement is provided as a local achievement. This is to say more than that it operates in a “microenvironment.” It is to say that the achievement of agreement is provided via the asserted relevance of one statement or activity to another in the actual setting of its production. By “local achievement” I mean the occurrence of agreement in these two senses:

(i) ethnographically local – agreement is achieved in the scene for those involved in its production. It is not available under analysis in a way that would surprise, affront, or claim to know better than the parties to the work of agreement;

(ii) conversationally local – the materials of an agreement are displayed in the temporal adjacency of conversational utterances. This adjacency involves not only the properties of pace, duration, sequence, and continuity of utterances in measured time, but also the ways in which one utterance can “address,” “refer to,” or otherwise make relevant a prior utterance. These relations work independently of analytic operations that isolate and compare temporally removed actions. Instead, they appear as collaboratively linked actions.

4 Relation to truth and correctness: assertions of agreement show determinate characteristics which are independent of whether or not parties “really” agree in the matters asserted. The assertion of agreement can work independently of criteria which assure the correctness of the asserted relationship. This is to say that members can display agreement independently of any compulsion to account for how they have come to agree under any notion of truth or correctness. Accordingly, at times, under analytic re-inspections, parties can be found to (or can find it of each other that they) have lied. Achieved agreement, given its production on occasions of interaction through the devices of conversation, bears no necessary relation to whether or not parties “agree” in their “underlying attitudes,” are “telling the truth” to one another (“being honest”), or “really believe” what they are asserting. However, achieved agreements are factual inasmuch as they are displayed interactionally. An agreement is a witnessable event for those parties on the scene of the agreement, and as such, it may be invoked on a later occasion in holding one or more parties accountable to what was agreed to. As public interactional events, achieved agreements have a definite, tangible, and binding character regardless of any “underlying” attitudes or personal commitments which might be involved.

5 The achievement of agreement is identical with its production and recognition by interacting parties. Agreement is not stipulated as a phenomenon which is presupposed in members’ actions, or which underlies their collaborative actions; instead it is manifest in-and-as those collaborative actions. Prior agreements may indeed set up later agreements, and “unspoken” agreements are accomplished on some occasions, though these are separate matters worthy of study in their own right. The matter of interest here is that on some occasions agreement is a witnessable and explicitly produced activity in the immediacy of members’ conversations, and as such, it is an empirically studiable phenomenon.

The striking feature of “achieved” agreement in comparison with “implicit” agreement is that agreement is something which speakers make happen on occasions of interaction; to put it differently, the language has devices with which agreement can be produced and displayed. Agreements do not require the social sciences to discover them as “underlying” conditions of social regularity; they are “already there” as commonplace matters for members’ production and recognition in discourse. This fact is largely ignored in science studies, except as a passing matter. When agreement is mentioned as an important social phenomenon, the matters of interest are “implicit agreement” or “consensus” among members of a group of scientists, and not the phenomenon of how such “agreement” manifests or is accomplished in particular instances of concerted action. We are sometimes given a generalized sense of how consensus among scientists “forms” around, for example, a new physical theory, where the dissemination of an account in a population of scientists “leads to” a variable pattern of acceptance. We are left in the dark, however, on how such alignments form in any particular instance, whether it is a matter of persons reading of the discovery and deciding independently that they agree with it, whether initially skeptical persons are “convinced” in debate, or whether a variety of ways are possible in achieving agreements. More central to the concerns of this volume is the matter of how agreement is involved...
in the working situation where a discovering account arises, prior to any public announcement of results.

As stated previously, Wittgenstein was quoted by Phillips as mentioning "expressions" of agreement in a discussion of typical training situations, as a kind of "yes, that's it" device which a teacher employs in instructing a student's compliance to a pre-established order of action (a "correct" way in a socially organized form of life). From Phillips we are given the image that in science such training devices are used for inculcating the varieties of standards which make up definite scientific procedures, as situated interactional agreements which are preliminary to, and in part responsible for, agreement on method, object, theory, or pre-theoretical reality.

The brief mentions that Phillips makes of situated instances of agreement are limited to training situations, where a body of knowledge or practices is transmitted from a presumably competent person to one who has yet to attain such a competence. In such circumstances, expressions of agreement mark a success in the student's compliance to the training and act as a kind of reinforcement of the student's actions. There is no reference to any possibility that "expressions of agreement" might be involved in situations where interactants are oriented to "facts" which have yet to be determined, or procedures which are in the course of being designed. In such situations, agreement has a different consequentiality, as it becomes synonymous with collaboration on "fact" or "procedure" at the scene of an inquiry.

The remainder of this volume will focus on "achieved agreement" as it was involved in lab members' work of arriving at factual accounts of neuroanatomical events. Unlike situations in which a body of knowledge is transmitted through a course of instructions, these agreements were consequential to the initial determination of what "the facts" would become for those involved in their discovery. Agreement, as discussed here, was part of the social accountability of discoveries, as a way in which discovering accounts were formed within the innumerable perplexing and indeterminate circumstances which were characteristic of laboratory research. Agreement between collaborators in research projects was observed to be a consequential part of the way in which discovering accounts emerged in day-to-day shop work. Matters of, for example, what "the data" exhibited, and whether particular documentary evidences could be counted as technically adequate displays of anatomical and physiological events were often contingent upon agreements among parties to lab projects.

As was elaborated in the discussion of artifact-accounts in Chapter 4, the definiteness with which neural phenomena were exhibited was inseparable from the technical manner of the production of laboratory displays. Accordingly, a standing issue in members' accounts was the possibility that artifacts in the display procedure were taken for "real" neural entities and processes. The possibility that evidences for, axon sprouting were vulnerable to reinterpretation as artifacts accompanied shop work in a persistent way and was often an issue in member's discussions when those evidences were produced and reviewed. In many instances where electron microscopic and other documentary evidences showed novel and unexpected features, no formal criteria of interpretation were available to assure a competent analysis. In such situations, accounts of what was going on were practically settled in members' shop talk, as manifested in agreement and disagreement sequences.

The involvement of agreement in the achievement of collaborative accounts of natural phenomena is not limited to problematic circumstances in research. It is perhaps most visible in such circumstances, but it is by no means relevant only on occasions when experimental results or observational records were remarkably ambiguous. Agreements were produced in definite ways in talk of matters which were not in dispute, though in such cases one could claim that social agreement was secondary to a recognition of matters that would be obvious to anyone. In any event, achieved agreements were examinable expressions which exhibited a collaboration on matters in shop talk, however obvious or obscure were the matters attested to.

Notes

1 Note, that in my remarks on Phillips (1977), I am not necessarily addressing Wittgenstein, though Phillips's account is primarily an interpretation of Wittgenstein's later works, such as Philosophical Investigations, and Remarks on the Foundations of Mathematics. I am only critical of the way in which Phillips appropriates Wittgenstein's philosophy for the purposes of a proposal for a sociology of science.

2 Phillips states, "Wittgenstein emphasizes that philosophy, like other language games, must base itself on concrete human existence, which includes agreement and socialization as basic facts. The concept of "agreement" is essentially a sociological concept" (p. 136, ibid.).

3 The policy articulated here relies primarily on Sacks's Lecture no. 23, Spring 1966 (Unpublished lectures, UCLA and UCI), and on Sacks's Agreement Notebooks, I, II, & III.

The programmatic version of agreement stated here is a reading.
of Sacks's notes in light of prevailing interests in the topic in science studies. I make no claim that Sacks himself had such programmatic interests in mind, as it appears more likely that his remarks on agreement were largely based on particular items of conversational data and were not provided as a comprehensive formulation on the matter. However, in the course of his remarks, commentaries are interspersed on agreement as it is found in social science treatments as against "in" conversation, and these comments have been adopted as a "policy" in the formulation of agreement here.  


5 Phillips (1977, pp. 64-73), on "The new image of science."  

6 Kuhn (1970) speaks of exemplars in the following remarks:  

The role of acquired similarity relations also shows clearly in the history of science. Scientists solve puzzles by modeling them on previous puzzle-solutions, often with only minimal recourse to symbolic generalizations. Galileo found that a ball rolling down an incline acquires just enough velocity to return it to the same vertical height on a second incline of any slope, and he learned to see that experimental situation as like the pendulum with a point-mass for a bob. (pp. 189-90)  

First, if I am talking at all about intuitions, they are not individual. Rather, they are the tested and shared possessions of the members of a successful group, and the novice acquires them through training as a part of his preparation for group membership. (p. 191)  

7 Polanyi (1966) discusses the essential character of tacit knowledge in scientific undertakings in the following way:  

I think I can show that the process of formalizing all knowledge to the exclusion of any tacit knowing is self-defeating. For, in order that we may formalize the relations that constitute a comprehensive entity, for example, the relations that constitute a frog, this entity, i.e., the frog, must be first identified informally by tacit knowing; and, indeed, the meaning of a mathematical theory of the frog lies in its continued bearing on this still tacitly known frog. Moreover, the act of bringing a mathematical theory to bear on its subject is itself a tacit integration of the kind we have recognized in the use of a denotative word for designating its object. And we have seen also that a true knowledge of a theory can be established only after it has been interiorized and extensively used to interpret experience. Therefore: a mathematical theory can be constructed only by relying on prior tacit knowing and can function as a theory only within an act of tacit knowing, which consists in our attending from it to the previously established experience on which it bears. Thus the ideal of a comprehensive mathematical theory of experience which would eliminate all tacit knowing is proved to be self-contradictory and logically unsound. (p. 20)  

8 Feyerabend (1970) speaks of "observation languages" as providing implicit constituents in any scientific account which are reflexive to the historical design of a language within ancient (and often questionable, at the time of a discovery) conceptions of mundane relationships and objects:  

the material which a scientist actually has at his disposal, his laws, prejudices, his attitude toward the absurd consequences of the theories which he accepts, is indeterminate in many ways, it is ambiguous, and never fully separated from the historical background. This material is always contaminated by principles which he does not know and which, if known, would be extremely hard to test. Questionable views on cognition, such as the view that our senses, used in normal circumstances give reliable information about the world, may invade the observation language itself, constituting the observational terms and the distinction between veridical and illusory appearances. As a result observation languages may become tied to older layers of speculation which affect, in this roundabout fashion, even the most progressive methodology. (Example: the absolute space-time frame of classical physics which was codified and consecrated by Kant.) (p. 43; see also footnote 151, p. 121 of Feyerabend)  

9 See for instance, Hanson (1958, pp. 4-30).  

10 In this discussion of Phillips I have mentioned how his notion of agreement makes reference to situations that "underlie" specific instances of action. By this I mean that matters of immediate conduct in, for example, scientific work are traced back to a prior condition of common training, or, more elusively, to a shared cognitive structure of some generalized social origins. Phillips, despite his mention of "concrete human practices" as a basis for agreement, makes no effort to specify how such "practices" manifest in any particular setting of conduct. Instead, his notion of agreement apparently does not require concrete origins in any particular situation of human conduct, but is a generalized rubric for how the "social" comes to inhabit "individual" actions - it is a proposed "solution" to a long-standing theoretical problem in sociology and social philosophy.  

With regard to the issue of postulating "underlying" social realities for commonplace events in sociological explanations, it is interesting to note that Phillips disavows any use of such a natural theoretic practice in the following statement:  

I have acknowledged that the conception of science and reality which I have put forward is a "possibility"; it is one way of looking at and talking about the world. In this sense my position differs not only from the dominant view of sociological inquiry but also from the position of ethnomethodologists like Garfinkel.
and Sachs [sic]. In common with most ethnomethodologists, they are concerned with the "discovery" of underlying patterns, regularities, and the like. For example, Garfinkel says that his research is directed towards "discovering the formal properties of commonplace, practical commonsense actions." And both Garfinkel and Wilson speak of the importance of documentary interpretation, which "consists of identifying an underlying pattern behind a series of appearances such that each appearance is seen as referring to, an expression of, or a 'document' of, the underlying pattern." Ethnomethodologists assume that by becoming self-conscious about their own research procedures and language-use, and by utilizing some sort of methodological reduction, they can gain objective knowledge of the "true" nature of social phenomena. In short, they claim an epistemologically privileged position. (p. 201)

First, pious statements about the relativistic character of one's theoretic position do not wall off the abyss of reflexivity. Second, I fail to see how Phillips could arrive at the above version of ethnomethodological research. His discussion of "the documentary method" appears to confuse Garfinkel's account of its use in ordinary social circumstances with a recommendation of it as a research policy for ethnomethodology. Garfinkel's account of members' work of seeing underlying patterns and regularities in the events of their everyday experience, is not a recommendation for "looking into" "underlying" realities of ordinary social events. Indeed, the position of Garfinkel and Sacks proposes just the opposite. A persistent recommendation in their researches involves an orientation to the local, in situ particulars of ordinary conversation and ordinary actions, as those particulars are accountable part of members' practical circumstances. If this policy involves a claim about "reality," the claim identifies any "reality" with the work of parties in their practical circumstances. This is hardly an orientation to an "underlying" or "behind the scenes" reality, posited through the use of social science methods which undercut the "subjective" accounts of members. The fact that one cannot find in ethnomethodology a coherent "methodology" to be used for any subject matter (e.g. any sort of members' conduct whatsoever), attests to the programmatic orientation to "method" in the detailing of circumstantial instances of members' actions. This is not to say that ethnomethodologists' work is exempted from the devices of natural theorizing (such as the aforementioned, "documentary method") through self-consciousness or "some sort of methodological reduction." Given the ubiquitous character of such practices, and the lack of any assurance to the contrary in programmatic statements of ethnomethodologists', ethnomethodologists are as party to such practices as are any members.

11 Wittgenstein (1953, p. 251) quoted in Phillips, p. 30. See also Phillips, p. 215, where he states, "Wittgenstein points out that the terms 'rule,' 'agreement,' and 'same' are internally related. Men agree in the language they use, they share a common way of conceptualizing experiences."

12 Phillips, p. 123:

The fact that we generally agree in our calculations is the result of how we learn mathematics, the way in which we are trained. As with any other language-game, the language-game of mathematics requires socialization into the rules, standards, conventions and grammar that one must master in order to play the game correctly.

13 Phillips, p. 141.

14 Here I refer to Schutz's formulations of the presuppositions of the natural attitude of daily life. See Schutz (1973, 1971). In another context, Phillips criticizes Schutz in the following statement:

The emphasis on the everyday language-game must, however, be distinguished from Schutz's[sic], interest in the common-sense thinking of everyday life and his idea that the reality of everyday life is the one paramount reality. For him, "there is only one external world, the public world, and it is given equally to all of us." In his view, there are signs and expressions which are meaningful and intelligible in their own right, regardless of whether anyone is thinking of them or using them. (p. 89)

Perhaps it is sometimes difficult to distinguish in Schutz's accounts whether he is stating a philosophical "position" or engaging in a kind of "paraphrasing" of natural attitude reliances on "reality." A comprehensive reading of Schutz's Collected Papers, should be sufficient to show, however, that his first-person explications of the natural attitude are not presented as his philosophical positions (i.e. that the "reality of daily life is the one paramount reality" in the natural attitude is not an ontological commitment for Schutz the philosopher.) Whatever faults Schutz accounts may be found to have, one of them is not that he subscribes to a "realist" version of science.

Schutz is relevant to this discussion in his discussion of "Commonsense and scientific interpretation of human action" (1973, pp. 3–26), wherein he elaborates upon sets of "abstractions, generalizations, formalizations, andidealizations" which are identical with common-sense knowledge of social structures. Although the attitude of "the scientist" is in some ways a distinct "province of reality" in Schutz's account, that attitude is also made possible as a use of ordinary language and ordinary reliance on an intersubjective world which indirectly invokes the presence of a social setting.

15 For an elaborate discussion of social science accounting practices which pertain to the issue of "deciding equivalences between events" in a way that "respects" achieved-equivalences, categorical identities and "natural" distinctions which are presumably "part of" social objects, see Bacchus (1974).

17 The distinction I have made between conversational analytic accounts and that of Phillips’s use of agreement is somewhat oversimplified. In the way I have constructed the conversational analytic account, I have provided for it as a “recognition” of some sort of naturalistic occurrence of agreement, independent of the analysts’ particular observational apparatus, or methodic “skills.” I cannot claim for conversational analysis that it is so situated with respect to its phenomenon that it does not exploit the analytic “distance” constituted in its use of tape-recordings and transcripts of “ordinary conversation”. Additionally, the relation of conversational analysis relation to “original conversations” is such that it provides an “overhearer’s” version of any conversation when the interpretation of utterances is from the perspective of how such utterances stand for “anybody” who might hear them rather than just those persons in the conversation (though a familiarity with the particular situation is gained and often comes to “inform” the analysis). The rehearability of tapes is additionally a resource for a skilled development of “hearing” by the analyst, and this “hearing” develops in detailed ways which are not identical to any “first hearing” of tapes, especially by naive practitioners of conversational analysis. How this is symmetric with the “hearing” of original participants in the conversation is open to question.

I mention these features of the analytic distance of conversational analysis not as a way of finding faults to be corrected, since those features are identical with the resources for finding “news” about conversation which is of such detail and complexity as to be unavailing without the use of tapes, systematic collections of “instances,” etc. All of these analytic operations mark conversational analysis as, in many respects, a constructive analytic enterprise, though with other such enterprises there is a strong claim for the discovery of features of conversation which in some manner “show” members’ activities are “oriented” to them.

However, the issue here is the existence in conversational analytic accounts of agreement of a unique recognition of the phenomenon in comparison to other social science accounts.

18 An interesting commentary on this property of analytic agreement which locates the work of finding consistencies between statements or activities in a kind of metaphorical history, is found in Harvey Sacks’s lecture on Agreement (unpublished lecture notes, UCLA, Lecture 23, 1966, pp. 8-11):

you want to remember that a notion that statements can be the same though the sentences are different is by no means an obvious one. And of course the essence of a notion of ceremony often is that exactly the statement – whatever that might mean – has to be redone, or failure will occur if the statement is powerful. And then, memory is the crucial matter, and memory has to be helped by very special techniques – most particularly the use of poetry as a mnemonic device. The forms of poetry become the ways that information is captured so as to be re-assertable in just the fashion it was first asserted. Now that’s not an independent discovery of mine, by any means. It’s the basis of Plato’s attack on Homer. When Plato attempts to reformulate the problem of education, the problem of knowledge for the Greeks, he launches in The Republic this very concerted attack on Homer, where he proposes that Homer is the storehouse of knowledge for the Greeks, but it’s lousy knowledge. And its effectiveness is given by the fact that it just gets built into everybody’s memory via the poetry, and if you’re going to have a progressive knowledge you have to undercut that form. That problem of Plato’s attack on Homer has been recently dealt with in some detail in a book called A Preface to Plato, though it’s about oral knowledge, by Eric Havelock, Harvard University Press, and it’s much worth looking at.

Now for matters that were so frozen, agreement has to be irrelevant, and the notion of agreement might never even emerge – it’s simply an issue of repetition. But in any event, for those kinds of things, it’s taken to be that the statements are powerful in some way or another, and if they’re followed exactly, then some set of consequences will occur. Now the emergence of a notion of agreement as a possible thing that can be sought and dealt with, is for purposes of available history, to be marked as an invention of Socrates, I suppose, for what was characteristic of his techniques were that he was willing to start with any statement that the person he was talking to held, and then bring him to see that that statement and some other statement that he had to be learned as a complaint. If you took it that what was said yesterday need have no relationship to what was said today, what was said in one context need have no relationship to what was said in another, or for that matter that persons were in any event well-formed, and ethics were consistent – whatever they were, they were consistent; then no such attack as Socrates launched could be made.

Now Nietzsche in his fantastic attack on Socrates – that’s a chapter in I think his book on Wagner, called “An Attack on Socrates” – sees just what it is that Socrates is up to, and treats him as destroying the Greek ethic – an ethic which didn’t involve people having to know what their views were, they just “acted” – as he puts it, and they didn’t have to have answers to questions as to what their opinions were. And when they had to, the society was sick and subject to the kind of undermining that Socrates wrought.

Now this phenomenon of inconsistency that he used, is a criterion which can have a kind of a strong appropriateness for
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a discussion, and yet of course, it was transformed into a total ethic, or a strong criterion of a total ethic. That is to say, one way to live one's life with a notion of consistency operating for any set of activities. Where of course, then the set of activities becomes things that are routinely transferable into statements, and the statements are to be compared. And by comparison of the statements one can decide their consistency, and if that's the use, then one can begin to see what relationship there is between, as he claims, knowledge and virtue. One was bound to be unvirtuous if one did not produce a set of activities which, once transformed into a set of statements, would be found to be inconsistent. And it's too much to expect that unless one has a statement, i.e., a set of rules, which one used in generating any given activity, one would not build consistent activities.

19 Analytic sociological accounts of agreement are a particular instance of what Harold Garfinkel (Lectures, UCLA, Fall, 1977) has called, “compliance production accounts.” “Compliance production accounts” are administered as sociology's way of assuring an amenability of ordinary social action to the apparatus of theoretical problem solving. This “device” consists of a postulation of ordinary activities as inherently “problem solving mechanisms.” Conceived of in this way, ordinary activities exhibit themselves as fertile grounds for the raising and solving of theoretical questions on how those activities achieve their observability. Phillips's account of agreement has this property, insofar as the achievements of science (discoveries, remarkably uniform procedures, disciplined lines of investigation) are described as a sort of “problem” which is solved through the provision for antecedent socialization mechanisms which brought members to a position such that they could “agree” on relevant scientific matters. Phillips's account is certainly plausible – I am not saying that it errs as an account of what may transpire in science – but it construes science in such a way as to distance itself from any concern with a science that can be performed or seen in its particular setting, and subsumes science within the antecedent conditions of a longstanding theoretical problem in sociology, that problem being how “actors” are brought into a condition of compliance to organized “social structures” of knowledge and behavior.

20 Detailed analyses of instances of agreement-relevant “yesses” in conversation show that “yes” is often more than merely an assertion of agreement. As Pomerantz (1975) and Sacks (1976) discuss, “yes” can do the work of “no” in some contexts. When I say that “yes” is an item which can be used in asserting agreement, I am not implying that it only does this, or that innumerable other ways are not available for doing so. Instead, I am simply noting that members can assert agreement instantaneously. They can achieve agreement on the spot, for the scene, and without any necessity for showing or analyzing “similar” behavioral items over the course of each other's conduct. The varieties of ways in which agreement can be asserted in conversation makes analysis of conversations considerably more difficult than simply searching for specific lexical items, such as “yes” or “no” and counting these as occurrences of agreement or disagreement. As Sacks noted (Lecture 23, p. 14):

Methodologically it's a tremendously troublesome thing to deal with, in that one needs, I guess, not merely a notion of “same” for those things about which the agreements can be, but a notion of “same” for the activities of agreeing. And that matter is normally simply supposed away, and the formula for its being supposed away are things like social contract theories, conventionality bases of everything, or some things, such practical tests as “if it turns out to work, then they must agree,” “they can talk so they must agree” at least on agreement, anyway. It is of course, supposed away in almost any public opinion survey which would say “do you agree on X,” assuming that whatever it is that “X” is may be problematic, but whatever it is that “agreement” is would always be countable anyway – treatable as “the same” for some purposes. And if it varies, then you could write “strongly agree” or “weakly agree” or whatever else.

21 Compare this situation to Wittgenstein's discussion of arriving at a solution for a puzzle (pp. 16-17):

42. There is a puzzle which consists in making a particular figure, e.g., a rectangle, out of given pieces. The division of the figure is such that we find it difficult to discover the right arrangement of the parts. Let it for example be this:

What do you discover when you succeed in arranging it? – You discover a position – of which you did not think before. – Very well; but can't we also say: you find out that these triangles can be arranged like this? – But “these triangles”: are they the actual ones in the rectangle above, or are they triangles which have yet to be arranged like that?

43. If you say: “I should never have thought that these shapes could be arranged like this,” we can't point to the solution of the puzzle and say, “Oh, you didn't think the pieces could be arranged like that?” – You would reply: “I mean, I didn't think of this way of arranging them at all.”

46. The new position has as it were come to be out of nothingness. Where there was nothing, now there suddenly is something.
47. In what sense has the solution shown you that such-and-such can be done? Before, you could not do it — and now perhaps you can.

In the case of the puzzle, the solution may not be obvious at the beginning, and may continue to elude the ongoing effort to solve it. Once the solution is given or "hit upon" it is usable as a strong demonstration that an initially unthought of solution was possible. Note, however, that one begins such an enterprise with an anticipation that a "solution" exists — i.e. that something can be done or "seen" which brings the solution into existence on that occasion (unless some nasty trick is involved in giving a puzzle without a possible solution). Once "seen," the solution appears to have been "there" all along ("why didn't I think of that?"). In scientific discovering work, no such assurance may be available. In the absence of a "solution" or "result" one cannot say without doubt that "I just haven't hit upon it yet." There is no external recourse to a solution; I cannot find someone to tell me the solution. The "puzzle" in such a case is not a demonstration which "convinces" me of its solvability. It has no such pre-available character. It remains to be seen what it portends for me. It is "hidden" — beyond the fact that I don't see it. No one else does either, at the time, or as far as I may know.

Agreement enters through several doors in such a situation: (1) We (the parties to the problem) collaborate in what could count as a possible "try" at a solution. (2) We agree on what to conclude from an unsuccessful "try" — do we try again in hopes that it will work "this next time" or try something else? (3) We agree on whether a "successful" result can stand for us as adequately demonstrated in our procedure, to say nothing of our collaboration on what the accountable visibilities might be that would make up a successful or unsuccessful result, (4) Given current results, we agree on what to do next.

22 Although scientists commonly claim that "agreements" are, at least in principle if not always in practice, based on a mutual recognition of the properties of the "things" studied, this claim does not preclude the analysis of such agreements as socially produced formations. Programmatic commentary on this matter is provided in Bloor (1973).

Bloor devotes the major part of his paper to establishing the relevance of sociological explanation to mathematical practices, a relevance which Mannheim's program for a sociology of knowledge did not provide for. Bloor asserts that sociology is relevant to mathematicians' work in more than the available provisions for (1) analyzing sociological antecedent variables which presumably explain how persons came to do mathematics, and (2) analyzing the "social" basis for errors and other deviations from logical or mathematical "truths," while having nothing to say about the "proper" conduct of mathematical practices. (pp. 178-9)

Bloor uses Wittgenstein's Remarks on the Foundations of Mathematics as a resource for claiming that any mathematical conduct (whether determined by practice to be "correct" or "erroneous") is based on convention, and thereby on socially structured "norms" of behavior. According to this account, very basic facts which are unquestionably part of ordinary mathematical practices are as much based on "agreement" as are historically located "erroneous" beliefs. In the case of the unquestionably accepted "facts" of mathematics, however, "agreement" is involved in the compliance of practices to the "facts" they achieve in any instance. The compelling character of the basic truths of mathematics (that if you dispute these facts or fail to accept them, then you are not doing accountable math and there is no basis for speaking within the language game) does not vitiate the conventional basis for the (invariant?) way in which those practices are performed, once they are mastered.

I have no argument with Bloor, insofar as he argues against a position in sociology that he defines as "realist," and uses a set of demonstrations from Wittgenstein to aid an alternative version of mathematical science. I take issue with Bloor, and with Phillips, on the matter of how the social basis of scientific practices are to be made explicit in an empirical study of actual scientific conduct. That science and math are proper subjects of study for a sociology, is not disputed here. It is argued here as well (in agreement with Bloor) that any sociology which leaves "adequate" scientific practices to a determinancy by the transcendental properties of "ideas" or "objects" is backing away from a fundamental issue in the sociology of science. The question then remains, what is this agreement; how is it to be elaborated, demonstrated, or detailed as part of actual occasions of scientific conduct? For this task we need more than an argument based on imagined or generalized notions of what scientists do in their daily work. We need to be able to locate "agreement" as a specifiable phenomenon which could be discovered through study of actual occasions of scientific conduct. Only then could a study develop which would be more than a matter of programmatically asserting that agreement is involved in science (through, for example, demonstrating the theoretic necessity for social agreement as a basis for uniformities in scientists' written accounts of objects in nature). Once we say that agreement is part of scientific work, we cannot be satisfied with having shown that a possible sociology is, after all, not to be degraded for its relevance to natural scientific enterprises. This is only a beginning. What then remains is to substantiate the claim about agreement by examining actual settings of scientific conduct (this, of course, is likely to bring about some modifications and perhaps an abandonment of the original claim). This does not necessarily entail a blind "data gathering" enterprise, but rather an effort to discover what "agreement" might be as part of the daily situations of scientific work.
7 Objects and objections: modifications of accounts of objects in laboratory shop talk

Introduction

The ways in which agreement is achieved in conversation are legion. An exhaustive account of those ways is an immense task which will not be attempted in this chapter. Instead, the chapter will place primary emphasis on one constituent feature of achieved agreement which is particularly relevant to the study of scientific shop talk. This phenomenon, "modifications of accounts of objects," will be presented here as a demonstration of a practical and social organization of accounts in scientific shop talk.

Briefly stated, the topic of modifications formulates the ways in which speakers change their descriptions or accounts of objects in the face of expressions of disagreement by others in a conversation. The ways in which such modifications are achieved can be specified in terms of systematic features of the referential formats which are used in assertions, counter-assertions, and reassertions. It is not particularly surprising that persons, in conversation, modify accounts of objects in the pursuit of agreement. Indeed, classic experiments in social psychology were designed to expose the fact that persons subsume the "objective" characteristics of objects in motivated compliance with the erroneous descriptions of experimental "shills." However, the point of view is taken in this chapter that re-descriptions of objects, in pursuit of agreement, do not necessarily depart from the "objective" features of objects. Instead, situations are examined in which the analyst has no independent or privileged access to the "objective features" of objects other than through the assertions of members. Accordingly, there can be no independent standard of "objectivity" with which to decide the correspondence of any member's account with its "real-world" object. This matter is necessarily left to the parties involved in the practical circumstances of any such determination. Furthermore, in the course of their assertions and reassertions, members report marvelous discoveries of features of the objects of reference which place the object "in a new light," and reveal the object as "supporting" what were initially asserted as discrepant accounts. In commonplace speaking situations, objects are not standards for discourse; rather, discourse provides resources for disclosing an objectivity in its elaborations of reference. The topic of this chapter is, therefore, not how members' pursuit of agreement distorts the objectivity of accounts in the service of agreement. Instead, the topic is how the talk of members establishes any "objectivity" for all practical purposes in the local setting.

Examinations of scientific shop talk in this chapter will point out instances of modifications in the accounts of working research colleagues. This will not suggest that such discursive practices erode the objectivity of descriptions. Rather, the analysis will demonstrate how "objectivities" are proposed, argued over, and "verified" as claims in an interactive setting. These arguments in shop talk do not commonly take the form of carefully reasoned proofs or empirical demonstrations of significant results. Instead, they are often brief and spontaneous disagreements over rather technical details which arise in the routine situations of day-to-day inquiry. None of the particular disagreements which will be analyzed here had monuemental significance when viewed in terms of the laboratory's accomplishments as a whole. However, the determinate course of the project consisted, in large part, of the unremitting achievement of such technical details in concerted work.

The following sections of this chapter begin with a characterization of a sequential environment in which modifications of accounts occur in conversation. Then a set of "devices" which were found to be characteristic of these modifications are outlined. Following this discussion of modifications in ordinary settings of discourse, line-by-line analyses of several transcripts of recorded conversations which occurred in a scientific laboratory will be presented. These running commentaries on the transcripts will focus upon particular modifications as they occur in parties' talk and will also touch upon varieties of other issues as they are identified with developments in the talk. The conclusion of the chapter discusses some of the consequences of modifications of accounts for scientific work.
Modifications of accounts of objects in conversation

Studies of conversation have shown that in various contexts initial accounts of objects undergo modifications across sequential turns of talk. Such an event occurs when a speaker makes an assertion about an object which is then modified in a subsequent assertion by that party or by another party in the conversation. In the utterance which modifies the prior assertion, some manner of reference to the object of the initial account is preserved while being reformulated in any of a variety of ways. This section will characterize some of those ways in which modifications are achieved before proceeding with an analysis of tape-recorded scientific conversations.

One of the ways in which modifications commonly occur in conversation is as same speaker modifications. Same speaker modifications are exemplified in the following instances of ordinary conversation which occurred in non-scientific settings, and which were tape-recorded and transcribed according to Jefferson's analytic conventions. In these instances, an initial account (a) is modified in a subsequent utterance (m) by the same speaker:

**Example 1 (TCI(b):16)**

(a) Linda: Well Steven's hair's a' same color ez Crai:g's,  
Joan: Is it?=  
Linda: =((falsetto)) Yeh=  
Joan: =I thought Craig's wz li:ghter.=  
(m1) Linda: =No:: I don't think so Craig's//hair isn:'t]  
Joan: =Ah=  
(m2) Linda: Jist about th' same color: //it might be a teeny bit:  
Joan: 'Yea:ah,  
Joan: t.hh Yeh//a]h  
Linda: But,]  
(m3) Linda: Just maybe streak:s,  
Joan: O//h (yeh)

**Example 2 (GTS 1, multi-copy version 1, p. 47)**

(a) Louise: You wanna know something funny? Whenever I go places I'm the first one there I'm never late,  
U-  
Al: Ha ha

(m) Louise: I am not! I have to wait for my parents usually (when-) I am al- I'm usually on time, here I'm always late!

**Example 3 (TG:1) (quoted from Pomerantz, 1975, p. 77)**

(a) B: .. Yih sound HA:ppY, hh  
A: I sound ha:ppY?  
B: Ye:uh  
(m) A: No::;  
B: N:o?  
A: No.

**Example 4 (IG:II.1-28) (from Pomerantz, p. 77)**

(a) D: If y'go tuh Switzerland yer payin about fifty per cent a' yer money on taxes.  
C: Not in Switzerland  
(m) D: No I think it i:s.  
C: hhh No::;  
(m2) D: Well yuh pay awful high ta(h)xes over there.

**Example 5 (SBL:3.1-8) (from Pomerantz, p. 79)**

(a) B: .. An' that's not an awful lotta fruitcake.  
(m) B: Course it is. A little piece goes a long way.  
A: Well that's right.

In the above instances, modifications are produced in the course of disagreement sequences. In the case of Example 5 the modification is produced subsequent to a lapse. In such a case a lapse is hearable as a "delay" or "withholding" of disagreement after the assertion, and as such it implicates disagreement. (We might also say that the lapse is hearable as a withholding of agreement, though in either case it is notable that subsequent to B's modification in Example (5), A immediately agrees with the assertion.)

Examples (1) through (5) can be sequentially characterized in the following way:

(a) An utterance is produced which asserts something in a rather definite and straightforward fashion in reference to an "object"
(“hair color,” the speaker’s “lateness,” the recipient’s “mood,” “taxes in Switzerland,” or “fruitcake”). When I say that the assertion says something in reference to an “object” I am not crediting such matters as “moods,” “lateness,” etc., with a status independent of the occasion of reference. I am, instead, pointing to an assertion’s inclusion of an object of reference in its design. Accordingly, I make no claim about the assertion’s relationship to some real worldly thing, or about the character of its information. When I speak of the “design” or the “format” of an assertion I am alluding to a structure that is constituted in the relationship of a modification to an original assertion; rather than being, for example, a set of independently discoverable logical or syntactic features of a first assertion. Once a first assertion is viewed in light of its subsequent modification (as in the above instances) it appears definite, unqualified, “objective” – as an “it is” sort of statement. These identifying terms (“definite,” etc.) are made relevant in the contrast provided by a characterization of the modification as “indefinite,” “qualified,” and “circumstantial.” (Here I am only speaking of the availability of the utterances to an analysis which can simultaneously compare a “first” assertion with a “second.”) A different analytic availability of these utterances is confronted in situ where any current assertion is temporarily prior (in the sense of “having priority”) to those which went before, and provides a basis for re-ordering the sense of the earlier utterances.6

(b) Immediately following the initial utterance, the assertion is challenged (or, as noted above, in Example 5 no agreement is immediately forthcoming subsequent to the assertion). In Example 1 Joan challenges with a question, “Is it?” and then makes a counter-assertion. In Example 2 Al produces a laugh subsequent to the assertion as a way of discrediting the account. In Example 3 a question displays “surprise,” and in Example 4 an explicit denial is produced.

(c) Subsequent to the challenge the first speaker modifies the initial assertion while preserving its reference.7 Such modifications are sequentially alternative to other possible “moves” following a challenge. Instead of modifying an initial assertion, speakers can reassert (as Linda does in Example 1 with “Yeh” following Joan’s first challenge). Speakers can also modify initial assertions by upgrading with definite modifiers and amplified tone of voice. Additionally, speakers can retract or reverse an initial assertion and thereby no longer hold to even a weakened version of their initial account. The discussion in this chapter is not particularly directed to characterizing a full range of sequential options which are possible subsequent to a challenged assertion. Instead it will specify some of the “devices” which speakers use in reasserting a prior assertion in light of an intervening challenge or potential disagreement by the recipient.

(d) In Examples 1 and 4 a modification is rechallenged, or weakly agreed to, occasioning further modifications by the first speaker. These further modifications appear to be serial “tries” at eliciting agreement in light of the failure of prior assertions and modifications to get a strong display of agreement.

Having provided a rough characterization of one sequential environment wherein modifications occur, this discussion will now focus on some formal properties of the asserted relations of modifications to the utterances they modify. There will be no attempt to characterize further the overall sequential structures of disagreements, as that task is beyond the scope of this discussion. Such matters as the varieties of conversational practices which are accomplished through the use of modifications; the uses of “other speaker” modifications in displaying disagreement or in correcting accounts, and the range of sequential alternatives to modifications which are available in disagreement sequences will not be examined. Although these matters may have a bearing on the organization of agreement and disagreement in scientific talk, the present analysis is limited to the implicative relations within one particular set of modifications (the analysis cannot go so far as to claim for this “set” a strong structural integrity in agreement and disagreement sequences since it has yet to fully characterize that “terrain”).8

The “designs” of modifications show some repeating features across different occurrences of their production. In a previous account of modifications in disagreement sequences, Pomerantz stated that “post disagreements and challenges, qualifying modifiers and suppositionals may be incorporated in reassertions.” Pomerantz’s examples and others chosen from transcripts of naturally occurring conversations will be used here to specify the character of modifications in terms of a set of “devices” which account for the work that modifications do in disagreement sequences. The devices are of particular interest here for what they “make of” the object of reference; i.e. how they redescribe the object or its relation to the speaker, and how those redescriptions show a sensitivity to a local context of agreement and disagreement. A listing of the devices follows:
(a) Redescriptions of the "object": expanding the "limits" of reference

Pomerantz analyzes Example 3 above in the following way:

B's initial assertion containing the term "HA:PPY" is reasserted in a modified form post A's disagreement. In the reassertion, the terms "sorta cheerful" replace the prior term "HA:PPY." The inclusion of the qualifying descriptor "sorta" is a weakening device. To weaken the reference, the reassertion changes the reference term while adding the qualifier "sorta" just prior to the modified term. Whether or not the modification is characterizable as a "weakening device" (it certainly provides a sense of being a "back down" in this instance), it is notable as a referential device which expands the limits of the original formulation. This does not mean that the original reference, "Yih sound HA:PPY" has clearly bounded "limits" of reference which are "expanded" in the modification, "You sound sorta cheerful?" Instead, the reformulation appears to provide a "hearing" of "HA:PPY" in light of A's denial of being "happy" which allows for A's being something less than ecstatic. Or to put this another way, the reformulation preserves the relevance of the original assertion while giving A another chance to agree, without there being a necessity for either party to retract its prior assertions. Pursuing the matter further with additional instances:

From Example 1:

(a) Steven's hair's a' same color . . .

(m2) Jist about the' same color//It might be a teeny bit:

In this instance the reference, "same" becomes "jist about th' same" in the modification (similarly, Linda's next try a modification, "It might be a teeny bit:" preserves the relevance of "same" while suggesting how "it" can be construed as not the same). The modification provides for "same" as not to be heard as exactly the same, but as roughly or approximately the same. In the original assertion it is not spelled out as to whether "same" is to be heard as "exact," or "approximate" in its referentiality. Indeed, had Joan agreed to the assertion there would have been no problem as to the meaning of "same," as the agreement would stand as a locally administered verification of the claim. Given a dispute over the term, the modification attempts to forward a hearing of "same" that would make it accessible to Joan's assent.

The point is that such terms as "same" are initially unproblematic as to their referential limits, though once their adequacy as a reference is disputed, they may be reformulated in such a way as to open them up beyond their locally disputed coverage. Similarly, in Example 2 Louise modifies from "I'm never late" to "I'm usually on time." Taking account of the inversion of the assertion from "never late" to "on time," the modifier, "usually," appears to weaken the original term, "never," by admitting exceptions. However, if the term "never" is regarded via the explication offered in the modification, it is "heard" not as an exact statement about an invariant behavior, but as a vernacular usage not to be heard literally or quantitatively. Accordingly, the modification can be spoken of as an injunction of sorts to the effect, "hear conditionally, not quantitatively."

In Example 4 an ostensible use of a "percentage" is disputed and reformulated in a non-numerical way:

(a) If y'go tuh Switzerland yer payin about fifty per cent a' yer money on taxes.

(m2) Well yuh pay awful high ta(h)xes over there.

Although it is the case that "awful high" is contextually variable (when considered as an estimate) in a wider variety of ways than is "fifty per cent," we can note that the rounded percentage figure ("fifty," rather than 47, 52, etc.) is by no means necessarily hearable as a measure or reported measure. Instead, "fifty" enjoys a usage in such a context which is not to be heard as specifying or interpolating a given numerical interval, but is a contexted expression for which accuracy may not be a requisite matter. However, the challenge of the assertion of "fifty per cent" by C makes how much a matter of specific relevance in the conversation, such that the reassertion ("awful high") marks its reference as specifically a circumstantial expression of quantity, where high is relative to assumptions about the run of taxes.

Reassertions which qualify initial references in the cases above exploit equivocal hearings in the ostensive use of the numbers and "exact" expressions. In doing so they expand the limits of reference by explicating a hearing of the initial reference as expanded all along - as not to be heard as a strict statement of identity or quantity but as a "specifically vague" or circumstantially adaptable expression.
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(b) “I think”: the display of agency, uncertainty, and ignorance

A common form of modification in disagreement sequences is produced through the prefacing of the reassertion with such phrases as, “I think,” “I guess,” “I suppose,” “I feel,” or “I don’t know, but . . .” In contrast, first assertions do not often use such “subjective” prefaces; they are produced as statements about some thing with no mention made of the relativity of the “thing” to the speaker or occasion of speaking:

From 1:
(a) Well Steven’s hair’s a’ same color ez Craig’s,

   (m) No:: I don’t think so Craig’s/hair isn’t

From 4:
(a) ... yer paying about fifty per cent a’ yer money on taxes.

   (m) No I think it i:s.

In both of these instances the speaker reasserts the claim of the first assertion, though with the preface, “I think” or “I don’t think.” Note that in the initial assertions no mention is made of the “agency” or “source” of the claim; instead, the claim is simply stated.

Rather than making too much of the “I think” format (as, for example, a circumstantial and vernacular “discovery” of the ego cogito) this discussion will explore what such prefacing accomplishes in the circumstances of disagreement. If it is granted that the “I think” preface is hearable as a qualification of an account, given that the account has been challenged, “I think” does not refer to the speaker’s “mental activity” or “self consciousness”; instead, it is used to mark the account as admitting of disagreement. Accordingly, the account is now not to be heard as noting an immutable fact, such that it cannot admit of the possibility of disagreement; rather, given the occurrence of disagreement, “I think” warrants the disagreement by admitting the possibility that the assertion could be later found to be wrong, or that it was simply one opinion among others - it provides for the possibility of contrary assertions. At the same time it acts as a format for reasserting the prior claim.

That “I think” is hearable as a preface which equivocates a position in an argument is not a matter of philosophic necessity (though one sometimes gets the impression in sociology that

“subjectivity” is to be mentioned only as a degraded manner of knowing) but of conventional formatting. (This is to say that an idealist philosopher who insisted on prefacing all assertions with the phrase, “I think” might come off as unassertive in ordinary conversation.13)

The mention of agency in a speaker’s reassertion of an account implicates doubt by locating the account with a personal source rather than with something anybody would say. In the following data fragment a mental patient vehemently protests the use of the preface, “you assume,” in the rephrasing of the patient’s account by a hospital staff member:

Patient: I do definitely err . . . get knocked up –
Mental Welfare Officer: – Yeh –
Patient: – As I say, at night . . . from ’ere other res residents ’ere but er, but – err (cough) . . . just a mo’ I want ta speak to ya’s clear’s possible err . . . but – oh’ this: I do want to say that err, you, you say to me that er I assume that er I get err woke up here by other residents.16 (from Coulter, Data Fragment A)

In this report of the mental welfare officer’s reassertion which the patient delivers, it is interesting that the patient takes issue with the preface “I assume” in his report of the mental worker’s rephrasing of the patient’s account. This displays a recognition of that modification as not, for example, a more careful statement of the observational account, but as a device for not accepting the account as it stands – as an expression or prefacing of a disagreement with the account. The mental welfare officer’s modification (or the modification attributed to the mental welfare officer by the patient) shows the work of attempting to preserve some elements of agreement with the original account while modifying only the terms of the patient’s relation to the account. Although the descriptive elements of the account are left intact, the patient finds the modification to be, practically speaking, a disagreement. A rephrasing of an account by a recipient rather than by the initial speaker has different consequences over how the modification can be heard to agree or disagree with the initial assertion, though in either case the modification opens up the account for renegotiation. Accordingly, it cannot be said that the statements themselves agree or disagree, though the statements provide the materials out of which agreement and disagreement are managed. Statements such as “I assume x” and “x,” when compared, show

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features both of agreement and of disagreement to which participants in the conversation have access in their work of displaying agreement or disagreement.

The preface, “I don’t know, but,” also appears in reassertions as a modification which “relativizes” a claim:

(a) N: Yah an’ an’ the fact that you’re you feel guilty about eating them that’s what makes you break out, because it’s- it’s all inside you.
H: So people who’ve broken out they’re just very emotional people, huh,
N: Heh heh heh, and they’re worried about it.
H: Heh heh heh heh

(m) N: I don’t know. It sounds kinda crazy, but:
H: hh just a little. (from Pomerantz) (JG:2.–3)

In the above sequence, the preface, “I don’t know. It sounds kinda crazy, but:,” acknowledges the disagreement expressed in H’s sarcastic utterance and laughter. The preface, however, is not a reversal of the prior assertion, but instead sets up a reassertion of the claim while also showing a sensitivity to the recipient’s disaffiliation from the claim. While this device might be interpretable as a “weakening” of the original account, it is regarded here as a speaker’s way of taking account of a disagreement while nonetheless going on to reassert the prior claim. Expressing “ignorance” is thereby a way to assert a claim while not insisting upon the assent of a recipient (assent is nonetheless “preferred”); the recipient can agree by merely admitting to the possibility that such a claim could be made without necessarily affiliating to that claim.

“Uncertainty,” in reference to a claim, can be asserted in other ways than by a preface which mentions the speaker’s relation to the account. That is, an account can be provided for not as what anybody would say, but as a possibility which is contingent upon a certain set of actions or circumstances. Such adverbial modifiers as “might,” “maybe,” “just maybe,” and “should” appear in reassertion of claims which were initially expressed in the “it is” format. (Other ways are available for expressing contingency or uncertainty, though the adverbs are very common.) Note in Example 1 that the modifications, “It might be a teeny bit:” and “just maybe streaks” employ the adverbial format in combination with other reformulations of the initial assertion to mark a degree of “uncertainty” for the claim about the character of the object.

Expressions of “uncertainty” have their interactional uses, as these instances demonstrate. Such expressions provide ways of accommodating a disagreement while re-offering an initial account. Furthermore, “uncertainty” (as a formatting device) is socially occasioned; it is expressed subsequent to a challenge, and is visible as an interactive device which implicates the sequential environment as well as the asserted relationship of the speaker to the object in question.

(c) Added accounts and explanations: discovered features of the “object”

One option exercised by speakers in a disagreement sequence is the reformulation of an account with the addition of an explanation. The explanation reformulates the “object” in such a way that it supports both the initial assertion and an ostensibly contrary position. For instance, in Example 1 Linda modifies her initial assertion about “Steven’s hair” with the assertion, “just maybe streaks”; and in Example 5 B reverses her initial account and then adds the comment, “A little piece goes a long way.” Note also the following instance:

(a) A: Un livers ‘n- an’ gizzards ’n stuff like that, makes it real yummy.

(1.6)

(m) A: Makes it too rich fer me::, but – makes it yummy.

(SBL: 3.1.–) (from Pomerantz, p. 80)

In this instance the speaker seemingly reverses the claim made in the first assertion with a modification. (Again, as in 5, we read the 1.6-second lapse as a silence which is disagreement-implicative). The manner of reversal, however, manages to preserve the prior account (that the livers, etc. are “yummy”) while “discovering” a quality of the object of the prior account which puts that account into a new relationship to the speaker (i.e. that it is “too rich” for the speaker). Though in the initial account the speaker appears to be expressing a liking of the food in no uncertain terms, in the modification she disaffiliates from the object through the discovered aspect of “richness.”

In Example 1 the modification m3 manages to preserve Linda’s initial claim (that Steven’s hair is the same color as Craig’s) while formulating the “hair” in such a way as to accommodate both her account and Joan’s disagreement. This is accomplished through the mention of “streaks” as an account of the object in dispute. Note that the “aspect” of the hair, whether or not “Steven’s hair” really is streaked, gets mentioned in light of the challenged assertion as a way of reconciling the assertion with the challenge. Accordingly, we can speak of “streaks” as a locally discovered
account of the object which became interactionally relevant (this is not to say that Linda was unaware of the "streaks" in the kid's hair prior to the occasion of interaction, but that within the conversation streaks became relevant as a locally occasioned "dawning of an aspect").

Similarly, in Example 5, "not an awful lotta fruitcake" is reformulated in such a way that a "little" is discovered to be "a lot." The account, "A little goes a long way" supplied the object ("fruitcake") with a quality not anticipated in a hearing of the prior account by itself, but which, once formulated, is not inconsistent with the object's docile character: the size of the object is not changed; instead, it becomes "more" as a relationship to its expressed usability. (Again in this case the speakers find fortuitous "loopholes" in expressions of quantity.)

Explanations are exhibited as practical inquiries into the "objective sources" of an actual or potential disagreement — inquiries which seek to remedy disagreement by locating a basis of misunderstanding in the object's analyzable characteristics. (Part of the work of achieving agreement from within a disagreement often involves formulating the disagreement as a "misunderstanding").)

As a feature of such locally occasioned inquiries, the character of the "object" in question is elaborated and redescribed. The explicature of the object assigns new qualities for the object ("new" at least for that local conversation) and/or its relation to the account; qualities or "aspects" which are not anticipated by any general formula, but which achieve an accountable consistency by subsuming the object within a social occasion.

Speakers often show great cleverness in providing for the object in consistent ways which nevertheless reconcile initially contrary assertions (this is not to say that parties always succeed in such work, as in Example 1, the explanation of "streaks" may not be particularly convincing to the recipient, as shown in the rather token agreement that results). In such explanations the object is not haphazardly reformulated but is provided with qualities, aspects, or implications which show a sensitivity to what a recipient (either that recipient in particular or "anybody" in general) might agree with. These exhibited implications are not always obvious specifications of a first-assertion, since they are occasioned in the specifics of a dispute (or a potential dispute) and are addressed to immediate issues of relevance as well as what "anyone" would readily describe as further specifications of the object. In other words, that "Steven's hair" might be described as having "streaks" becomes particularly relevant in light of the particular character of the dispute in 1. In the temporality of the dispute, "streaks" emerge as a retrospectively "rational" account of "Steven's hair" and not as a projectedly obvious implication of the initial assertion about the hair. It is in this sense that these explanations are construed as locally occasioned discoveries.

Modifications often show a combination of the above devices. In several of the above instances prefaces and qualifiers are used together in reassertions. Furthermore, in some instances several modifications occur in a series of reassertions within extended disagreement sequences; each successive modification providing for its object differently (though not haphazardly), while giving the recipient yet another chance to agree. Disagreement sequences are often closed with a display by the recipient that they agree with some sequentially prior account. Closure occurs when parties find some matter for asserting agreement — often a rather tangential implication of an initial assertion. This closure is evident in the fact that agreement displays are often rapidly followed by topic shifts, such that once something gets agreement by the parties involved, whatever was topical to a dispute is often dropped.

Modifications in scientific accounts of objects

The above discussion of modifications has elaborated upon a particular way in which descriptions of objects are sensitive to a local interactive context, a context wherein object accounts are reformulated in the pursuit of agreement. Relatedly, it has been suggested that achieved agreement (asserted agreement) acts to ratify a current account of an object, while disagreement extends the work of reformulating accounts in pursuit of agreement.

The potential bearing of this analysis of modifications on a study of scientific talk may already seem obvious to the reader. Insofar as vernacular speaking practices are an intimate part of the ordinary circumstances of scientific laboratory work, conversational devices used in agreement/disagreement sequences might be significant as ways in which scientific accounts emerge within a collaborative setting of inquiry.

In suggesting that scientists' accounts may be tied to the social setting of their production, however, it is not being claimed that scientists play "fast and loose" with accounts of objects while, for example, "conforming," in ways contrary to the ideals of their discipline. As already stated modifications are produced in a way that preserves the object of reference as an integral phenomenon, while explicating such matters as the speaker's relation to the object, remedially discovered aspects of the object, and the terms of expression used in the account. Indeed, the work of modification consists in explicating an assertion in such a way as to
subsume it within the ongoing interactive context without being found to be inconsistent. In other words speakers do not get away with just any reformulation of an assertion.

Focusing on modifications in scientific accounts does not necessarily lead to a discrediting or a doubting of particular scientific claims (or of science's claims in general), though "doubting" or "discrediting" become accessible as parties' actions in the course of scientific disagreement sequences. What this analysis provides is an entry into the operative social circumstances in the establishment of matters of "fact," "uncertainty," and "significance" in scientific laboratory work, without impugning the vertical character of those "facts," etc. to those who produce them. Additionally, the above analysis of discursive modifications provides some analytic insight into locally organized interactive discovering work, since some modifications can, in a particular sense, be called discovered-accounts of objects.

It is to be expected that scientific shop talk will exhibit vernacular speaking practices in a way indistinguishable from ordinary conversation. Indeed, preliminary examination of some instances of laboratory shop talk indicates that much of what was said in the foregoing discussion of modifications can be documented with instances of laboratory shop talk as well. Furthermore, in many other respects, shop talk exhibits conversational analytic organizations as an instance of ordinary conversations, and not as an organizationally distinct way of speaking. The intent here, however, is not merely to demonstrate this, but to point to a circumstantial analytic within scientific shop work which achieves the occupationally specific sense of the talk.

To show, for example, that scientists modify their accounts of objects in the face of the disagreement of their colleagues, says nothing about how such modifications are accomplished in displaying "reasonable" accounts of objects. To say that what counts as "reasonable" is whatever subsequently results in a display of agreement is to gloss over the interior character of that achievement. Nor is it accurate to explain how agreements are achieved by specifying an open set of rules for logical and rational discourse, which parties may or may not be attending when in the midst of argument. Members may indeed employ some manner of locally organized "logic," though this is not to be confused with formal logic - with generalized rules of inquiry or argument which operate to determine such matters as consistency without regard to the particulars of any sequence of assertions in a specific interactive situation.

The locally organized work of collaboratively produced object accounts will be treated here in an "instance by instance" analysis of selected fragments of tape-recordings of scientific shop talk. As yet, this analysis cannot claim the discovery of any general features which might be widely used among scientists as typical ways of asserting, arguing, or modifying accounts of objects. Nor has the analysis uncovered collections of discursive devices which might be distinctive of scientific talk. Nor is it established that such collections would be discovered by a more extensive and sophisticated study of scientific talk.

By examining the following fragments of spontaneously occurring laboratory shop talk my purpose is not to analyze the fragments for their correspondence to the three "categories" of modifications which have been outlined previously. Instead, instances will be analyzed in terms of how accounts were interactively achieved in those instances; not as a way of documenting a general structural phenomenon, but as a way of exhibiting how talk done on given occasions exhibits parties' collaborative achievement of accounts of objects. The issue of modifications will be pursued in this fashion as a way of providing access to practices which are reflexive to ordinary conversation as recourse to such practices is found in the specific circumstances of a scientific inquiry.

Summary account of instances

The following list is a summary account of the fragments of laboratory shop talk which are analyzed in the remainder of this chapter. It can be read as an annotated table of contents for the more extensive accounts of the data which follow. Analyses of instances 1 through 5 are directed rather specifically to a particular modification or series of modifications in accounts of objects. Instances 6 through 8 are more extensive transcripts of shop talk, and are not used simply to exemplify particular modifications. Instead, they are analyzed on a "line-by-line" basis. The commentaries are relevant not only to the topic of "modifications," but are addressed to a complex of issues pertaining to the interactive achievement of accounts in lab work. In these cases it was not feasible to isolate instances of modifications from the ongoing context of shop talk, as their involvement in more extended sequential and referential matters did not permit analytic extraction.

The instances are placed in the following order:
1. A modification during an interview. In the interview I ask a question which is occasioned by an electron microscopist's announced "noticing" of an ultrastructural phenomenon in the electron microscopic field. The question is treated as a challenge...
of the “noticing,” and the practitioner immediately denies the “noticing’s” claim upon its object.

2 “Tour challenges” (a) and (b). Two sequences are analyzed in which a visiting scientist challenges a lab member’s account of a biological phenomenon. In the modification of the account the technical character of the prior assertion is emphasized. The challenge brings out formulations of technical “agency” which provide for the initial assertion about the phenomenon to be a conclusion based upon the circumstances of its achievement.

3 “Tour challenge” (c). A visiting scientist takes issue with an explanation of an experiment by a lab member. In the challenge the phenomenon of microglia is mentioned as a feature of the explanation which is of disputable credibility. In answering the challenge the lab member separates the “visibility” of the experimentally demonstrated phenomenon from the troublesome name, “microglia” used in the prior explanation.

4 Light microscopy account: A light microscopist’s assertion that a collection of slides are analytically “the same” is questioned. Following the question, the account is denied and then modified to be “basically the same.”

5 Serial modification of an artifact account: In the absence of confirmation by the recipient of his assertions, a lab assistant serially modifies an assessment of some electron micrographs in his account of their purported artifactual basis.

6 “Mixing mix up”. Two lab workers negotiate over how a staining solution is to be mixed while explaining the procedure to a visitor from another lab.

7 E.M. Lab, Transcript A. Three parties involved in the ultrastructure project together review some newly assembled electron micrographic montages.

(a) A disagreement emerges over a characterization of some specific axon terminals as “small,” in which the issue becomes “small with respect to what?” The disagreement is remedied as a “misunderstanding” in which one of the contending parties is shown how he misread the montage display.

(b) A lab director accuses an electron microscopist of misshooting a row of micrographs. The electron microscope specialist counters by asserting that the trouble with the photographs is not a matter of the photography, but of the artifactual obscuring of the ultrastructural material, a less localized source of trouble in terms of its implication of responsibility for any error.

(c) An assessment of the micrographs as “bad” is agreed to, though with the account added that they are “numbers” (usable as data). This account is picked up in a reassertion of the initial assessment, while in the modification an account is given which separates the appearance of the photographs from their utility for the project.

8 E.M. Lab, Transcript B. Parties in the ultrastructure project engage in projections of “what to do next” based on the assessed adequacy of the materials they are currently analyzing. Arguments emerge over what the existing photographs show with regard to the temporal organization of axon sprouting, and over the implications of those materials for further work of demonstrating that anatomical phenomenon.

(a) One party asserts that axon sprouting occurs at a given time point (“five days”). This receives no immediate agreement, and another party begins what appears to be a “pre-disagreement” with the account. The topic is shifted by the initial speaker before the possible disagreement is pursued.

(b) Parties negotiate over the adequacy of “2½ days” as a “baseline” for the work of demonstrating axon sprouting in a temporal series of photographic displays. An initial “worry” about the adequacy of the “2½ day” photographic records is documented with particulars from one such display, and subsequently modified in light of a counter-argument with the same displays.

1 “Interview”

Background: An electron microscopist (J) is engaged in the work of analyzing a montage of electron micrographs while I look on and ask questions about how he does so. J’s current task involves locating accountable instances of axon terminals within the field of the photographs and marking these with color-coded traced outlines which denote “degenerating” vs “intact” axon terminals. The distribution of the terminal fields will later be plotted by tallying the relative frequencies of marked intact and degenerating terminals in given sectors of the photographic display. While doing this, J is also giving me instructions and responding to my questions and comments. Throughout the conversation/interview, I offer occasional comments to J on what I see in the sections, perhaps as a way of displaying my new-found ability to interpret the electron microscopic renderings of brain “ultrastructure.” These “precocious” comments occasionally lead to my being corrected by J and on some occasions lead to disagreements between us. These disagreements are not touched off as part of an “elicitation strategy,” though they sometimes result in rather interesting accounts and explanations. The following account is one such instance:

1 M: Ther no:t, clear there= 
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2 J: =Th'thing is's thee ehm
3 (1.5)
4 J: This is garbajhe- Ooh there's one right there!
5 (2.0)
6 M: Is: it? =
7 J: =Wehh I dunnuh
8 M: ^'Nuhhh (thet) doesn' look like vesiculs.
9 (3.0)
10 M: Hhlooks more like a spine er s'm-
11 (1.0)
12 J: Mm well it would be one of two things hh I guess those could be microtubules cut et an angle so (we won't) circle it. (th' when w')

Analysis: just prior to the above fragment, J has asked me if I can “resolve vesicules” in the photographic depiction of axon terminals. He has complained that the micrographs are of poor quality, such that the rather tiny synaptic vesicles (whose distinctive circular appearance in the cross-sectional rendering are invoked in identifying instances of axon terminals) are blurred and difficult to distinguish. In (1) I complain that the “vesicles” to which J is pointing are not “clear.” J credits this complaint by re-emphasizing that the photographed section is “garbage” (2-4) but cuts this account off in (4) and proceeds to announce that he has located yet another “one” (axon terminal). Presumably, he finds the material adequate to his work of finding axon terminals (and their associated vesicles) despite the usability of the account of the material’s blurriness for explaining my difficulties in doing so.

In (6) I challenge J’s “finding” with the question, “is: it?” and J immediately backs off from his initial claim with the modification, “I dunnuh.” This modification can be analyzed to show a sensitivity to the prior account within the interview. While the first utterance is produced as a spontaneous noticing and is by no means a qualified or ambiguous announcement the micro-discovery, once a question is raised, the “for the record” accountability of the “discovery” comes into play and it is completely backed away from.

In (8) I express a counter-assertion to the effect that what J has noticed “doesn’t look like vesicules.” In addition to reiterating the challenge and specifying a counter-proposal, the utterance is distinctive in the way it uses the phrase, “doesn’t look like,” rather than a more “positive” format (e.g. “isn’t”) in locating the disputed referent. The mention of the “looks” of the thing in an assertion shows an interactive sensitivity to the occurrence of the utterance as part of a disagreement. In this and in other instances speakers employ “appearance” terms as a way of providing for their claims relative to contrary assertions which find in the “looks” a different “thing,” while in assertions which are not challenged or which do not act as challenges such formatting occurs less often than “it is” or “there is” accounts.

After a rather lengthy pause (9), I offer the further assertion that the phenomenon in dispute “looks more like a spine.” A spine is anatomically characterized as a dendritic organelle which synapses with axon terminals, but which is a distinct ultrastructural phenomenon from axon terminals. After another lapse (1.0), J tries to resolve the disagreement by stating that the phenomenon could be one of two things. One of the “two things” is presumably the axon terminal indicated through J’s disputed recognition of vesicles, which J has mentioned previously. In (12) he mentions the alternative account (voicing it in the conditional as a “guess” – thereby committing to it only as a possibility), which is that the “vesicles” may be “microtubules” cut at an angle so as to appear like vesicles. In saying this, he suggests, in partial agreement with my account in (10), that the tissue section may have been sliced at an angle so that dendritic microtubules would appear larger than they would have been had they been cut cross-sectionally, and would thereby appear somewhat like the larger synaptic vesicles. This account provides for the photographic evidence as possibly showing the presence of dendritic material (though not necessarily a “spine” as suggested in my earlier account), rather than the originally asserted axon terminal.

J then goes on to state that “we won’t circle it,” which assesses the disputed phenomenon as ambiguous and not to be counted analytically as an instance of an axon terminal. (“Circling” refers to the action of tracing the outline of a terminal with a felt-tipped marker, which marks a visible phenomenon as an adequate instance to be included in the statistical data on terminal distribution and density in the observed tissue.)

J’s modifications in (7) and (12) show a sensitivity to my challenges in at least two ways. First, in (7) a definitely stated object account is reformulated as “uncertain” in light of my questioning of the account. Second, the explanation in (12) incorporates (at least partially) my counter-assertion about what the disputed phenomenon “looks like” into an account of the two possibilities that the thing might be. It appears that the ambiguity of the object’s appearance was interactionally occasioned, since the inquiry into what it could be is touched off by an expression of disagreement over an initial account – an initial account which made no mention of alternative possibilities until one was mentioned subsequently.
The explanation in (12) does the work of preserving the initial account (of "vesicles") while providing for a "reasonable" alternative, given the circumstances. The initial account is not abandoned, though it is "relativized" in its expression. Note the practical effect of the disagreement on the enterprise of tallying axon terminals: a possible item of data was not counted due to the interactive emergence of ambiguity. I can only speculate over what might otherwise have happened in the absence of a challenge to the initial "noticing," though I figure that the terminal would have been counted had an alternative reading not been expressed.

2 "Tour challenges" (a) and (b)

Background: in the previous instance and in other instances in which lab members provided accounts of their work to outsiders, assertions were modified in such a way as to display "careful" and "qualified" speaking. Challenges by visitors sometimes occasioned reconstructions of prior assertions which used a format of "carefully reasoned" argument in which the claims of initial assertions were reduced to the witnessable "facts in hand" and mention was made of the technical agency of the conclusions and findings which were presented as claims in prior assertions. These responses to challenges from outsiders displayed more stereotypic features of "scientific argument" than did scientific shop talk amongst working colleagues. It would seem that challenges acted as injunctions to "speak scientifically" in the way that scientific writing shows a familiar and distinctive format, though in the case of modifications the format was not available as a consistent "style" throughout a continuous discourse, but was instead produced in an ad hoc fashion as particular assertions touched off in specific sequential environments.

The following instances of tour arguments were recorded on the occasion of a visit by a brain scientist from another laboratory to the lab investigated in this study. The visitor was quite contentious in his reactions to accounts of the lab's findings during his tour of the lab, and three of his challenges and their subsequent treatment are reproduced here. I shall first discuss two of the challenges and the accounts they occasioned which showed a somewhat similar organization. A third sequence is discussed separately in item 3 of this list.

Instances (a) and (b) occurred in the course of an explanation delivered by H (the lab director) to V (the visiting scientist) on how a particular experiment was performed. The experiment involved the removal of a "slab" of hippocampal tissue from the animal brain, and the transfer of that tissue to a chamber where the tissue was kept in vitro for up to twenty-four hours after its removal from its living conditions. For this period of time (when the technical work involved was successful), electrophysiological "readings" could be taken off the neurons, and the glia cells showed detectable activities (such as "migration" and "division") which were hypothetically associated with the in vivo brain's recovery process from injury. The experiment being explained in (a) and (b) involved the use of radioactive thiamadine as an indicator of whether microglia cells underwent meiosis while the brain was in the in vitro state. Thiamadine was known to be associated with meiosis, and it was used in assessing whether the microglia cells would "take up" relatively large amounts of the trace chemical and register prominently upon an X-ray photograph (the thiamadine having been rendered radioactive for the purpose of the experiment).

The experiment was used in suggesting a possible "role" of microglia in the "reaction" of the brain to injury. It was reasoned that a "proliferation response" (the widespread meiosis) of the microglia in reaction to experimentally induced brain injury documented an as-yet unspecified "role" of the microglia in the brain's response to injury. Furthermore, the observation that the "proliferation response" occurred in the in vitro condition was taken as a demonstration that microglia were endogenous constituents in the brain, and did not arrive in large numbers from the bloodstream on occasions of brain injury. In the course of the account the visitor (V) challenged the lab's director's account on certain particulars:

"Tour challenge" (a):
1 H: (We) take it out'n (we maintain it) aloive. 'n' well thers a specially designed chamber uh'll h'f teh show :t tw yeh.
2 V: (Uh yew: said) aloive
7  (0.4)
8  H: These cells will divide

Analysis: in the first instance, H is telling the visitor about a “chamber” apparatus which was developed in the lab for maintaining slabs of brain tissue “alive” in an in vitro state. In line (1) H makes the claim that “we take it (the brain tissue) out ‘n we maintain it alive,” and then goes on to refer to the chamber apparatus developed in the lab for that purpose. The chamber is mentioned as a particularly noteworthy achievement in the lab, a way of marking the absence of any appreciation shown by the visitor to the mention of the chamber in (1).

In (4) the visitor questions H’s use of the term “alive” in (1) and laughs in emphasizing the question’s challenging character. This challenge displays no appreciation of H’s remarks on the “technical breakthrough” since it locates a disputable term in the account prior to the mention of the chamber. In doing so, V questions the claim which H made prior to his elaboration of the technical basis for the claim. In (5) H answers the challenge by modifying “alive” to be “alive by these kind of criteria.” H does not immediately elaborate upon what these “criteria” are, though I take it that he refers back to his mention in (1) and (3) of the particular technical agency which is used in maintaining “viable” tissues. This is borne out a few turns later in the conversation where the visitor displays his understanding by stating:

V: ((loud stagey voice)) They’re able to maintain it et least so yew c’n get potentials en’ stuff.

In the modification the disputed term, “alive” is reformulated as a term with a relatively limited technical “scope” which is tied to particular conditions of methodical observation and agency.

In “tour challenge” (b) the visitor asks a question about where “they” (microglia cells) come from in (1). H answers in (2) that they are an “endogenous population” and begins to explain that account by reference to “what happens” when “you” perform a lesion which effects the hippocampus. (Subsequent to the lesion which “deafferenates” the hippocampus, microglia cells were observed to “proliferate” within the brain.) The visitor cuts off this account in (3) and challenges H’s use of the term, “endogenous.” Again, as in the previous instance, the challenge does not address an intervening explanation and challenges a term used in a claim prior to the explanation (in which the “conclusion” was presented prior to the account which supported it). Subsequent to the challenge, H rather briefly explains an experiment supporting the claim that the microglia are “endogenous.” The experiment is explained in more simplified terms than in (2), replacing “deafferenate the hippocampus” with “take the brain out” and going on to use rather “plain” terms in the “methodological” account. It can be conjectured that this simplified format is responsive to the lack of any show of appreciation by the visitor to the initial account given in (2).

Note that in both (a) and (b) a claim is made on behalf of “brain cells” (they are alive, or they are endogenous). This is then explained in terms of the technical agency of the claim’s understanding (“you take the brain out”; “there’s a specially designed chamber”). In these instances the agency is expressed in a rather impersonal format, as a “method” (with an impersonal “you” being used to mark the “anonymous” character of the agency – though in this case the experiment was unique to this particular lab’s researches at that point in time). What is challenged is the claim’s use of descriptive terms about the cells which can be heard as referring to more general properties of the cells independent of the circumscribed test system which exhibited those properties. The challenges are addressed by providing an understanding of the terms, “alive” and “endogenous,” which refers them specifically to the technical conditions of their agency. The modification accomplishes a sort of injunction to “hear ‘alive’ as a technical term” or to “hear ‘endogenous’ as circumstantially demonstrated.” In both cases, the challenges “drive” the speaker into a careful technical explication of what was initially stated as a worldly matter. In these cases the challenge may be relevant not so much to the “facts” of the challenged assertion as to the manner in which those facts are to be displayed interactionally as an instance of speaking accountable science. (Note that in (b), H provides the technical account in lines (4-8) but in a simplified format, perhaps as a “snub” of the visitor’s insistence that he speak like a scientist.)

3 “Tour challenge” (c)

Background: the following sequence occurred during the same “tour” as did “tour challenges” (a) and (b). In this sequence H is explaining the experiment on the “microglia proliferation response” (see “background” for (a) and (b)). The visitor again takes somewhat delayed issue with a term used in H’s explanation, the term in this instance being “microglia.” The visitor raises two objections to the use of the term. First, he makes reference to the fact that “microglia” are a questionable class of glia cells, as
some electron microscopists have claimed that no reliable anatomical basis exists for distinguishing “microglia” from another glial cell type, “oligodendrocytes.” Second, “where microglia come from” is a subject of some controversy in the brain science literature. The cells are known to show up in large numbers in the brain shortly after the occurrence of injury to brain tissues. One theory holds that microglia are carried in the blood stream much as white blood cells are, and are transported to the site of damage after a brain lesion. An alternative theory, which H is supporting in his account of the thiamadine experiment, holds that microglia are an endogenous constituent of the brain and undergo cell division within the brain. A possible chemical “mechanism” which triggered the meiosis (and perhaps other events after a lesion) was sought in the lab’s experiments with the in vitro test system.

Analysis: in lines (1) through (5) H explains the experimental result to V, with the explanation being interspersed with lapses in (2) and (4) in which the visitor does not take the opportunity to comment upon the explanation, or to give token shows of “understanding.” After another lapse in (6), the visitor asks a question that appears not to have attended H’s account in (1) in which he already talks about the demonstration of the “microglia response.” In (8) H inserts the question, “in vitro?”, perhaps seeking further specification from the visitor on what he could be asking that has not already been answered. The visitor answers in the affirmative, and H inserts a further question, “proliferation response?”, again perhaps in search of further elaboration of the question (“Proliferation response” replaces the reference, “microglia response,” thus specifying the cell division of microglia, and not other possible “responses” of microglia such as “migration” in the brain).

The visitor does not respond to the request for clarification, and instead pursues a more challenging line of questioning in (13), using the “How do you know?” format of a challenge and pursuing a matter which previously had been rather tangential to the account being given of the experiment. The experiment was not recounted as an inquiry into what microglia cells were or into how they might be recognized, though the account used the term “microglia” as a name for a central phenomenon in the explanation of the proliferation response. The question uses a format of “unmasking a presupposition” in the way it addresses the term as now a problematic matter. In (15) H postpones an answer with the token, “uh::h,” while showing that an answer is forthcoming, and V pursues his question further during the delay. In a very loudly voiced utterance in (19), V speaks on behalf of the literature and displays his knowledge of the controversies attendant on the use of the term “microglia” (21).

In (22), H, no doubt thoroughly annoyed by now, curtly dismisses the relevance of the question for his enterprise. Here he may be alluding to the fact that lab members were satisfied as to the facticity of “microglia” for the practical purposes of using the phenomenon as an element in a system of explanation, and that their local demonstration of the phenomenon overrode any continuing controversies which remained viable in the literature.
V reasserts his statement on the "importance" of the "question" of microglia in (23), and in (25), H reiterates his prior denial - this time not so abruptly or earthily. As he proceeds with the denial, H formulates the dispute as a matter of the use of a name. He then goes on in (27-9) to assert that by any other name the phenomenon that is being called "microglia" is experimentally demonstrated. This modification of the term "microglia" accomplishes an analytic isolation of the term as a term (and not, for example, as a "thing"), and once this is managed the term is separated into its use in a widespread literary account of "microglia" and its use as a name for a demonstrable feature of neural recovery in the lab's particular researches. The modification throws out the term but seeks to salvage the entity by separating the act of referring from the word used in accomplishing that reference. H then (in 28) uses a visible account of an object (a microscopic photograph in which the referred-to cells are labelled with a "Del Rio Hortega" staining procedure) to display the phenomenon in question. The "little things" thereby stand as definite objects, despite controversies over whether they are analytically distinct as a glia type, and over where they "come from." Accordingly, through the devices of the modification, the thing claimed is referred to as an independent referent to the particular term originally employed.

H's accounts to V in this instance are not only analyzable as explanations. They exhibit other relevances as well. Consequently, H's account will not be analyzed here in terms of whether it can be assessed as a "good" or "bad" explanation. The visitor asks his questions and offers his challenges in the above sequence in a loud and adversarial fashion, and the replies he receives are not so much "rational" as they are expedient in shutting down the visitor's attack. It appears that H for the most part drops any concern for the "properly scientific" appearance of his replies in the interactive situation, and his answers are analyzable as thrusts in a verbal combat as well as explanations. In other sequences, such as (a) and (b) of this same tour, H's explanations are recognizable as conventional displays of "speaking scientifically." In this particular sequence H's utterances are not primarily relevant as complete and detailed explanations. In other sequences, such as (a) and (b) of this same tour, H's explanations are recognizable as conventional displays of "speaking scientifically." In this particular sequence, H's responses to the account of the thiamadine experiment that he misses much of the detail that is provided. Furthermore, it may be the case that H has no great stake in honoring V with a precise and carefully constructed argument (though, whether or not this is so, it appears that in the interaction between H and V that there are local displays of disattention to the other speaker's accounts).

4 Light microscopy account

Background: in this sequence a post-doctoral researcher from Germany (R) and a graduate student research assistant (J) are collaborating on a light microscopy project. At the time of this conversation, R is examining a slide of brain tissue to assess a pattern of "labelling" in a particular brain system. "Labelling" refers to the action of a staining compound which bonds with selected chemical constituents of the tissue and renders them visible (where previously they would be transparent or only dimly contrasting with their surroundings). Opaque "labels" were used to bond with proteins on the surface of membranes as a way of revealing the configuration of those membranes. They were also used in experiments in which a given biochemical substance would be introduced into a cellular region, and the subsequent "uptake" and "transport" of the compound would later be traced in microscopic examinations of the distribution of the chemical's visibility within the anatomical region. In this sequence the action of a "label" on the "singular cortex" was taken as a significant indication of a particular neurochemical system. The sequence exhibits an interactional use of an artifact account and a modification on what counts as "the same" for the purposes of the inquiry:

1 R: . . . In the context you've got . . . richly labelling in the (singulate)
2 (3.0)
3 (): Huh?
4 J: Mm  mmm
5 R: I don't know whether there's an artifact
6 (1.0)
7 R: But the
8 J: Check the other sections.
9 R: They're the same.
10 J: They're the same?
11 R: Uhhmm noh.
12 (1.0)
13 R: Noh uh but uh but basically the same, yes.
14 J: There is something in- in th- in the singular cortex?
15 R: There is something
16 J: Well.
17 (1.0)
18 J: That would make sense.

Analysis: in (1) R announces a "fact about the object" he is currently examining that it shows "labelling" in the "singulate." No immediate reply is made to this announcement, though in (4)
J provides a weak token of understanding. Subsequent to this, R mentions a possibility of artifact (5). R's mention of the "artifact" possibility marks the prior announcement as less "certain" than might have been inferred from the original statement about the locus of the "labelling." The artifact possibility raises the question of whether the label reveals an interior anatomical or biochemical phenomenon in the brain system, or whether it identifies more or less irrelevant structures. R's mention of the possibility shows a sensitivity to J's lack of commentary on the original announcement and may be "searching" for an expression of affiliation or disagreement with the prior account.

J does not immediately comment on the artifact mention, and R begins another account in (7), perhaps a counter to his prior mention of the artifact possibility. J cuts this off and instructs R to "check the other sections," referring to a collection of slides of the experimentally treated anatomical area. Doing so would allow an assessment of whether the particular instance R is examining is consistent with the others. This, of course, would not dismiss all possibilities of artifact, but J is suggesting it as a check for any particularly unusual configurations which would single out the slide in question as strange or unrepresentative.

In (9) R asserts that "they're the same"; showing that he has already examined other slides in the collection and can assess them as supporting his assertion in (1) made in reference to the particular slide currently being examined. In (10) J repeats the assertion with a question intonation, thereby challenging the assertion. R subsequently denies the prior assertion in (11), though he makes the utterance in such a way as to project that more will be forthcoming. (The "denial" is thereby constituted as a "preface".) In (13) R continues with his modified account, restating the preface, "noh uh," and then following it with the claim that the object in question is "basically the same."

The modification accounts for how the slides in question were not "the same" under every examination but were assertedly "the same" for the purposes of the work at hand. The modification does not explicate the sources of its claim that the slides are "basically the same"; instead, it takes the authority of claiming that for practical purposes, the slides can be treated as "the same" under the thematized aspect. J in (14) then rephrases R's claim in a question format, as a way of requesting further confirmation that the determination made from the one slide can stand on behalf of the collection of slides. The question refers to the "singular cortex" rather than the analytic conditions of "the sections." This is a shift from a confirmation about the adequacy of the slide as a document to a confirmation about the phenomenon that J's analysis and R's provision of slides in different orientation and preparatory conditions is not "the same" for the purposes of the work at hand. The modification accounts for how the slides in question were not "the same" under every examination but were assertedly "the same" for the purposes of the work at hand. The modification does not explicate the sources of its claim that the slides are "basically the same"; instead, it takes the authority of claiming that for practical purposes, the slides can be treated as "the same" under the thematized aspect. J in (14) then rephrases R's claim in a question format, as a way of requesting further confirmation that the determination made from the one slide can stand on behalf of the collection of slides. The question refers to the "singular cortex" rather than the analytic conditions of "the sections." This is a shift from a confirmation about the adequacy of the slide as a document to a confirmation about the phenomenon that J's analysis and R's provision of slides in different orientation and preparatory conditions is not "the same" for the purposes of the work at hand.
he sees the sense of the claim without there being a necessity to explicate it any further. In doing so he displays an option not to argue further, i.e. to validate the claim *locally* by closing off further inquiry.

5 Artifact account

**Background:** two electron microscopy specialists are assembling a montage of recently developed electron micrographs for use as data in the ultrastructure project. In the course of this work, J complains of the poor quality of the photographic rendering of the ultrastructure, and the parties subsequently speculate over what the artifactual basis for the trouble might be. Because the material does not become accountably visible except during particular phases of the lengthy process of rendering the animal available for electron microscopic viewing, assessment of the adequacy of the work sometimes occurs only after the entire sequence of preparatory procedures are completed, such as in this case. As a result, an inquiry into sources of artifactual trouble takes the form of a “historical” interpretation of the visible record, elaborated by the practitioner’s remembrance of any events in his prior experience with the particular animal his practices rendered.

In this instance the parties express disagreement over two related matters: (1) how “bad” the photographs appear to be in terms of their analytic usability, and (2) what particular source in the standardized phases of the project of rendering the brain available is implicated in the assessed “badness” of the photographs. The following sequence shows particular ways in which the inquiry into the sources of the trouble is organized interactively:

1 J: (Heh wh- lets) j'st put-together an figgure it out then. (Ah w'll this's) kinda f' backwards way t' do it.
2 (1.0)
3 J: I guess w're ( ) any animal anyway.
4 (1.4)
5 J: Ther' *dog*: shit fixes.
6 (3.0)
7 J: 'Ss too bad.
8 (2.5)
9 B: Achshully id *looks* prettty go:od yeh know?
10 (0.8)
11 B: Meybe iss-
12 B: Meybe it just di'nt pick up theh *stain*.
13 B: ( ) Achshully not theh bad.
14 (1.7)
15 B: Meybe sum'n else went wrong=
16 J: =Yeh than cud be- it cud be: uh s'm
17 J: Some sort of a-
18 J: A maybe (its in da)
   (it stained uh)
19 J: Yeh know what (h) *can*: ses ziss I think it stays in theh *lead* too long.
20 (6.0)
21 J: I wuz told thet it-
22 J: It c'n cast a blur over it.
23 B: Hnn hun
   (0.8)
24 B: Yeah that- that- that's a ( )
25 J: Yeah
26 (0.7)
27 J: 'T jest looks like shit.
28 (1.1)
29 J: 'S looks like theh *do:g*.
30 (0.7)
31 J: W'll I'll paste these (mothehs) together, what the hell.

**Analysis:** in (1) J proposes that he and B proceed with assembling the montage, despite the as-yet-undetermined artifactuality of the photographs. He mentions this as a “backwards” way of proceeding, since the data are being assembled into a presentational format prior to any final assessment being made of their adequacy. In (3) J adds that in the project they are using “any animal anyway.” This account acts to disclaim the immediate relevance of whatever trouble might be found with the photographs for the practical purposes of the project. (“Animal” is used here to refer to the analytically and instrumentally rendered brain tissues from a particular, long-dead laboratory rat.) In (5) J assesses the materials as “dog shit fixes,” implicating the work of fixation in the assessment of the photographic visibility. Fixation is a procedure in which the brain is soaked in a fluid which stabilizes the degenerative process of the brain shortly after the “sacrifice” of the animal. Presumably, J is asserting that for some unexplicated reason the fixative did not preserve the tissue adequately, and the photographs show indistinct membranal outlines of ultrastructural phenomena. In (7) J makes the further assessment that “it's too bad,” remarking that the trouble is an unfortunate happening regardless of what may have caused the trouble. Note that so far B has not commented upon any of the prior assertions and assessments, and has not shown any affiliation to those utterances.

B finally does speak in (9) countering J’s prior negative
agreement in laboratory shop talk

assessments. The counter-assessment is qualified with "looks" and the question-intonation in "yeh know?" "Yeh know?" tags on a request for confirmation within the assertion format, such that the assertion is provided with a contingent character, the contingency being supplied in the projected acceptance of the account by J. J does not reply immediately, and B modifies his account in (11–12) by tentatively suggesting (with "maybe") that the trouble arose with the "stain" not taking. I speak of this as a modification of the prior account in the sense that in (9) B assesses the photographs as "pretty good," but in (12) elaborates upon a troublesome aspect of the photographs in partial affiliation to J's prior negative assessments. However, the "stain" account in (12) is placed in disagreement with J's "fixation" account in (5). Furthermore, the preface, "maybe it just," marks the staining problem as relatively less troublesome than might be imagined, and, relative to J's mention of "fixes," acts to downgrade the significance of the artifact. An ultrastructural rationale can be conjecturally supplied for this, since a stain's not having taken would affect the visibility of the rendering's contrastive features, but would not distort the membranal configurations themselves as would the degradation of tissues due to a fixation artifact. The staining problem would thereby provide a dim, though relatively authentic, rendering compared to the fixation artifact.

In (13) B forwards another assessment of the photographic displays. This utterance, "( ) achshully not that ba:d." reasserts the assessment in (9) with the modification of the phrase, "pre:ttty go:od" to "not that ba:d." This may be interpreted as a weakening of the initial assertion in the face of the absence of any assent by the recipient to the prior assessment and account of the photographs in (9–12).

After another lapse, B modifies once again by stating that "meybe" something else went wrong. This modification shows a sensitivity to J's non-display of agreement to the prior account of "staining" and provides J with an opportunity to now give an alternative account of the trouble. The modification locates the prior account as a speculative "try" at determining the "source" of the artifact; a "try" which became fragile in the face of no agreement by the other party to the inquiry. The modification in (15) acts as if to say, "OK, now you try, if you don't like this one," and "hands" the turn to J.

In (19–22) J does not discredit B's account of the artifact, but partially affiliares to B's account of staining trouble. His agreement is qualified in a number of respects. However, in (16) J uses the phrase, "that could be" in qualifying his initial token of agreement, and when giving an explanation of how a staining artifact may have arisen in the practices of soaking the tissue sections in a lead solution, he employs the prefixes, "I think" (19), and "I wuz told" (21). These qualifiers display J's account as a construction of a "plausible" explanation of how a staining artifact may have been "responsible" for the trouble, while leaving J uncommitted to a strong affiliation to the account. In addition, agreement is qualified in the details of the account itself. In (19) J mentions as a trouble source that the section of tissue can stay in the lead solution "too long," with the result that "it c'n cast a blurr over it" (22). The "lead" is the element which bonds with membranal proteins, resulting in a contrast between membranes and their surrounding protoplasmic environment. Leaving the section in the stain for "too long" a period of time is cited by J as creating a "blur," presumably because the lead would deposit so heavily that it would smother finer order contexts of membranes. Therefore, although J has picked up from B that the problem may be a staining artifact, his account provides for the artifact as a matter of too much stain, while B's, in (12), was a matter of not enough stain (the tissue may not have picked "up theh stain"). J has presented a contrary account of the artifact through the vehicle of an agreement with B's prior account. More importantly, however, J's displayed "reasoning" in (16–22) borrows (or is touched off by) some of the referential features of B's prior account, and in that way is an interactonally contingent account of the artifact. This is to say that the source of the artifact arrived at here would not likely have been referred to had B not previously invoked "staining" as a possible locus of the trouble (recall that J had previously focused on fixation as the trouble source).

In (23–4) B expresses rather tentative and weak affiliation to the account, which J accepts in (25). J then goes on, after a lapse, to close the sequence with a reassertion of the terms of the earlier assessment in (5), but with some significant modifications. The terms "shit" and "dog" are used in assessing the sections, and recall their prior usage in the phrase, "dog shit fixes" in (5). In (27, 29), however, it is no longer "fixes" that are being referenced, but "it" (presumably the photographic display of the sections). Additionally, the terms "just looks" and "looks" are used in (27, 29) and do not appear in the earlier negative assessment. As used here, these terms achieve a separation of the negatively assessed features of the photographs from their projected usability as data. This phrasing suggests that whatever the specific nature of the troubles with the photographs, the troubles are "just" a matter of the way the photographs "look"; they may be adequate for the purposes of their use in the project despite their "aesthetic"
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drawbacks. (Here “aesthetic” features are not available as invariant features of micro-photographs, but are locally constituted in the sequence . . . the “aesthetic” horizon of the photographs are distinguished as a way of doing interactional work.)

In (31) J goes on to conclude that he will assemble the micrograph into a montage, despite their assessable troubles, the conclusion reiterating what was proposed in (1), though there is no mention at this point that the adequacy of the micrographs needs later to be “figured out.” Accordingly, the inquiry into the sources of any artifact is dismissed from immediate relevance. The somewhat tentatively agreed-to speculation which J proposed in (19-22) now stands as closed to further argument, at least for the time being, in light of J’s injunction to proceed with the task at hand.

6 “Mixing mix-up”

Background: a research assistant from a neighboring laboratory (G) is currently receiving instructions from a lab research assistant (L) on how to prepare a particular, highly volatile, microstructural “label.” Since the solution is routinely employed in L’s lab, G has been sent over to learn how to use it for application in her lab’s researches. While demonstrating how to mix the solution, L attempts to “shake in” a small amount of a particular powder into the liquid solution and “too much” falls in. Subsequently, J, another lab member, comes on the scene and attempts to instruct L on the “correct” procedure for mixing the chemical. This leaves G, the original recipient of the instructions, somewhat confused, as her instructors negotiate over what the procedure is in the first place:

1 J: Jesus Christ! You made up a bunch didn’t yuh.
2 L: I know.
3 J: How concentrated?
4 L: I made it too concentrated. I was trying to shake just a little bit.
5 J: When you make that up-
6 L: I know.
7 J: Use uh, a silver spatula. Usa a-
9 L: Y’use spatulas in it?
10 J: Yeh. Yeh use five milligrams with ten milliliters.
11 L: Uoh- oh kay.
12 J: That’s more of the order of twenty milligrams and five milliliters.
13 L: No, that’s a thirty milligrams.

Analysis: in (1) J exclaims that L has “made up a bunch,” to which remark L asserts that he already knows that he erred in mixing the solution. J then asks L how concentrated he made the solution, and L remarks that he made it “too concentrated;” that “a big glob” of the powder “fell in” (4-5). J then goes on to begin an instruction in (6) to L on how to “make that up” and L cuts off the instructions with the assertion, “I know.”

In this sequence, from (1) through (7), both J and L acknowledge that a mistake has occurred in L’s preparation of the solution: L has mixed “too much” of one ingredient into the solution. However, J and L formulate the “mistake” differently. Where L asserts that he “knows” how to do the procedure, and attributes the mistake to an incidental accident, J formulates the mistake as an error in procedure. J (3) inquires about a matter of the “erroneous amount” of the ingredient in the solution, and L answers (4) with a non-numerical formulation of the “concentration” and an assertion to the effect that whatever the incorrect amount which was added on this occasion, it was an isolated incident. As an incident, the mistake does not implicate L’s
“knowledge” of the proper amount as it does his embodied attempt to measure out that amount on this occasion.

Despite L’s claim (7) that he knows the procedure, J persists with his “correction” (8), where J instructs L to use a “silver spatula” during the procedure (presumably to handle the corrosive ingredient while transferring it from its container to the solution). L cuts this instruction off (9) with an expression of surprise over J’s mention of the spatula. This expression locates the mention of the spatula as “news” for L, though L has not as yet accepted the instruction as a correction of his own procedure. He may as easily be heard to be commenting on the strangeness of J’s method for accomplishing the procedure.

In (10) J affirms the instruction on the spatula and continues with the correction/instruction by specifying a numerical ratio of milligrams per ten milliliters of solute. L receives this bit of instruction with a rather noncommittal “ohkay.”

J then refers to the “mistaken” concentration of solution which L has just prepared and gives a numerical estimate of weight to volume. L disputes the figure for milligrams (13). (I believe that L is asserting that there are “thirty milligrams” in the entire solution he has prepared, while J has given a ratio of “twenty milligrams” per “five milliliters” of solvent. Since there are many more than “five milliliters” in the solution L is preparing, J’s formulation is comparatively higher than L’s in terms of concentration.) In (14) J argues that L has “hundreds” of milligrams in the solution, and then retracts that estimate (15). L asserts agreement to the retraction, and J begins a reassertion of his account of the mistaken amount (17). In (18) L attempts to close the argument over the amount with a summary of J’s prior assertion, reformulating what J had provided as a numerical ratio with the expression “way too much.” Again, as in (4, 5) L gives a non-numerical formulation in the face of J’s provisions of a measured amount. The reformulation appears to de-emphasize the exact degree to which the measurement of the ingredient erred, and to define the mistake not as a mistake of measurement, but as an isolated error not worth making a fuss about. The visitor accepts the formulation, and L accepts her acceptance (20), as they exchange tokens.

The visitor then requests confirmation on her account of the concentration, as she formulates the concentration numerically (21). L’s response to the request (22) is a non-numerical formulation which employs the terms of a vernacular competence with amounts rather than the readings of a measurement. J follows this immediately with an account of a numerical measurement which corrects the visitor’s formulation (twenty milliliters is corrected to "ten something"). Having been given the two variant formulations, the visitor then inquires (24) as to the necessity for weighing the ingredient prior to adding it to the solution. J responds first and reasserts his formulation of the correct measure, “ten,” and affirms (with “yeah”) that weighing is involved. L replies that he does not weigh the ingredient, expressing his account in terms of his procedure rather than in terms of the impersonal format which J employs. L then invokes an impersonal authority (“it says that yer not supposed to”) in an explanation of his assertion (26).

In (27) the visitor provides a receipt of L’s account, and in (28) J begins an account which will presumably specify that he weighs the ingredient out. L cuts this off with a question to that effect, and J then launches into a procedural explanation on how to weigh the corrosive compound without putting it into contact with the metal surface of the scale. In (31) L explains that he had “heard” that the compound was not to be weighed on the balance (presumably because the compound reacts with the metal of the scale, confounding the measurement and damaging the instrument). In this utterance, L modifies his previous account (26) with “I’d heard” replacing “it says” in attributing authority to his method (the replacement phrase invoking a somewhat more circumstantially based authority than the indifferent, “it”).

In L’s account (31), the prior negotiations between J’s numerical formulation of the concentration and L’s non-numerical formulations are provided for as the outcome of different procedures, in which J uses a scale to weigh the material and L uses an embodied assessment of “how much.” Note that once the procedural discrepancy emerges, L accedes to J’s account, and the parties show a preference for the numerical measure. (L accounts for his procedure by addressing why a measurement was not used, while J provides for how a measurement could be done, both accounts showing a preference in this case to measure when possible.)

7 Electron Microscopy Lab, transcript A

Background: in this sequence, H, the lab director, is reviewing some photographic montages which two research assistants, J and B, have prepared. The montages are strips of electron micrographs (about 22,000 X magnification) arranged end-to-end to show a continuous “vertical” aerial view, arranged roughly parallel to the “growth” of hippocampal granule cell dendrites. The montages are analyzed as records of the distribution of intact and degenerating axon terminals resulting from an experimentally produced lesion on a “distant” region of the cerebral cortex. A key matter in the analysis of the documents is the stratification of the
"fields" of terminals into relatively discrete layers of "intact" and "degenerating" terminals. The micrographs were shot so as to display the "degenerating" layer of terminals in the upper sections of the vertical strip of photographs and to display the "intact" terminals in the lower sections. In between the two regions a variable zone of overlap documented the in-course replacement of "degenerating" terminals by "intacts" during the process of axon sprouting. Several issues involving the "reading" and assessment of the montages arise in the following sequence (see Figures 2.1-2.5):

1. H: Yeah, (nice isn't 't) thes' fuckers 'r rilly small.
2. (right)
3. H: Yeh know that
4. J: Eyeh
5. H: These terminals?
6. J: Eyeh
7. H: The mo:re-
8. J: [W'l I think they are anyway.
9. (1.0)
10. H: (W'll ther'a-)
11. J: Small err
12. (0.8)
13. H: D'yew think these things are abnormally large-
14. (0.5)
15. H: Err or not=
16. H: =I see what you're saying, yeh
17. J: I think they're ss- larger down here than they are up here anyway.
18. (0.8)
19. H: °Uhh huh
20. J: See this this- this's a low end of a beginner-
21. (1.0)
22. J: I hope you know what (I mean here)
23. H: (I'm doin' here) .
24. J: Whaddy- (ah mm-)
25. H: Nno::w I see
26. J: An' it goes to the end
27. (0.6)
28. J: Begin=
29. H: Eh hho:=
30. J: =It goes to the end=
31. H: =You foo' me::e
32. (0.8)
33. H: 'Ayy Jerry, you foo'd mee
34. J: W'll 'ts still, its still uh (-) you know
35. J: What the hell
36. H: W'll its nice
37. (0.5)
38. J: Look't th' g- look eh' I mean look et where the degeneration starts=
39. J: =Fer one thing you have you have degeneration here, here, here, a big astraglia-
40. (H): ((rapping noise on desk, 14 taps, duration 4.0))
41. H: BANG GLA DESH!
42. (0.6)
43. H: Is that degen- D.G. there?
44. J: °Eyehhnn
45. H: Much dreaded D.G.?
46. J: Yeah, I don't know if thet's the starting point]
47. (H): [* " * * " ((finger snaps))]* * *
48. (0.7)
49. H: Yeah 'ss kinda unfortunate
50. J: Yeah
51. (0.4)
52. H: Shot this a little high, didn' yeh
53. J: Nnua?
54. (0.3)
55. H: Noh-
56. (0.8)
57. B: °Uhhn
58. (0.2)
59. J: //No,] but I mean it- it's really densely- I don't know if it's=
60. H: (Hhokay)]
61. J: =Sprouting err' normal population right in here
62. (0.4)
63. J: Where it's starting to go out really well it's still (varied)
64. (0.2)
65. J: I haven't mar- finished markeng this one
66. (1.5)
67. H: What's a po:or boy to do:
68. (0.4)
69. J: Ahhn I don't even know if (you'd) want to look at those,
70. J: *ther' so ba::d=
71. H: =They are bad (-) well they're numbers aren't they
72. (3.0)
73. J: They're users but they ain't lookers
AGREEMENT IN LABORATORY SHOP TALK

74 (3.5) H: ((walking out)) Uh::hn sheeit!

(a) Analysis of lines 1–37
In this sequence J expresses disagreement with H over H's characterization of a given field of terminals as "small." The disagreement is delayed in its expression and is resolved as a "misunderstanding" of the way a particular column of micrographs was arranged in the montage format.

In line (1) H remarks pronominally that terminals in a particular area of the display are "really small." This receives no immediate confirmation from J or B and occasions H's request for a response in (3). J gives a very tentative token of agreement under this prodding in (4) and H displays a sensitivity to this lack of full agreement by modifying his prior assertion in a question format and specifying the referent as "these terminals." In using a question format the modification explicitly marks its claim as a matter for agreement, a claim which seeks support from its recipients rather than being directly asserted as a "fact" (as in (1)). In (6) J again replies tentatively, though H's subsequent utterance appears to project an assertion which takes J's token of agreement as a warrant for proceeding further with the account. At this point J cuts the assertion off and comes forward with a disagreement, a disagreement which appears to have been withheld or submerged in and through the prior weak agreements.

In his counter-assertion J uses the preface, "I think," in marking the account as a contending version relative to H's prior claim. What J asserts is that the terminals which H has called "small" are generally small in that particular stratum of the hippocampus molecular layer. H's assertion has implied that the terminals are small because they are newly formed "replacements" for axon terminals which degenerated due to the lesion. While H is treating "small" size as a document of the terminals' origins in sprouting, J is now claiming that the small size is not unusual for "mature" axons in the particular region being examined. According to J's account, both the replacement axons and those they replaced would very likely be "small."

In (11) J expands upon his prior account by adding the term "small err." By appending the term, "small" with the emphatic, "err," J appropriates the term "small" from H's prior reference (1), while modifying it in a way that disputes H's account of what "small" documents. J's usage here asserts a "qualified" agreement with H's reference, preserving the relevance of "small" as a characterization of the "terminals" in question, while disaffiliating from what H may be "making of" that characterization. H

"checks" an "understanding" of J's modification by referring to an adjacent field of terminals in the photograph and asking if J "thinks" that the terminals in the adjacent field are "large err" or not. Note the way H picks up the peculiarly appended character of J's earlier utterance of "small err."

In reply to H's understanding check, J reasserts his account in reference to the field of "larger" terminals which H pointed to in (13, 15). H responds to this account with a noncommittal token of understanding ("uhh huh"), and then J goes into an explanation which attempts to remedy the discrepancy between his and H's accounts. In this explanation (20), J refers to a feature of the micrographic montage format ("this's a low end of a beginner") as a way of relocating the "terminal fields" which were characterized in the prior accounts of this sequence. The explanation thereby brings about a shift in parties' orientation to the "terminals" as items local to the immediate presence of the photographic layout, rather than as objects relative to the distributions of axon terminals in the brain.

The terminal fields in question are located in (20) in reference to the vertical alignment of the particular montage of photographs. The photographs were placed end to end in two separate columns, where the "beginning" of the column was located with a cell layer cross-section at the "lower" end of one of the two columns. The displayed scene proceeded up the column and on through the adjacent column in a construction of a continuous view of a cross-sectional surface of granule cell dendrites and two layers of axons connecting to those dendrites (see Figures 2.3–2.5). J's account provides for H's assertion as misplaced with respect to the columns of photographs, and J (20–30) leads H through the formatting of the columns into a continuous scene starting from the "beginning" and going through to the "end." Apparently, J is showing H that H's account of terminals is a result of a mistake about where the "beginning" of the series of photographs was located in the two adjacent columns. H follows J's instructions by repeatedly exclaiming (31, 33) that J "foo'd mee." (I believe that what H is asserting here is that J constructed the two columns differently than usual by placing the "beginning" column on the left-hand side rather than on the right-hand side of the other column in the series.) The trouble is thereby remedied interactively as an error mediated by the unusual formatting of the photographs in this case. The character of the "terminals" is accordingly no longer in question as it was when the disagreement emerged. Note that a "disagreement" in this case is reformulated by the parties involved in this sequence as a form of "misunderstanding" and not as a matter of contrary ultrastructural accounts. As such
it is remedied without either party retracting a substantive version of ultrastructural events. (H retracts his initial assertion (1), but not as a reference to anatomical possibilities; rather the account is dismissed from relevance to the particular documents initially used to support it.)

(b) Analysis of lines 43-66
In this sequence H and J agree on a characterization of a feature of the montage format as "unfortunate." H then accuses J of having erred in the way he photographed the electron microscopic section, and J denies this accusation. J provides an account in support of his denial which locates the trouble with the ambiguous rendering of ultrastructural constituents given in the particular photographs. The account implicates error in a less definite fashion than does H's accusation.

In (43) H phrases a noticing in a question format, perhaps as a way of seeking confirmation from J in the projection of a topic for further discussion. In the utterance H begins the term "degeneration" and cuts this off and replaces that term with a locally used abbreviation, "D.G." J confirms the noticing rather weakly, and H re-asks his question in (45). The question is modified with the insertion of the phrase, "much dreaded," as a play on a sort of casual "horror movie" way of delivering the noticing. J does not pick up on the playful format of the question, and replies affirmatively but with a qualification (46). J's reply confirms that the particular item to which H referred is an instance of a degenerating terminal. However, J asserts further that he is uncertain as to where the adjacent fields of intact and degenerating terminals grade into one another in the photographic layout (the "starting point" being the place in the column of photographs where a relatively uniform field of intact terminals breaks into a relatively uniform field of degenerating terminals and axons). This reply answers not only the specific question H asked, but comments upon an unexpressed implication retrospectively attributed to the question.

In (47, 48) H "marks time" with finger snaps, presumably while examining the micrographs in light of J's prior account. In (49) H agrees with that account and assesses the matter as "kinda unfortunate." J asserts agreement with the assessment, and after a slight pause H assigns blame for the trouble to J's action of shooting the photographic series. The blame-account asserts that the indiscriminate breaking point between intact and degenerating terminal fields resulted from J's having mislocated the column of photographs in the areal plane of the electron microscopically rendered section. The column is shot so as to cut across two adjacent lamina of axon inputs into the granule cell dendrites. The lower field with respect to the column is a stratum of "intact" commissural axons, while the upper field is a stratum of "degenerating" axons and axon terminals which show the effect of a lesion in the entorhinal cortex of the brain. If, as H claims, J shot the column "too high" the border between the two axon layers would not appear within the field of the photographs, thus accounting for the difficulty in finding a clear breaking point.

Note that references such as "high," "starting point," "above," and "lower end" are locational terms which identify the accessibility for looking and telling of the ultrastructural arrangement. These usages are conditioned in the formal accessibility of the visible arrangement of cells, dendrites and axons through the microscopic preparatory work. The hippocampus was selected and rendered as a "structure" which showed relatively discrete and parallel layers of axons coursing perpendicularly to the dendritic "trees" of a layer of granule cells. The pre-selection of a cellular arrangement which was relatively amenable to description and display as a "geometric" order of axon layers, dendritic "trees," and cell body layers provided for the possibility of linear measurements, as well as the use of such locational terms as "up" and "down," and "above" and "below" in rendering "structures" accountable. These usages were further set up by the artful work of shooting a linear column of photographs such that the relatively planar surfaces and areal construction of the photographic documents could be subjected to plane-geometric accounting practices. The orientation of the photographs within fields of rectilinearly interpreted ultrastructural alignments was used to support a representational tie between measurements and other constructive activities using the documents and the interior anatomy of the hippocampus. Operations such as identifying and counting images of axon terminals on the surface of the photographs and providing for the tallies in terms of ratios of intact and degenerating terminals per square-micron sector of the brain cross-section were made possible through the formatting of the photographic column to incorporate a scale of magnificationally transformed "microns" along the edge of the column.

In (53) J denies the accusation, and in (59) he gives an alternative account of the trouble. This later account provides for the indistinctness of the "starting point" as a sort of material indistinctness. In (59) and (61) J refers to the "it" of the displayed fields of terminals and the "here" of a locale within the field of the photographs where an ambiguity is visible. In (61) J asserts that he is uncertain as to whether the terminals in a given locale are distinguishable as "normal" terminals or "sprouted" terminals.
replacing degenerating structures. This account provides for the locale of the presumable “starting point” as available within the field of the photographs, but as obscured and not analytically discrete. H does not comment on this account and J goes on further to say that he had not finished “marking this one.” “Marking” is the work of tracing on a sheer plastic sheet the outlines of “degenerating” and “intact” terminals as they are analytically detected in the montage of photographs.

J’s mention of the unfinished character of the work appends his mention of the material difficulty of the photographs with an account of how he has not yet fully analyzed the materials.

J’s account gives an explanation of how the “recalcitrance” of the photographic material to a particular analytic use was not attributable to an error in photography. This does not dismiss other possibilities for attributing error to J’s preparation of the materials. Indeed, later in the sequence (69, 70) H and J agree to an assessment of the photographs as “bad.” What possible errors might have been found responsible for the poor quality of the materials is not explicited here, and J is not further accused of any specific avoidable error. Instead, the materials are referenced in denigrating expressions and no mention is made of their agency. It is notable that in other contexts lab members mentioned that some troubles were not seen as fully controllable, and a kind of blameless resignation accompanied their noticing. J’s account in (59-63) dismisses an attribution of blame while providing for the trouble in a way which does not clearly implicate his agency and competence. In (65) he renders the inquiry into sources of the trouble more ambiguous by characterizing the analysis of the materials as unfinished.

(c) Analysis of lines 67-75
In this sequence J first assesses the photographs as so “bad” as to possibly not merit being “looked at.” H agrees, but then asserts that the materials are “numbers” (usable as “N’s” in a project), and J modifies his prior account in a way that incorporates H’s assertion.

In line (67) H uses a stock phrase which expresses resignation to a negatively assessed situation. J picks up on this assessment and characterizes the photographs currently being reviewed as so “bad” as to possibly not merit “looking at.” This utterance is phrased in such a way as to leave it indefinite as to how consequential the “bad” quality of the materials will be. In the next turn H asserts that they are “numbers.” (“Numbers” here refers to the usability of the materials as “data” — material from which to add “numbers” to the corpus of graphic displays of terminal distributions.) J then asserts, in agreement with H, that “they are numbers,” and after a pause modifies this assertion with the clause, “but they ain’t lookers.” After another pause, H provides a fitting commentary on the situation as he leaves the scene.

J’s account in (73) specifies how the materials are usable as data, though not much to “look at”, the account providing a way of reconciling J’s original assessment that the photographs were “bad” with the later agreement that they were “numbers” by asserting for the “looks” and the “use” of the materials a separable accountability.

Note that the distinction invoked here between “looks” and “use,” though it accomplishes interactive work in the local scene, was used on other occasions. However, the distinction is brought to bear on the matter immediately at hand in the interaction . . . it is not merely the invocation of a (to lab members) familiar distinction, but does particular work relative to the immediate circumstances in the shop talk.

8 Electron Microscopy Lab, transcript B
Background: the following excerpt was taken from a transcript of a conversation which occurred as the three participants in the Ultrastructure Project (H, J and B) reviewed a recently completed collection of “2½-day” montages. During this conversation the three parties assess the montages in terms of their adequacy as data for the purposes of a temporal display of axon sprouting. While documenting their remarks with the materials at hand the parties negotiate over proposals: on the adequacy of specific montages for demonstrating a particular temporal condition within the hypothetical process of axon sprouting, on how many “n’s” at that “time point” and other “time points” will be sufficient for the purposes of the project, and on what other “time points” should be documented with montages given what available montages show under analysis.

This conversation gives evidence for some of the ways in which the design of the project was achieved as an in-course accomplishment of the interacting parties. In some respects the project was not organized as a matter of advance planning and subsequent enactment. Rather, at various times prior to the completion of a written report of the ultrastructure project parties reviewed the results in hand and projected further phases of work on the basis of what could be collectively decided. The local work of agreement was prominently involved in shaping the subsequent course of the project.

During negotiations over proposals the parties involved invoke
different orders of “authority” in support of their claims. Where H, the nominal head of the lab’s research, invokes his access to an extensive range of projects performed under his direction, J displays his competence with the ultrastructural details of the micrographs prepared in his work. H, unlike some lab directors, commonly involved himself in the actual performance of experiments and analysis of microscopic slides. However, his acquaintance with the particular materials prepared by the research assistants in the ultrastructure project was relatively limited. This matter is exploited in arguments by J which are laid out in terms of “at hand” details in a photographic display.

In this conversation the topic of “days” is central to the work of assessing the results-in-hand and planning on the basis of those assessments. “Days” are provided for as an indexical unit in the display of the temporal structure of axon sprouting. Each “animal” and the constructive-analytic materials that were provided through the rendering of that “animal” were indexed in terms of a “day” interval. The “day” interval was an approximation of the number of calendar days which passed between the laboratory activities of lesioning and sacrificing the animal. If, a laboratory rat was subjected to a brain lesion on a Monday morning and then was sacrificed on a Wednesday evening, it was designated as a “2V2-day animal.” A variable number of “days” were produced for different animals in constructing an orderly basis of comparison for assessing the temporal organization of axon sprouting.

Each “day” was significant as an index of an amount of time which passed subsequent to the injury to the animal brain. If, for instance, the animal was kept alive for three days after the lesion, the sacrifice of the animal and the removal of its brain for microscopic analysis provided researchers with a document of the “recovery process” of the brain up to that point. With the construction of a series of such “day points” from different animals it was hoped that a serial reconstruction of the “recovery process” would be made sensible as regular changes in visible microstructural arrangements between the various “days” in the series. The montages were constructed in such a way as graphically to portray spatial distributions of intact and degenerating axon terminals which could be compared across the series of “day” integers.

The matter of which time points were to count as members of the series was not settled by the prior designation of a regular series of temporal units (such as, for example, 1, 2, 3, 4 . . . n days) to be used in the production of montage displays. Instead, selection of such time intervals showed a “preference” for integral

“day” units with each particular “day” being chosen with an orientation toward possible events that might be revealed in the comparison of that “day” with other, already documented “days.” The eventual series of days which was selected through the production of montages showed a rough progression of integers from “2 days” through “11 days,” (2, 2V2, 3, 4, 5, 5V2, 6, 7, 8, 9, 11). The actual order of production in the lab work of the numbers in this series was not done as “2 days” first, then “2V2 days” . . . and on through “11 days.” Instead, “days” were chosen circumstantially when parties in the ultrastructure project made a case for “looking” at a particular “day” point. One residue of such “looking” for evidences of “axon sprouting” was that the temporal interval at “around 5 days” was documented with a relatively intensive production of montages within the interval of “4–6 days.” Similarly, the interval from “2–3 days” was provided with relatively intensive documentation. Issues in the documentation of that temporal vicinity are subject to parties’ negotiations in the conversation transcribed below.

The matter in question for parties in that conversation is the acceptability of “2V2 days” as an adequate document of a “baseline” for the demonstration of axon sprouting. What is at issue is the use of that temporal interval post the lesion as a “before” condition in the demonstration. After the lesion was performed on an animal’s entorhinal cortex, and within two or three days afterward when the animal was killed and the brain tissue electron microscopically examined in the hippocampal region, widespread evidences of axon degeneration were detectable. The degeneration was detectable as a darkening (in terms of a particular stain assay, selected specifically for labelling degeneration) and degradation of microscopic configurations identified with axons and axon terminals. This degeneration was taken as a document of the extension of the destruction of the entorhinal cortex cells to encompass the axons arising from cell bodies in the entorhinal cortex and synapsing in the hippocampus. These effects of the lesion in a relatively distant brain locale did not become visible until a lapse of two or three days after the lesion enabled the degeneration of axons to proceed sufficiently to be visible. Accordingly, the electron microscopic display of the extent of the degeneration in the hippocampus required a certain latency period after the lesion to allow degeneration to spread from cell bodies to the distal terminals. This display was used to show the microscopic distribution of degenerating terminals and axons in the hippocampus prior to any “sprouting” of axons from the adjacent lamina to replace the destroyed synapses.

A problem involved in selecting an adequate “baseline” is at
issue in the work of the parties in the sequence below. In the sequence, H expresses a concern over “false positives” which at “two-and-a-half days” would be terminals presumably affected by the lesion, but which had yet to show up as dark under a degeneration assay. On the other hand, selection of a later day interval was avoided, and an earliest possible day was sought since any significant rearrangement of ultrastructural constituents within the early stages of the recovery process would be missed if a “baseline” was chosen after their onset. This problem is solved in the talk among the parties in the conversation as that talk elaborates itself in accounts of the scenic particulars of the electron micrographs at hand. The solution takes the form of agreements over such relative matters as how many “false positives” were tolerably part of an adequate demonstration, and how many “animals” were necessary to establish the adequacy of that time point as a generalizable construction.

1 H: How m'ny w'll this give us at two en a half
2 (0.8)
3 J: Wll, I've got five montages th'oo that o:ne, but we've got uhh
4 (1.5)
5 J: Eh three other good ah- hn should be good animals et this time point that we haven't cut up yet.
6 H: E(h)mm(o'key)
7 J: Enn those ones we jest did thee other day=
8 J: =This sis the worst of that batch
9 J: =Thissis the worst of that batch
10 H: Wha- What's further on beyond that noaw. Number one.
11 (0.5)
12 H: Like four days
13 J: Hmm mm
14 (0.5)
15 J: Hhmmnothing
16 (0.5)
17 J: Thee uhhmm
18 (0.5)
19 J: Yeah, wer goin-n do some uhh
20 (1.0)
21 J: Wer goin' teh do some lesions one of these days (an' wer goin' end up-)
22 (1.0)
23 J: (Ree- you whuh we)
24 B: If we do it like on Thursday
250

25 J: (0.5)
26 J: Yeah, we can do that.=
27 B: (T'morrow)
28 J: =I wuz(thinking we should) git-
29 B: Then we'll wind up with-
30 J: Some of this-
31 (0.6)
32 J: Do et least a couple of these animals, first
33 H: Right right
34 H: Right, "yah
35 J: An' then we ken maybe hit all the five-day points.
36 J: 'Y'know do a
37 (2.0)
38 J: Three or four or five an' a half days
39 (0.6)
40 J: An'n just do those.
41 (0.8)
42 B: Ehlyeah
43 H: I'm sure, I'm gonna stare at a thousand of those light microscopic stains,
44 H: 'Ts ha:ppening 'n th'-
45 (1.5)
46 H: (Nnearh) enn the fifth day
47 (1.5)
48 J: 'Hh well-
49 (0.8)
50 H: (N'ye)h rilly (0.2) whuh wuh
51 (0.5)
52 H: (Here) our problem. Letss g- thr right.
53 H: (B'fwee uh) you should cut those (othehs) up, let's go through this one
54 (0.8)
55 H: B'cause
56 (0.8)
57 H: Ther is a pt-pt-potential problem here right thet
58 (0.8)
59 H: Uhh
60 (1.5)
61 H: Thee inacts will not have per- the deegenerating ones will not have perceeded far enough=
62 J: Yeahh, (ther) is a possibility, I- I think m-m-umm a (thet)
   good majority of them (0.2) are there
63 H: I'll (have) already=
   (all)
AGREEMENT IN LABORATORY SHOP TALK

64 J: =Yeh have gone=
65 J: =Gone to the dark stage ?=
66 J: =Yeh
67 (1.0)
68 J: Because, if yeh look
69 (0.9)
70 J: eh know if yeh look et this stuff it- ss things tht are degenerating
71 (1.0)
72 J: Ar- are very definite
73 (1.6)
74 J: En thers no real question about it.
75 (0.8)
76 B: Th'ts th' thing tht rilly blew me out once I wuz looking
77 (J): (Yeh, yeh we::ll hh)
78 B: =Three day (stuff) ('n the) terminals were alreddy uhh
79 (1.5)
80 B: Phag'itized by thee uh, by the glia.
81 H: (Ehn, snn ce)
82 J: (=Oh yeah ther are some like that naow
83 (3.0)
84 H: Yeah I'm not worried 'bout th' ehh-, it the false positives tht worry me=
85 J: =Yea:h, yeh
86 H: Like this
87 (0.5)
88 J: ↑ "Hh Oh yeah w'll that one-
89 (0.5)
90 J: I didn' mark I don't think
91 J: Yeh know I j'st put a little X there, because that's marginal,
92 J: 't this one looks like it has a density
93 (0.8)
94 J: R right there
95 H: ↑ Ye:en this one looks
96 (0.4)
97 H: Pretty good
98 J: Yeahh
99 (0.8)
100 J: ↑ So there are- there's a 1- there're-
101 (0.4)
102 J: Two or three right in this region.
103 (0.6)
104 J: But

OBJECTS AND OBJECTIONS

105 (0.4)
106 J: Beyond there
107 (0.4)
108 J: There's o: ↓ ne
109 (0.5)
110 J: En a marginal
111 (0.5)
112 H: Right
113 (1.0)
114 J: An' the beyond here I couldn' rilly see any tht rilly
115 (3.0)
116 H: Nnrih:nt
117 (0.8)
118 J: (Turn teh) one right here tht,
119 (0.5)
120 J: ^Eth I probly shouldda marked an' I just looked at it.
121 (0.8)
122 H: Ye:ah yer right though, ther'r
123 (1.8)
124 H: W very few chances here of seeing anything
125 (2.2)
126 H: Y'ed ever want teh call an intact
127 J: Yeah
128 (1.0)
129 J: Will this one (↓) breaks right in:
130 A: ((in background)) Bill?
131 J: This is kinda the breaking point see theh deegeneration
132 H: ↑ Right
133 (0.8)
134 J: En' then there's a couple of (↓) intactst here
135 J: So thch (↑) hh
136 (0.4)
137 H: Enn hnn
138 J: This is theh
139 (H): ^Yeah you're right
140 H: That's good, this'll be good
141 J: Nye::eah ↑
142 (0.8)
143 H: Gi' us our baseline
144 (2.0)
145 H: The baseline
146 H: Theh cleaner the baseline the less we need (↓)
147 J: Hlyeah
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148 H: Further ehl-long obviously
149 (B): Yeah
150 J: W'll this- thisis-
151 (0.5)
152 J: M I mean- eh yeh know its not-
153 J: It's not (-) ss- sterile but its fairly clean=
154 H: Yeah, yeh
155 (4.5)
156 ( ): Hhhhh
157 J: (Now w- yeh) we'll have three more of these
158 (0.4)
159 J: By- in a couple a hours
160 H: Shoot!
161 (1.0)
162 J: (So)
163 H: (Scaurf)
164 (0.5).
165 J: (Sorf)
166 (1.2)
167 J: So we c'n-
168 J: Bill, you're cutting one of those no:w, right?
169 B: Eyeh, number five
170 J: 'Ehh hh
171 (1.0)
172 J: So, w' want another animal
173 (1.0)
174 J: 'N eh couple of days↓
175 (0.8)
176 H: Hh'kay, may'ee we should just analyze th' bejesus oudov those (ones) an compare what we get here, teh what we got over here at six
177 J: I-I think es- es a
178 ( ): (I think it wou- be) a good idea
179 H: 'Cause we may not want to keep on doing these animals if things
180 (1.0)
181 H: Ye'know, the stuffs not black and white th'hh
182 H: I mea- i' y'know' hh rilly comes down to how many numbers we need=
183 J: =Yeah
184 H: At this point

(a) Analysis of lines 1–52
In this phase of the conversation H requests "inventories" from J and B on "how many" montages have been completed at specified

"time points." While replying to H's requests, J and B negotiate over which "time points" will be investigated next in their research. In response to a proposal by J to do more "2½-day" montages before proceeding with further constructions in the "day" series, H specifies a particular temporal locale for "axon sprouting." J responds to this assertion with a "disagreement preface," and H then shifts the topic back to the "2½-day" montages, thereby postponing further discussion of the (possibly) disputed prior assertion.

H, J, and B have arranged themselves around a particular "2½-day" montage which has been laid out on a drawing board. In (1) H asks how many "2½-day" montages have been assembled, including this most recent addition to the collection. J answers in (3) and continues in (5) after a lapse, adding that three other "good animals" have yet to be "cut up." "Animals" here refers to fragments of brain tissue embedded in plastic and stored in the lab. (The original animals are long since dead.) The fragments from three separate brains are available in this rendered condition, and have not yet been processed into electron-microscopic sections. Innumerable sections and photographic montages can be produced from each embedded tissue fragment. In (5) J modifies the term "good" to "should be good" with an in-course reassessment. The assessment of the quality of the brain materials is initially made in the utterance in reference to the incomplete renderings of the "animals," and then modified with a qualification about the projected adequacy of the "animals" once they become visible as electron micrographs. The modification asserts that the assessment will be reserved until the materials achieve their fully determinate character.

In (8) J assesses the currently visible montage as the "worst" of a cohort of "animals" which were processed together. This assessment implicates the others in the cohort as adequate for the purposes of the project if "this" one is assessed as such.

In (9) H asks, "what's further on beyond that" and receives no immediate reply. He then specifies the prior remark with the phrase, "like four days." The specification displays a hearing of the prior remark which places it in the context of a temporal trajectory of "days" in the schematic demonstration of axon sprouting. "Beyond that" may also be heard as a reference within a temporal order of lab activities, such that the question would be asking J and B about what they will be doing "next." In selecting a formulation from the "day" series the specification employs a particular number to invoke a temporal context for hearing the number as an "instance."

In his reply (12–14) J answers, "nothing," and then quickly
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marks the "old four days" ("4-day" montages which were produced in a somewhat different format than those presently used) as a "dispreferred" response, but a response which allows H the option of accepting it. B then projects a plan to "do some lesions one of these days." In doing so B displays a hearing of H's question as an injunction to produce an-answer-as-a-project. The proposal is generated out of an absence of a "preferred" reply to H's question.

J interrupts B's proposal in (23, 26, 28) with an ostensible show of agreement to the proposal, followed by a counter-proposal. In (26) J asserts agreement with the proposal with "yeah," but marks the agreement as qualified with the phrase, "we can do that" - where a conditional form is used in equivocating the receipt of the proposal. In (28) J then initiates a counter-proposal while B overlaps with a continuation of his earlier proposal. J's utterance persists through the overlap in (30) and in (32) the proposal is continued. J's proposal is heavily qualified in the course of its announcement. In (28) the proposal is prefaced with the phrase, "I wuz thinking we should," which marks its occurrence as a contending possibility rather than an uncontested plan (note that B's prior proposal was asserted in the more "definite" format, "we're goin' to"). In (32) J proposes that "we" continue doing "these" ("21/2-day") "animals" prior to starting any others in the project. H repeatedly asserts agreement in (33-4) and J then makes the further proposal to "hit all the five-day points." (This proposal, like J's earlier one, is qualified as a conditional possibility - conditional upon other parties' agreement.) In (38) J specifies the "five-day points" by mentioning three "day" integers which are thereby given candidacy as yet-to-be produced documents of axon sprouting. ("Five-day points" refers to the production of temporal intervals in the vicinity of five days. Parties in the project had previously established for their own satisfaction that a rapidly occurring growth of axons into the degenerating region occurred "around five days" after the lesion.) In (40) J concludes his proposals with a continuation to the effect that the "five-day points" that he mentions may be all that is further necessary to be "done" to document axon sprouting for the ultrastructure project.

B gives a very minimal token of agreement in (48) and then H provides an account supporting the relevance of investigating the "five-day points." In this account (43) H invokes his experience with another (light microscopic) project which also involved a temporalization of axon sprouting with a "day" format. H asserts very definitely that "it's" (the rapid growth of axons) "happening in the fifth day." Note his use of the progressive tense in formulating "it" as a singular event at the scene of the talking. Axon sprouting is thereby made accountable as the unfolding of a "story" . . . as "it is happening; not "it happens in every instance," or "it's happened every time we've checked it."

In replying to H's assertion, J produces the delayed and equivocal preface, "well," which expresses less than full agreement with the prior utterance. What might be at issue here is as yet only prefaced as a "dispreferred" response to the assertion. A possible issue for disagreement may be H's formulation, "on the fifth day," which restricts the occurrence of the possible event somewhat more than J's prior specification of the "five-day points" as "three or four or five an' a half days"). Before any further substantiation of the possible disagreement is expressed, H begins an utterance which was unintelligible on the tape, and follows this with a formulation of a new proposal in (52). In this proposal H does work to insert an assertion of agreement to J's prior proposal, and then ostensibly reasserts that proposal. This reassertion postpones the issue of the "five-day points," thereby removing from immediate relevance any dispute over H's prior assertion on that topic. This is not to say that the disagreement will not come up in a later conversation.

(b) Analysis of lines 52-184

During this section of the conversation H proposes to undertake an analysis of the "21/2-day" montage to assess whether or not that "time point" is adequate as a "baseline" in the temporal series of montages. J assures H that it is adequate by referring to the montage at hand and claiming that the terminals are already degenerated by that "time point." H then mentions that his worry is about the possibility that some terminals which appear "intact" may actually be degenerating terminals which have not yet shown their condition ("false positives"). In mentioning this worry H cites cases from the visibly available montage to document his claim. J argues on a case-by-case basis that the particulars which H cites are "marginal" or exceptions to an overall pattern. H then agrees with J on the point that the possible "false positives" are "few" in number, and that the "21/2-day" montages will be adequate as the "baseline." H then proposes to hold off production of further montages until some further analysis is completed.

H's proposal in (52) affiliates to J's prior proposal (32) to "do" the "21/2-day" animals prior to proceeding with the production of further "days." In (53) H elaborates the proposal in a somewhat discrepant way from J's earlier proposal, however. Here H proposes that the parties "go through" the particular montage at
hand ("this one"), while J had earlier proposed to render "a couple of these animals first" at the "2V2-day" point. In (57–61) H gives an account in support of his proposal, saying that there is a "potential problem" with the use of the "2V2-day" animals. This makes his proposal quite a different matter from J's "doing a couple of these animals first" at that point, as he is suggesting that parties make a careful assessment of the adequacy of the "2V2-day" animals. J had presupposed that adequacy in proposing to make more of the montages at that "day point." The display of agreement in H's proposal (52) thereby affiliated to one feature of J's earlier proposal (focus on 2V2 days first) in retrospectively affiliating to it. Once that relevance had been set up, H managed to assert quite a different plan.

At this point in the sequence it is somewhat unspecified as to what "go through" in (53) suggests for the parties to do. It can be heard as an injunction to produce further montages and analyze those carefully, or it can be more immediately relevant as raising a topic for parties to address by analyzing the montage at hand in the setting of talk. As it later turns out, parties settle the matter with their subsequent talk, and the "2V2-day" montages are agreed to be an adequate "baseline," without further proposals being called for to reserve the assessment until more "materials come in."

The "potential problem" which H mentions in (57) is specified in (61) where H asserts that the degenerating terminals will not have degraded "far enough" to be detected as degenerating, and not intact, terminals. J then gives a qualified account in disagreement with H's account. In (62) J initially asserts agreement and goes on to modify that agreement by marking the prior account as a possibility (such that his agreement now stands as an assertion about the possibility of the prior account and not a commitment what the account says), and then asserts an alternative account qualified by an "I think" preface. The account is further qualified through J's use of "a good majority" in reference to how many of the possible degenerating terminals he asserts are visible at 2V2-days. This indefinite quantifier allows considerable room for negotiation in the event that "how many" becomes a matter specifically at issue. In (63) H shows his understanding of J's account by collaborating in the extension of the utterance in a way that further specifies its reference. J then collaborates with H's affiliative production of the sentence begun by J by extending it still further in (64), while also displaying agreement with the understanding H expressed in the prior extension. This dance continues for another round as H repeats the last word of J's last extension and extends it to completion, ending with a question intonation as if to say, "is that right?" In (66) J accepts H's collaboration in the specification of the account. In this account, degeneration has been characterized with the term, "dark stage" in reference to the appearance of degeneration as "dark" in the micrographic rendering (a somewhat optional appearance since the photographs can be developed to show "electron density" as either "dark" or "light").

Having just been given a strong display of understanding (if not agreement) by H, J continues with his account in (70–4). Here he refers his claim to the montage at hand, prefacing his account with the phrase, "if yeh look." Unlike J's assertion in (62) this further assertion (it is not done as a "reassertion") is stated in a very definite format (indeed, using the phrase, "very definite" in characterizing the visibility of degenerating terminals in the photograph). Here J is claiming that in the "2V2-day" montage degenerating axons and axon terminals are very prominently visible, which implies that degeneration has fully set in by that time point. The upgrading of this assertion from the prior assertion by J appears to be organized in reference to the strong show of understanding by H in the intervening collaborative sequence. This modification acts in the "reverse direction" from those which were previously characterized here as accounts intervened by a challenge or display of disagreement.

In (76–80) B affiliates to J's prior account by recounting an incident in support of the utility of the "2V2-day" montages as a "baseline point." Here, B mentions seeing incidences of "phagocytosis" (the removal of degenerating material through ingestion by glia cells) in "3-day" montages. "Three days" was the serially next time point which was investigated in the study. If the "2V2-day" materials were assessed as too early for a clear documentation of the degenerating condition, "three days" would very likely be the next available candidate. What B is asserting is that at "three days" the process of removing degeneration is well underway, and that use of the "time point" as a "before" condition would be confounded by the regenerative process underway at that point. J in (82) adds further that what B said for the "3-day" montages can be said for the "2V2-day" montages as well, thereby adding further testimony to the account that degeneration is well underway at "2V2-days" and that it is by no means too early a "baseline." (Note J's use of "naow" in (82) establishes a contemporaneity of the "moment" in the conversation with the "time point" in the temporal display of axon sprouting which is currently topical. In this and other instances of shop talk, the talk is produced as though its temporality were "interior" to the "objective event" being analyzed in-and-through that talk.)
Following a lapse (perhaps occupied by displays of “studying” the montage), H displays agreement to the prior demonstrations by J and B, but then goes on to reassert the prior “potential problem” as a “worry” in (84). The “worry” is reformulated in such a way as to take account of the demonstration of the advanced visibility of degeneration in the montage, while still insisting that degeneration may not have fully developed at that “time point.” Here H invokes the possibility of “false positives,” axon terminals which appear intact but which have yet to show their “real” degeneration. This possibility of a negative artifact is used to argue that some of the terminals, which appear intact in the montage, actually are associated with the axons from the destroyed entorhinal cortex cells, but have not yet degenerated to such an extent as to show that condition in the electron microscopic rendering. Accordingly, H argues that the visibility of advanced degeneration in the montage does not rule out the possibility that some not yet visible degeneration may be hidden at the “time point.”

J responds to this last account with a token of agreement and then documents his argument in (86) with an instance. The instance of a “false positive” is detectable as an isolated intact-looking terminal in the degenerating zone (the stratum of entorhinal axons and terminals). This field of terminals was said to be relatively homogenous in its composition, with but a few terminals arising from brain cells with cell bodies in regions other than the entorhinal cortex. Although it is theoretically possible that the instance of an intact terminal in the field of degenerating entorhinal terminals was associated with a non-entorhinal cell, H uses it as a candidate “false positive.” In response to H’s citation of the particular instance, J (88–91) provides for that case as a “marginal.” (A “marginal” is a terminal-candidate which is incompletely visible as a countable terminal. Often, phenomena which appeared in the photographs were accountable as terminals whose association with a synapse did not appear in the plane of the ultra-thin section. The presence of a “post-synaptic thickening” appearing at the borders of a terminal’s association with a synapse and staining very “densely” was cited as a criterion for counting axon terminals in this project. In analytically marking instances of terminals, J and B circled the outlines of those terminals which were counted under the criteria, and drew “an X” over those which were recognized as terminals, but which were not adjacent to a visible “post-synaptic density.” J thereby asserts that the instance which H had located is not officially an instance of a terminal within the conditions of the project.

In (92) J then points out an instance (presumably of another “false positive”) which is associated with the criterion of a post-synaptic density. This instance is cited as a somewhat better case of an intact terminal in the degenerating zone. With its use, J collaborates with H’s work of finding exceptions to the prior demonstration by J and B. However, he is, as well, exhibiting his competence in analyzing the materials by correcting H’s use of that material. H then finds another “pretty good” instance in (95–7) and J assents to that finding in (98). Before any further instances are pointed out, J sums up the project of finding such “false positives” in (100–10). In this account he points out “two or three” in acknowledging those which had been located in the prior assertions and then asserts that the available cases are almost exhausted with those noticings. He passes his assertion (100–10) in an accountable order of “lookings” and “telhngs,” pausing between each announced observation. H acknowledges J’s account in (112) and then J asserts that no further instances “rilly jumped out” (a nice expression assigning an independence to the found-organization of terminals, while implying that the finding was obvious within a routine order of looking). This account implicates a prior analysis by J, thereby exhibiting for those present that J has not overlooked the possibility of “false positives” in his previous work with these materials. After a rather lengthy lapse (115) H gives a rather tentative acknowledgment of J’s prior utterance, and soon thereafter J amends his previous statement by adding another instance to the local corpus of false positives. He provides for this instance as one discovered at the moment which he “missed” previously. (“I just looked at it.” Either he looked and didn’t “see,” or he looked, “saw,” but did not “mark.” In any event he attributes the miss to his own activities, and not to any inherent ambiguities in the appearance of the phenomenon, as he did in the case of the “marginal”.

In (126) H asserts agreement, and characterizes the “chances” of finding “false positives” as “very few.” Note that J’s tally of “false positives” in (100–14) provided for those enumerated as exceptions, though he never explicitly made an assessment of the collection as “few,” “not many,” or “not enough to worry about.” Here H says what is more allusively projected in the way J previously formulates his count. Note further that in this utterance (126) H refers to the objects as “anything y’ed ever want teh call an intact,” while he initially referred to the items under analysis as “false positives.” In the modified account, which dismisses the consequentiality of the objects for the project, H emphasizes the indeterminate character of “ints” in the degenerating region. As stated previously in the analysis of line (84) such objects were not necessarily “false positives” but were also accountable as
intact terminals from other neurons than the destroyed ones. In (84) however, H had committed to the possibility that a particular instance was a “false positive” for the sake of the argument. J asserts agreement to H’s assessment and provides further in (129) that the montage “breaks right in,” in referencing the relative clarity of the division of the montage into discrete zones of intact and degenerating terminals. What is involved in this account is that a display of the “before” condition of axon sprouting is a display of the effects of a lesion on two adjacent layers of axon terminals, where one layer is affected by the lesion (the degenerating region) and one is not (the intact region). In the comparison of the “before” montages with “after” montages, axon sprouting becomes accountable as an overlap of intact and degenerating terminals in the region between the two (previously non-overlapping) regions. In saying that the display “breaks right in,” J is asserting that in the “2½ day” montage, the degenerating and intact regions are clearly demarcated, making the display applicable as a good basis of comparison.

H then (139-40) asserts agreement once more, and assesses the montage as “good.” J agrees in (141) and H further firms up the agreement in (143) to use the “2½ day” montages as a “baseline.” In (146) H further asserts that “the cleaner the baseline” (the more discrete the demarcation between zones of intact and degenerating terminals), “the less we need” (the fewer number of “day points” will be needed). J agrees in (147), and H specifies his prior assertion in referring to “further euth-long” in the day series. This is to say that if “2½ days” will do, then, for example, “three-day” montages will not be necessary to document the extent of degeneration, and any further production of “three-day” montages can be avoided for that purpose.

J asserts agreement to this last utterance and then asserts further that the montage in hand is not “sterile” but is “fairly clean.” In this instance, and in the prior instance in which H used the phrase, “very few chances” (124), we find parties formulating an “objective situation” with circumstantially relative assessment terms. We find matters of “how much is enough” solved for all practical purposes by it being asserted that something is “clean,” if not “sterile,” or “very few” rather than “too many.” The solution does not entail a use of a criterion of statistical significance and the work of calculating whether the given instance is acceptable within the numerically formulated range of tolerance. Instead, in these cases claims are presented and argued as matters of “common sense”; the “commonsensical” visibility of an array of intact and degenerating terminals is (e.g. in 152-3) made relevant as a determinate basis within a particular assertion format. This is to say that “common sense” is invoked circumstantially in presentations of claims which assert that something is “this way” without it having to be argued further through the devices of a proof. This does not mean that such claims are not, on occasion, subject to further argument; indeed, matters asserted in the “you know” format of “common sense” are usable by either party to an argument as extensions of the argument.

H asserts agreement to J’s assessment, and after a lapse J adds that “three more” montages at the “2½ day” point will be assembled by himself and B “in a couple of hours.” After a series of “nonsense sounds” J continues his display for H that things are well underway by asking B a question about where he currently is in the process of rendering the aforementioned “animals.” In asking this question, J exhibits the progress of his and B’s work for H’s benefit. B replies in the affirmative and cites the number assigned to the “animal” in the cohort. J then formulates a plan to do another “animal,” presenting the plan as something previously established (perhaps as a reassertion of a prior proposal (32, 35) which had not been clearly accepted or rejected in the intervening talk). J then proposes to intensively analyze the materials in hand (“2½ day animals”) and compare them to already completed montages at “six days.” This counters any proposals to do “animals” at time points intervening “2½” and “six.” This would provide for “2½” as a “before” and “six” as an “after” display of the inferred process of axon sprouting, with no further documentation of time points within that interval. J gives a qualified assertion of agreement, using the “I think” preface and modifying to a conditional tense. H then extends his account further with an account of the project’s viability, which will be assured upon a clear demonstration of axon sprouting in the comparison of the “before” and “after” montages. Accordingly, he asserts that unless an assessment shows that the comparison of the “2½ day” and “six-day” animals clearly supports the axon sprouting account, there will not be much point in pursuing this electron microscopic demonstration, and any further production of “animals” will have been wasted effort. (Note that the project of producing each “animal” as an electron micrographic display was a difficult and time-consuming task.) H then (182) asserts in support of his prior account that “numbers” of animals at the already available time points, and not further documentation of temporal intervals, are what “we need.” J asserts agreement and then H adds the phrase, “at this point” . . . formulating how what has been projected and agreed to at this “point” in the project is relative to that temporal situation and may be modified in the light of further developments.
In summarizing this session, the following general features of the way in which parties decide together on "what we have here" and "what we'll do next" are indicated:

(1) Matters of "how much," "how good," and "how frequent" are predominantly formulated in practical terms ("far enough," "the worst of that batch," "very definite," "pretty good," "fairly clean"). In these cases a quantitative formula does not describe these circumstantial usages of quantitative terminologies; the expressions are indexical to the projects at hand and show a sensitivity to an interactional environment.

(2) In showing a sensitivity to a local environment of interactively generated accounts by the same or other speakers, these formulations are displayed relative to prior assertions, assessments, and proposals in constituting agreements, challenges, and modifications of prior accounts.

(3) Until subsequently challenged or modified, these terms stand as determinate accounts, assessments, or proposals with a projective relevance.

(4) Displays of agreement act as locally relevant "validations" of prior accounts. Displays of partial agreement, disagreement, or no agreement often bring about subsequent modifications of accounts, assessments, or proposals.

(5) Modifications are serially produced by both parties until a "latest" account stands unchallenged.

(6) The "common sense" of recipients is often (rhetorically) invoked to enlist agreement for accounts, assessments, and proposals.

Conclusion

In the sequences of tape-recorded talk from a scientific laboratory presented above (exhibits 1-8), what counts as a notable finding, a definite anatomical entity, a thing's attributes, a procedure of measurement, an adequate display of data, and a plan of methodic action are asserted and reasserted in an interactionally sensitive manner. This is to say, (a) that statements of "fact" and assertions of methodological adequacy are often modified in scientific conversations, and (b) that these modifications exhibit an orientation of agreement or disagreement to other utterances in the immediate conversational environment. The way in which modifications are achieved in shop talk is not solely "governed" by the presumptively independent character of "the facts" or by the principles of logical discourse. Instead, modifications are devised as "moves" which draw out and use initially hidden aspects or relationships in reasserting "the facts" within an argument. Such reformulations occur in successive assertions in a disagreement sequence, and continue until a prior account is "validated" for all practical purposes by displays of agreement.

In the above discussion of modifications reference was made to arguments in scientific shop talk. These arguments are not to be confused with the phenomenon of logical or scientific argument which is discussed in the literature on rational discourse. The arguments which were studied here are empirical phenomena which are identified with the speaking practices of scientists in collaborative situations of inquiry. Spontaneously occurring arguments are not limited to scientific discourse by any means, as they are a familiar practice in vernacular speaking which scientists inherit in their use of ordinary discourse. These often discredited practices of "natural argument" are consequently part of the day-to-day work of scientific inquiry.

In asserting that an interior feature of scientific inquiry is the work of spontaneous arguments, the following topics are raised for consideration:

(1) Local verification work: in the immediate circumstances of interaction, assertions of agreement can accept a prior account as given, without that account being explained any further. It cannot be demonstrated at present how such agreed-to matters stand as constructional features of a corpus of understandings among lab members. Whether agreements in shop talk achieve an extended relevance by being presupposed in the further talk and conduct of members, or whether they are treated as episodic concessions to the particular scene which later have no such relevance, cannot be definitively addressed in this study. However, there is some reason to believe that agreed-to accounts are held-to in some circumstances, since laboratory records (such as the electron micrographs) are created in the course of projects as documents (or "transcriptions") of courses of action. In the above exhibits (particularly (1, 7, 8)), records are constructively modified and decisions are made to include particular records in a corpus of data on the basis of achieved agreements. The analytic records thereby embody locally achieved agreements in their stable visibility throughout the further work of the project, regardless of what later courses of action are taken (agreements may be "overthrown," but are nonetheless relevant at the point of their being reviewed and overridden).

(2) Explanation: one might say that matters agreed to in the exchange of conversational tokens have a "deeper" basis in the assumption by parties that an agreed-to account can be supplied with an extensive rationale. Accordingly, agreement would mark that a speaker need not provide anything further than would index
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a co-understanding. Analysis of the tokens of agreement would then not give access to any "scientifically defensible" arguments which would be "latent" in the expressions of agreement. This is a troubled notion, however. In the above exhibits there were occasions on which members displayed "uncertainty," or "qualification" when challenged subsequent to accounts which were expressed as unproblematic statements. In such cases, explanation did not take the form of a "backing up" of the claim with, for example, principled and lawful explanations. Instead, locally available details of objects in their descriptive formats were modified by explanations with varying degrees of "rigor" depending upon the operative interactive and practical circumstances. This is not to say that such explanations were specifically illogical or irrational, nor is it to say that parties could not have given impressive and carefully constructed arguments had they been given time to do so. Instead it is merely to point out that explanations used in scientific work exist in an empirical plurality of interactionally occasioned "types."

(3) The rationality of arguments: we are left with a question about the validity and rationality of the practices outlined above. In comparison to "proper scientific reasoning" the devices of vernacular argument appear "weak" insofar as they orient to the immediate issue of settling differences in whatever way will do in the local setting of discourse. If such arguments are governed by a transcendent rationality, that rationality is not discoverable other than by imputations that, for example, parties must be "keeping in mind" some notion of scientific fact or principle. What can be said to this? At this point I can only say that in the absence of a disciplined understanding of the phenomenon of "natural argument" – practices of argument as they occur in conversational settings rather than carefully constructed writings, debates, etc. – these practices cannot be evaluated for their rationality or irrationality.22 It is premature (it may always be premature) to do so. I have, as yet, only alluded to that phenomenon in a piecemeal fashion.

Notes

1 More comprehensive treatment on the topic of Agreement/Disagreement is found in Pomerantz (1975) and Sacks, Agreement Notebooks I, II, and III, compiled at the University of California, Irvine in 1976, currently held at the Department of Sociology, University of California, Los Angeles.

2 Two of the most well known of these experiments were Sherif's (1936) experiments on the "autokinetic effect" and Asch's (1952) line discrimination experiment. Asch had subjects participate in a "line discrimination test" along with two shills who were instructed beforehand to assert that one of two lines was shorter than another line placed next to it, when in fact the other line was longer. After hearing the contrived judgments of the shills, a significant percentage of subjects asserted agreement with the stooges' account, though they often showed visible signs of nervousness while doing so.

In this case, the experimenter had established an *a priori* objective difference in line-length which was used as a standard in contriving the experiment. Recourse to such a standard is not available in the above examinations of accounts in shop talk, since we do not have access to any extrinsic grounds for interpreting who is "right" or "wrong" in a dispute. Instead, the determination of such matters within the particular conversational sequences is elaborated.

3 There may be no reason to assume that discovery in science is always (or even often) witnessable as a "key event." Although sometimes a discovery may be formulated as a relatively discrete event in a project, such as a "crucial test," it will be contended here that many discoveries are accounts which retrospectively organize a series of laboratory activities, experimental results and non-results, and "small" discoveries into a "story" of a definite event or structure. Furthermore, any single experimental result is often taken as only an allusion to a discovery since the technical conditions of its accomplishment often lead to a partial distrust of a result until corroborated by a series of repetitions and variations on the original instrumental theme. Experimental events in the lab discussed here were attended with varying degrees of intensity. One event, which at the time was formulated as a key experimental result later became complicated by indeterminacies arising from technical troubles in the design of the experimental test. For a historical account of historical contingencies in the constitution of the notion of a discovery, see Brannigan (1981). Brannigan's treatment is suggestive, though it makes no reference to the local history of practices that the discoverer uses in assembling his accounts.

4 See Pomerantz (1975), pp. 77-86.

5 For an explanation of the transcription system used in this chapter, see Appendix.

6 As will be elaborated later in this chapter, a modified assertion provides for a re-hearing of the prior assertion, so that the apparent "objective" structure of the first assertion's semantic format is given an emphasis in the re-hearing which is alternative to the apparently definite interpretation.

7 This chapter will not systematically address the issue of how the "identity" of reference is preserved across utterances which are visibly different. It is claimed here that successively modified assertions "somehow" retain their reference to a same referent. This claim is not to be read as speaking about some same thing-in-the-world which remains indifferent to the manner in which it is described in successive assertions. Indeed the fascinating feature of modifications is that the accountable character of the object changes over the
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11 Modifications are not necessarily to be treated as modifications of the "meaning" of utterances. Instead they are treated here as modifications of the format of accounts - of the concrete ways in which accounts are "designed." Whether modifications "say the same thing" as the prior (unmodified) assertions though, for example, "the words" are different, is not a matter to be settled here. On some occasions it seems that members provide a modification as a clarification of a hearing of the first assertion. In such a case the modification is not forwarded as a retraction or change in what is first said, but is given as a claim that "this" is what the first assertion said all along (thereby providing for any disagreements that were occasioned as "misunderstandings"). On other occasions it seemed clear that modifications were, in fact, produced as changes in what had been asserted previously. In those cases the accomplished-relationship of the modification to the original utterance is one of "correction," "disagreement," "weakening a claim," or "retraction." This is especially so for modifications produced by different parties than the speaker of the modified utterance. These matters are not as clearly demonstrable with transcribed materials as are the varieties of formats used in modifications. In conversation, terms such as "never," "always," "everybody," and "everyone" enjoy alternative usages to the descriptive or utteral sense of them as exclusive expressions. Indeed, persons who challenge such alternative uses open themselves to the charge that they are being too "literal" in their hearing of the term. The varieties of conventional uses of the term "everyone" are discussed by Sacks (1975) in the following passage:

Consider such uses as "Everyone's going, can I go?" That is not - though it might be so construed - a paradox. "Everyone" there is apparently being used in a programatically relevant fashion.

Again, the upshot of this last discussion is: It may be the case that a determination of what "everyone" refers to, turns on the utterance and the occasion of its use; and, by virtue of some of the possibilities suggested - such as that "everyone" can be used programatically, can be used for a rather small set-population, can be used for categories - by virtue of these, an approach which sought, as ours does, to find how it might be possibly true, seems not necessarily to be burdened with what might appear a more attractive approach - and that is to find that it could not be true (i.e., to formulate such a sense of "everyone" as permits the location of falsifying evidence). Such an approach might simply not be terribly informative about the uses of "everyone." (pp. 63-4)

13 For further discussion of the use of "numbers" in everyday conversation, see Churchill (1966). For a more extensive account of the use of numerical formulation in laboratory accounts of natural "events," see Lynch (1977).

14 This contrast between formulations of "quantity" and "circumstantially adaptable expressions" is similar to Garfinkel and Sacks's (1970) distinction between "objective" and "indexical" expressions. There are some differences, however. Garfinkel and Sacks allude to the distinction as a phenomenon which is thematic to the work of philosophers, logicians, and social scientists whose enterprises are characterized by attempts systematically to replace "indexicals" with "objective" expressions. The paper can be read as suggesting that the "distinction" is an artifact of those enterprises rather than being postulated as two separate classes of terms by Garfinkel and Sacks (this distinguishes their treatment from Bar-Hillel (1954), for example). Garfinkel and Sacks allude to the distinction as a "reality" of "objective expressions" as a class of terms, but instead propose a study of the systematic uses of "indexical expressions" in everyday interaction. In this chapter, a distinction between formulations of "quantity" and "circumstantially adaptable" expressions does not refer to a difference between terms per se, but to alternative "hearings" of the identical terms which are circumstantially managed through the work of conversationalists. Accordingly, the availability of both sorts of hearing locates such terms ("numbers," expressions of "quantity") as "indexicals" whose "meaning" is at issue in the disagreement sequences we are analyzing. The "distinction" is thereby constituted within these sequences rather than being a stable and inherent classification of the terms in isolation from those sequences.

15 The use of "I know" as a preface in such sequences has an entirely different effect. Rather than equivocating an assertion, a modification in the form of, for example, "I know it is," acts to reassert the initial claim in an upgraded fashion.

That there is a difference between the interactional consequences of using "I know" as a preface for a claim rather than "I think," "I suppose," "I guess," or "I don't know, but . . ." may seem to be an obvious matter of the respective "definitions" of the terms. However, a "definition" of what it means to "know" is not likely to be very satisfying in accounting for the varieties of interactional uses of the term.

In regard to the disagreement "environment" specified above, it is conjectured that "I know" is an alternative among other prefaces.

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of the “I . . .” sort which appear in reassertions. As such a preface, “I know” marks a claim as “strongly” reasserted, and as not conceding one’s initial position. This preface exhibits a distinct sequential prospect for its claim which is not displayed with “I think.” This has as much to do with the interactional use of these terms in doing the work of disagreement/agreement as it does with any definable meanings of the terms. As a reminder of the futility of any attempt to reduce the work performed with a term to a definition of its meaning, consider the following passage from Wittgenstein (1965):

The idea that in order to get clear about meaning of a general term one had to find the common element in all its applications has shackled philosophical investigation; for it has not only led to no result, but also made the philosopher dismiss as irrelevant the concrete cases, which alone could have helped him to understand the usage of the general term. When Socrates asks the question, “What is knowledge?” he does not even regard it as a preliminary answer to enumerate cases of knowledge. If I wished to find out what sort of thing arithmetic is, I should be very content indeed to have investigated the case of a finite cardinal arithmetic. For (a) this would lead me on to all the more complicated cases (b) a finite cardinal arithmetic is not incomplete, it has no gaps which are filled in by the rest of arithmetic.

16 Coulter (1975, p. 386).
17 For same-speaker modifications there seems to be an issue of reformulating a prior assertion without necessarily “retracting” it. That is, the utterance is not merely reasserted in the face of a disagreement. It changes with a sensitivity to intervening utterances. It preserves elements of not only the format, but the “original claim” of the prior assertion as well. One way this is done is by providing for the original claim as not apparent in the way taken up in the challenge – as “misunderstood”; the modification provides a hearing that is compatible with the first assertion, though not necessarily anticipated on first-hearing (or by the hearing imputed to the other speaker). This practice reminds us of Wittgenstein’s (1953) number series paradigm, in which varieties of compatible extensions of the number series were imagined as alternatives to the “conventional” or “intuitive” extension (see Chapter 6). The imaginable extensions at first seemed “odd” in comparison to the line most readers take up given the initial fragment of the series. However, one quickly is given to understand how these “odd” extensions are demonstrably continuous with the initial fragment. In the analysis of the conversations above, it is not claimed that a particular “hearing” (or sequential extension) of the first assertions is a “preferred” or conventional alternative which the modification counters with its provision of a “re-hearing.” However, in those instances the recipient of the initial assertion exhibits a line on the assertion which the modification attempts to redirect.

Reference to Wittgenstein’s number series performs somewhat different analytic work here than it does for Phillips’s (1977). Where Phillips uses the paradigm as part of an argument against a “realist” philosophical position, the paradigm is here used as an analogy for a dialogic phenomenon in conversation. The work of “relativization” is here seen to be immanently part of a sequence of interaction, and specifically as a practice of argument rather than as a resource for an argument in the presentation of a general philosophical position. Phillips’s usage constitutes an instance of what I am analyzing (as might my own arguments if they were treated as instances of conduct in an interactive setting).

18 Saying that scientific shop talk was an instance of ordinary conversation, does not debunk science as merely ordinary talk. This motivates two further questions:

(1) How are vernacular practices identified with scientific conduct?
(2) How does the analyst miss whatever might be distinctive about scientific work by analyzing shop talk as practices of conversational analysis?

The first question is addressed in a preliminary way in this chapter by using transcripts of scientific conversations as exhibits of the conversational practices discussed under the heading of “modifications.” However, this analysis cannot be conclusive until the second question is addressed. The second question is not answered here by specifying whatever might be distinctive of science. Rather, I allude to the specific character of scientific practices by citing the failure of analysis (existing analytic specifications of methodological rules, norms, and networks of scientists, and including conversational analytic findings) to account for the technical details of actual instances of scientific conduct.

19 The possibility of a circumstantial analysis of laboratory work is discussed in the “Introduction” of Chapter 4.
20 In the above sequence, H’s reformulation of “’ss kinda unfortunate” to “shot this a little high, didn’t ye” follows what Pomerantz speaks of as a standard form for attributing responsibility in a blaming account (1978, pp. 115-120). Pomerantz characterizes this sequence as a two-step formulation of (1) “an unhappy incident” specified without the mention of an “actor/agent”, followed by (2) a “blaming” where an “actor/agent” is specified as “performing a blameworthy/praiseworthy action” (p. 116). Note that in the above sequence, J counters the “blaming” account by reformulating the “unfortunate” micrographs in a way that leaves his agency as the producer of those micrographs unspecified.
21 Sacks (Agreement Notebook III) provides the following commentary on the “closure” achieved through the work of agreement: “People do not explore the sources of their agreement as they do the sources of their disagreement.” Sacks’s analysis of a “preference for agreement” in conversation specified two distinct sequential courses
subsequent to assertions of agreement versus disagreement. Where disagreement occasioned further extensions, explanations, and elaborations of initial accounts, agreement brought these “inquiries” to a close. Accordingly, Sacks posited an intentional character to such sequences (though not necessarily to the persons speaking) in raising the following question: “... how much work will someone do to be able to find a confirming answer. They do attempt that ... if they don’t they are being ornery.”

In compiling a variety of analytic features of agreement and disagreement sequences, Sacks formulated a structural notion of a “preference for agreement.” (This is discussed throughout Pomerantz (1975).)

The possible bearing of such a “preference” on scientific conversations is not conclusively ascertained in this study, though a plethora of the analytic features which conversational analysts use in specifying the “preference” are discoverable in an analysis of scientific talk. Given a concern in science for elaborate explanations, extended inquiries, and careful argument, one might expect to find unusually heavy reliance on the devices of disagreement in scientific shop talk. Such a functional rationalization of scientific talk, however, is not obviously supported by our materials (though the materials are not extensive), as it appears that the practicality of “getting the work done” overrides any possible usability of extended “questioning” or “dispute” as a general organizing feature of the work. Instead, agreement and disagreement are managed as circumstantial features of getting the work done, rather than being organized in a “micro-environment” of “skepticism” distinctive of science.

These remarks do not constitute a “refutation” of Merton’s (1957) formulation of “organized skepticism” in his papers on the “ethos” of science. Merton provided for his “norms” as indifferent to the variations in the empirical practices of scientists in everyday settings of scientific conduct. In such settings, the idealized notions of distinctively scientific work are, as it were, polluted by other influences arising from science’s institutional identity with an “environing” society. The “norms” are instead provided for as trans-situational “designs” inherent in standards of rigorous method, experimental replication, careful attention to detail, and other answers which a scientist might give to a request for a definition of science while at leisure from actual working situations. (This is not to say that such definitions do not perform a service for science. As Mulkay (1976) points out, Merton’s formulations act as “ideologies” in a sort of historical advertising campaign for science.) Because of the way Merton’s idealizations of science are defined as inscrutably trans-situational norms, they cannot be upheld, denied, or otherwise addressed in an examination such as this of the visibilities of the work’s practical accomplishment. Indeed, an unfortunate consequence of the idealizations of science by Merton and others is that one might erroneously interpret this study as an exposé of the “transgressions” of these norms by the particular persons or laboratory arrangement studied here. I suggest an alternative, which is to suspend a reliance on the credibility of the idealizations of science while reading this volume.

22 In an early paper, Garfinkel (1967, Chapter 8, “The rational properties of scientific and common sense activities”) follows Schutz (1943) in pluralizing the notion of “rationality” in terms of an empirical variety of practices or “rationalities.” Accordingly, “rationality” is not upheld as a standard by which to evaluate instances of conduct, but is located with the practices themselves, as their immanent ways of achieving practically accountable results. The consequences of this program for the social sciences is a radical disaffiliation from the presumptive use of standards of “scientific rationality” in evaluating common sense practices of inquiry. Here it is suggested that the purported standards of “scientific” rationality, if held as exclusive criteria for judgment of adequate and acceptable work, create a paradox for science in their inability to account for the detailed actions which constitute inquiry in practical scientific settings.
The previous chapters have given an account of scientific work as it was observed in a particular research laboratory. The intent of those discussions was to specify the work of science as a social activity through an ethnography of day-to-day laboratory practices and an analysis of the “shop talk” which was an inseparable accompaniment to those practices. Specific attention was paid to artifact accounts as accounts of the social origins of scientific productions which were situated within the practical circumstances of laboratory inquiry.

The orientation to artifact accounts in this study (as a way of addressing the issue of the social origins of laboratory work) contrasts with the stipulative adoption by the social science analyst of an “anti-realist” position as a principled resource for treating scientific work to be amenable to sociological analysis. Rather than arguing on the basis of a general philosophical position that natural scientific discoveries, facts, and methods are inherently subjective or constructive in their origins, I have treated the matter of origins as a researchable matter to be held accountable to the “self-explicating” character of research activities. At the conclusion of the study I can say that the question of the social origins of laboratory discoveries is an inherently open question for those who make and defend such discoveries. To insist, in principle, that scientific work is essentially artifactual (subjectively or socially constructive) would be to overlook the situated relevance of questions of artifact during actual courses of inquiry.

Laboratory members’ accounts of the social origins of candidate neural phenomena were exhibited in previous chapters with reference to features of electron microscopic data displays (see Chapter 4), and in terms of interactively occasioned modifications of reference in laboratory shop talk (see Chapter 7). Artifact accounts were not produced as disengaged “descriptions” of laboratory work, but were intimately bound to the collaborative production of embodied courses of inquiry. Had those accounts been appropriated as decontextualized descriptions, they would have shown a senseless variability to the “sciences” reflexively revealed on different occasions of artifactual trouble. These attributions to scientific work of social origins, and the consequentialities of such attributions were inseparable from the work in progress in the “collaboratory” setting; including, among other matters the materially specified character of the inquiry, the requirements of its embodied performance, and its local history and local prospects. Any treatment of such accounts under the rubric of adequate (or inadequate) sociological description would fail to grasp the productive efficacy of such accounts while holding them to extrinsic requirements of completeness and theoretically informed consistency. This is to say that artifact accounts did not stand proxy for a sociological observer’s description of scientific work, but were instead exhibited as endogenously produced observations and claims for the practical purposes of doing competent biological research.

A distinctive characteristic of artifact accounts was that they were practically consequential; their production and resolution as claims about the “constructive” origins of specific phenomena of interest were concretely part of the on the scene management and determination of specific inquiries. Consequently, it would do violence to the situated competence involved in the practical use of artifact accounts for this writer to adopt or invent a principled position on the general problem of science’s relation to its subject matter (by arguing, for instance, that such a relation is one of subject-object correspondence, social construction, or subjective constitution). Each of the available positions is supportable with selected examples of accounts authored by particular scientists or attributed to ideal-typical scientific “actors”. However, treating the contingent productions of scientific work as evidence for principled epistemological positions and arguments holds the work accountable to issues and questions which are not demonstrably part of the practical setting in an invariant fashion. The preference in this study was to do the reverse; to address the issue of the social character of scientific findings in a fashion which would be informed by and accountable to the instance-by-instance specifications by lab members of the artifactual character of their findings.

This preference entailed my holding my “anthropological” resources in abeyance in the face of an indigenous “archeology” which preceded and superseded whatever I might say about the work that produced laboratory artifacts. By appropriating that
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inquiry as a basis for my own, I gained an appreciation of how the social origins of scientific facts were continually being rediscovered as a productive feature of the lab's inquiries.

The practical sensibility\(^4\) of artifact accounts was reflexive to a competence with particular experimental and observational practices in an instrumental complex. Such a competence in the particular lab studied entailed a detailed familiarity with brain anatomy, physiology, biochemistry, and the technical practices appropriate to the investigation of those phenomena. This is to say that the competent posing of situated questions on the "constructive" origins of laboratory products was accessible from within a highly technical order of affairs. The technical access to these issues of the social production of natural order was distinct from those technologies of inquiry which are featured in sociology methods texts and departmental pedagogies.

That the sensibility of artifact accounts (what was locally at issue; which neuro-anatomical phenomena, instrumental technologies, embodied practices, and personally distributed competences) was not extractable from their occurrence within specific courses of laboratory inquiry poses several related problems for the study of work, and more specifically, for the study of scientists' work. Thus far, I have not extensively commented on these issues, though at the conclusion of the study it is perhaps appropriate to address them. The following section on "distinguishing the natural sciences from the social sciences" will be developed in terms of the practical availability of that "distinctness" as a reflexive feature of this study.

Distinguishing the natural sciences from the social sciences

For the purposes of the discussion that follows this question will be treated as interchangeable with the question of the distinction of science from "common sense". A first comment that can be made about the question is that it is a misleading question when regarded from an ethnomethodological standpoint, insofar as it orients us to an image of the sciences as a uniform body of methodological or epistemological criteria. Since, as stated before, I have chosen to turn away from the stipulative formulation of general rules as an account of scientific work, and have preferred to examine scientific work as embodied practices in specific settings of conduct, the question invites the very programmatic stance I have specifically discarded. However, the question is relevant to this study when heard to betoken a practical distinction, not between the social sciences and the natural sciences, \textit{per se}, but between the situated adequacy of a social science back-

\(^4\) Ground and the technical masteries that make up competent shop work and shop talk in a natural science research laboratory. The distinction arose in this study in terms of the practical troubles for my inquiry engendered by the unknown technical practices and technical meanings which exhibited the work in what seemed to be an unremarkably orderly and apparent manner to members, but which eluded my comprehension to the extent that I was a novice to the lab's work. Needless to say, an education in social science methodology did not and could not anticipate the technical skills required for competently doing biological lab work. The inaccessible character (to myself) of the action did not necessitate the conclusion that such actions were of no relevance to the study of social order, since, clearly, lab work was visible as a multi-party activity in which persons working together and talking together collaboratively organized their work and its outcomes.

As shown in the analysis of artifact accounts (Chapter 4), the social order in the lab was inseparable from the natural orders of neurobiological phenomena which parties were engaged in discovering (this identity is perhaps better put as: the social order in the lab was most cogently and integrally present in reference to specific biological phenomena achieved in the work, as practices manifested in the observability of biological phenomena). As a programmatic matter, the technical character of the work was not other than a social phenomenon, though it was one that was opaque to a competence limited to the practices of natural language and the distinct technologies of analytic sociological inquiry. To abstract the "social aspects" of scientific work from their technical context on the basis of common language resources or the application of schemata of general analytic theorizing and literary exegesis, immediately omits questions on how the technical work of the laboratory exists in a discovering relation to its phenomena. In the absence of an orientation to such questions, the work of science becomes defined in terms of institutional networks, or refined applications of common sense or natural language theorizing, and becomes indistinguishable from what sociologists already know by virtue of being sociologists.

Collins and Pinch\(^6\) provide an apt example of how the externally appropriated indicators of "science in general" are of little practical avail when treated as the identifying features of competent scientific work. The authors observed that parapsychologists have recently attempted to legitimize their work \textit{vis à vis} the natural sciences by mocking-up the institutional and exogenous features of scientific inquiry (founding "research institutes," starting journals devoted to parapsychological research, using "technical hardware\(^8\)" and principled versions of experimental procedure and
resulted in some limited success, though for the most part the substantive claims of the parapsychologists continue to be disregarded and dismissed by natural scientists. Though it may well be the case that the parapsychologists are the victims of an incorrigible prejudice by many natural scientists, it seems just as likely that the situation is akin to that of the urban poor who attempt to mock-up the exterior appearance of the wealthy by purchasing indicators of wealth (fancy cars, clothing, gold watches, etc.) thereby impoverishing themselves further, and, in the intractability of the contextual appearance of these indicators, transforming the indicators of wealth to the emblems of a conspicuous poverty.

Similarly, Merton's recent overview of the sociology of science proposes that the "nascent speciality" is "self-exemplifying" in the way its institutional characteristics correspond to those of emergent natural science specialities. Merton cites a number of indicators from conventional sociological studies of science which are then applied to the "sub-discipline" itself in demonstrating its identity as an instance of its phenomenon. Such parallels in the academic, administrative, and institutional form of the respective specialities indicate common bureaucratic environments within which the natural sciences and social sciences are practiced, but cannot account for the disciplinary specific discoveries of, for example, biology or physics which many natural scientists invoke when claiming the superiority of their practices to those of social scientists. Clearly, the identifying phenomena of a discipline are inseparably part of an integrated practice with a distinct pedagogy, unique technologies of inquiry, and a corpus of relevant discoveries. Although one interpretation would have it that disciplinary boundaries are to be respected if sociologists are to continue to do sociology, biologists to do biology, and physicists to do physics, an interest in the social production of discovering work cannot adequately address its phenomenon by remaining isolated within the parent discipline of sociology.

The character of this study

Following from the above, it is obviously absurd to claim that the foregoing study was, from its onset, a scientific study, or that such a claim could be specified in terms of a criterional application of the study's "findings" to the study itself. Tempting as it is to claim "scientific" authority for this study on the basis of a formulation of its method of inquiry to be like that of its subject matter, I cannot do so for at least two reasons. Firstly, there is no clear reason to suppose that neurobiological research should be exemplary of the problematics and methods of a study of social order. Since, as stated before, an orientation to the disciplinarily distinct organizations of scientific work is a thematic component of the present study, it would be a curious recommendation to suggest that a separate discipline of study should be exemplary of this particular one (in the general sense of defining methodological procedures to be applied to social phenomena of all kinds). Secondly, although a scientific setting was the "object" of this study, the adequate observation of which entailed an appropriation of the locally used scientific methods, this particular study cannot claim to have gone so far as competently to master those methods in other than a very limited sense. Since, as stated before, my appropriation of the technologies of lab work was limited to an interpretive competence with the microscopic data from a
particular project, and a bystander's appreciation of a somewhat wider range of procedures and problematics of the lab's various inquiries, the accounts of scientific work provided in the previous chapters are vulnerable to the counter-claims of observers/practitioners who can demonstrate an embodied access to the practices of scientific work. The adequacies/inadequacies of the observational-and-descriptive methods used in this study are therefore most pertinent assessed ("tested") from the standpoint of the locally practiced scientific methods which were studied rather than with reference to abstractable tests of social science validity and reliability.

In another sense, however, the "findings" of this study are applicable to the reflexive analysis of its inquiry as a way of elucidating or disclosing its presence to a subject matter. Doing so does not, however bolster any claims by the study to a scientific status in the absence of an essentializing grasp of scientific practice. An application which can be attempted involves the question of how this study is subject to issues of artifact, and how, in being subject to artifactuality, the character of its inquiry is disclosed. A concern with artifact can be readily located, and is a persistent feature of the way the study delineates its phenomenon, addresses that phenomenon, and, in the end, finds that the ways in which it has "missed" that phenomenon constitute productive uncertainties for further work.

The concern with artifact in the previous chapters appears simultaneously with the thematic orientation to an in-itself subject matter. The in-itself is delineated in terms of the endogenous features of laboratory inquiry which are other than those natural language, literary, and sociologically technical resources which provide an initial way of access to the phenomenon. The struggle with the recalcitrant phenomenon, a struggle which validates the experience of [it] as independent, emerges as the insufficiency of the author's resources adequately to "contain" or apprehend the phenomenon. Simultaneously, the phenomenon is available as an obscured presence, a baffling presence, or a series of failures, corruptions, and refutations of the author's mode of addressing [it].

The adaptations entailed in the circumstances of the inquiry were both remedial and constitutive elements of its situated troubles, and involved the continuing attempts to render the embodied practices of lab researchers into a written availability. Simultaneous with this, these troubles with the literary practices were projected on behalf of the endogenous production of textual formats of data and the eventual "writing up" of courses of lab work into research papers. However, while it was reasonable to suppose that the eventuality of endogenously produced texts was intentionally part of the way in which projects were designed and produced as embodied work, no such intentionality could be posited for the relationship of the work's endogenous production to the disengaged writings of the author. An essential artifactuality inhabited this "distance" and was other than that artifactuality which inhabited the recognizability of technical practices in the textual remains of laboratory inquiries. An attempt was made to secure the author's accounts with reference to the artifact accounts which were produced by lab members in the course of their work, though this "security" was nevertheless vulnerable to the evident difference between the practical observability of the work to its producers and the author's appreciation. The substantive character of the described phenomenon and the methodological adequacy of the description were simultaneously called into question by virtue of that difference.

Various attempts were made in the previous chapters to produce textual exhibits (in vitro demonstrations, if the metaphor is to be carried further) of the embodied (in vivo) work of laboratory projects. The resources of a disengaged observer's "behaviorized account" of projects (see Appendix I) were used to allude to the temporalizing features of laboratory work. The artifactuality of such ethnographic accounting was evident not only in the inability of the author to bodily perform the in vivo practices, but was also involved in the disengagement of the sensibilities of writing from the situated, tactile, thoughtful, visual, effortful, and conversational work of doing a laboratory project (nor did the format used access the visibility of the bodily work as a "movie" rendition – an observable course of exogenously recognizable bodily movement). Again, this disengagement of writing from the embodied action was displaced from the relationship of the ethnography to the laboratory work, and asserted as an interior feature of the practical troubles of written methods, though the adequacy of this displacement was again "uncertain" on the basis of a difference in competence. Subsequently (Chapter 4) an attempt was made to locate the work of the lab in the textual formats of its data. As a limitation on the innumerable artificial readings which could be made of the data (conceived as texts to be analyzed without constraint), a number of accounts authored by lab practitioners were collected and explicated in terms of how they attributed a "social" order to those which were produced in situ, the manner in which practitioners' references to artifacts implicated unmentioned features of
a local history of embodied practices was essentially uncertain and could only be speculatively addressed. Therefore, although a common textual resource (the electron microscopic data together with lab members' verbal accounts) was used in this treatment, it could not give direct access to the course of bodily work which presumably produced the textual features analyzed. Once again, this problem was seen to inhabit the in situ work of examining the documentary displays of data, though, perhaps in a different way.

Finally, audiotape recordings of practitioners' shop talk were rendered into the format of detailed written transcripts (Chapter 7) and analyzed for the ways in which agreement was achieved in the actions of parties to the discourse. Two essential sources of artifactual concern inhabited the analysis of these texts. One involved the manner in which the audio transcripts provided for an accessibility to the work of science which was visually and tactualy "blinded". The resources of ethnographic "back-grounding" alluded to the visible presence of the embodied surroundings of the disengaged sequences of the talk, but these ethnographic accounts were not productive of the comparatively rich circumstantial detail which was analyzably present in the recorded talk. Consequently, the analysis of the talk (in the form of written transcripts) construed the work of science to be a praxis using talk as materials, with but a conjectural relationship to the material presence of the work of science in the embodied setting from which the talk was abstracted. The second source of artifactuality involved the use of conversational analysis to get at the work of science. Although the ways in which the talk of scientists exhibited more generally available features of conversational organization could be specified in detail, it was far more problematic to assert that these features of conversation had anything in particular to do with what might be distinctive of the talk as scientific talk.

These artifactual features of the study are not listed as an apology for incomplete or inadequate work; instead, they point to a phenomenon. The phenomenon has to do with the orientation to a "productive mechanism" in the temporal character of the phenomenon studied (analogous to the unknown chemical factor which simultaneously eluded and motivated the attempts to produce an in vitro demonstration of the temporal sequence of axon sprouting). Artifactual troubles resided in the attempt to appropriate that phenomenon so as to make reference to its existence in a textual demonstration, where the very apparatus used to make the demonstration raised questions over its adequacy. A demonstration would be a practical appropri-
some adaptability of those materials from one context to another. What also needs to be addressed as a certain mode of bodily orientation to those materials which distinguishes the practices. Consider, for the moment, the material, paper, which is used in inquiries of all sorts and provides an exploitable “bridge” between activities. For those whose inquiries analytically employ the properties of paper (analyzing literary texts, tables, graphs, diagrams, photographic displays, questionnaire items, and conversational transcripts) it may seem strange to speak of such work as a praxis with paper, since in the ways in which these inquiries speak of themselves the materiality of the paper goes without mention, and is merely the concrete and invisible background for the abstract “contents” of the representational systems which “happen” to be written on paper. Whatever is “on” the paper might as well be on a blackboard, microfilm, canvas, or any other material upon which writing can be done (or, for some purposes, reproduced as a course of talk). As a praxis with paper, social science inquiries never seem to “touch” the materiality of the paper, or if they do so, it is not explicitly part of their accounts of method. Compare this with the following incident reconstructed from field notes taken in the lab studied:

The paper doll method

Researcher H. was very proud of a method he had invented for solving a measurement problem, and was happy to demonstrate his “paper doll” method to me. The analytic problem solved by this method involved an attempt to detect and measure the extent to which the Commisural/Associational layer of axons had expanded with respect to the Entorhinal layer of axons, subsequent to the degeneration of the Entorhinal axons resulting from an experimentally induced lesion. Lesions were performed on a number of animals, which were then sacrificed after a lapse of days following the lesion. This lapsed recovery period ranged from two days to over one-hundred days, though in most cases it was between two and eleven days. Light microscopic cross-sectional slides of the hippocampus were developed under degeneration assay techniques (which distinctively stained the degenerating axons, presumably in the Entorhinal layer of axons). The slides from different animals were indexed to variable lapses of time in the recovery period from two to eleven days and were projected at a standard power of magnification by means of a camera lucida (a projecting microscope which projected a magnified image on a paper screen placed beneath it) onto a sheet of white paper, and traced by hand by lab researchers. Traces were made of the linear “borders” of the intact and degenerating fields of axon terminals “above” the granule cell layer. Traces for “2-day” and “10-day” (before and after the presumable occurrence of axon sprouting) slides looked something like Figure 8.1.

The discovered method made use of a “hidden” property of the graphic display; that property being the incidental fact that the graph was written on the surface of a sheet with a relatively uniform width and weight per unit surface area. This property was exploited in solving a problem which arose as a visible feature of the graph. The discovery was “counter-intuitive” in the way it brought into play a praxis with paper which was continuous with, but not included within, the operations on the paper which treated it as the incidental ground of a two-dimensional visual image. It might be far-fetched to suggest that the availability of the discovered-praxis with the paper was continuous with the emphasis in lab work on the embodied and manipulative access to a dissectable object of inquiry, such that paper could be discovered to be other than the invisible ground of a representational text. However, the properties of paper which were exploited in the method relied upon the circumstantial presence of a scale, which “happened” to be on hand for weighing minute quantities of chemicals used in the lab’s preparations. The indexical properties of the paper (its locally operable properties) were thereby reflexive to the availability of distinctive materials at hand in the immediate laboratory setting. Clearly, more was involved in the invention than what could be explained through an inventory of “material” features of the immediate lab environment, since the relevance of the scale to the measurement problem was researcher H’s thoughtful-and-bodily achievement. The point being made here is that the appropriation of a “text” or “record” as a common ground of analysis between a scientific practice and a social science analysis of that practice cannot be assured of the properties of such a text in isolation from the mastery which brings the text to life in a specific instrumental complex.

The “paper doll” invention raises the question of how laboratory practices are to be re-presented in a textual demonstration. Even in the instance where the resources of “paper” texts are treatable as common facilities for natural and social science inquiries, the issue of what constitutes the facticity and usability of a “text” is confounded by the distinct practices of the respective disciplines. Lines and words on paper become appropriated within different practices of inquiry, and, in the instance above, the very facticity of “paper” itself was indexically tied to an inquiry’s local rendering practices (making the matter more than an “interpretive” problem). It is in this sense that the very material of identification runs through an ethnographer’s fingers.
Figure 8.1 Reconstruction of the "paper doll" method, showing comparison for "traced" outlines of "2-day" and "10-day" microscopic preparations

The dotted line in the figure (A) was drawn to roughly interpolate the "dividing line" in the projection between the darkly stained (degenerating) axon profiles and the faintly stained (intact) layer of axons. The lower frame of the projection was set along a roughly standardized distance from the upper edge of the granule cell layer [G], and the magnitificational power of the various slide projections and the cross sectional locations of the microscopic sections in the three-dimensional shape of the hippocampus were held relatively constant. Through the use of the careful work of approximating such standards in the practices of sectioning, slide preparation, slide projection, and tracing, the traced-projections were given a graphic availability and comparability. A comparison of the various graphic renditions showed a visually detectable "expansion" of the I/C layer with respect to the E layer (note the difference between the 2-day and 10-day tracings in Figure 8.1) at some time during the sequence of days. Although this "expansion" was visible, it offered some problems for accurately measuring the degree of expansion, since the dotted line (A) which interpolated the "boundary" between the two axon strata was quite irregular in most instances relative to the upper edge of the granule cell layer (B). Numerous solutions to this measurement problem can be imagined on the basis of taking the traced lines as discrete series of points within the planar and rectilinear confines of the "graph." (For instance, one could take a series of linear vertical measurements aligned with the linear edge of the projection and reckoning the distance between lines (A) and (B)). The solution which was achieved was less complicated than this and differed in the way it concretely employed the indexical properties of the rendering; treating the graph as discrete zones of a substantive mass of paper to be dissected and weighed. The solution consisted in cutting along the drawn outlines of the I/C layer (lines A and B) with a scissors, thereby isolating that layer from the rest of the rectangular sheet of paper corresponding to the borders of the projection. The segment of paper was then weighed on a finely calibrated balance, and the resulting weight was divided by the total weight of the entire rectangle of paper within the frame of the projection (this latter weight was a constant, for all practical purposes, given the use of the identical grade of paper). The ratios were converted to percentages, and these percentages were compared across the various "day" recovery periods for which slides had been produced. A reliable figure of slightly less than 25 per cent was computed in this fashion as a measure of the extent to which the Entorhinal layer had expanded subsequent to "axon sprouting."
CONCLUSION

It may very well be the case that the artifactuality of this study is inherent in any attempt to address a phenomenon that has attributed to it endogenous features which are then specified in accordance with the ordinary apparatus of an exogenous form of inquiry. For a study that is already committed to its phenomenon in the very fashion of its being already underway, this makes little practical difference, though there is little comfort to be taken in the realization that artifact is inherent in the way in which the inquiry proceeds. The concern with the artifactuality of the relationship manifests as improvisational work with the research apparatus in a particularizing relationship to the current situation of the inquiry. Whether or not such situated work is ultimately adequate as a basis for the ecstatical mode under which the inquiry localizes itself in terms of its phenomenon cannot be settled in advance without denying the inquiry of its productive uncertainty.

Following from the above, certain suggestions can be made on how the “exotic” practices of natural science inquiries might be more adequately demonstrated than in the present study. These suggestions do not entail an ultimate solution to questions on the origins of scientific practices, but instead are technically committed to the identification of social phenomena with materially specified natural science competences. Firstly, in taking up the recommendations of Harold Garfinkel’s Manual for the Study of Naturally Organized Ordinary Activities, one would want personally to attain a deep competence with the practices of the inquiry being studied, entailing an apprenticeship at designing and performing embodied courses of action with the appropriate sets of laboratory materials in the company of professionals. This is, by no means, an easy undertaking within the conditions of a professional career in sociology, nor is there full certainty that anything “newsworthy” will be discovered by adhering to such a seemingly appealing, authentic, or “grounded” way of approaching the phenomenon. However, such a project of action is more than a conceivable possibility in the current state of ethnomethodological research. Secondly, the demonstration of these practices as more than a textual referral to what one has learned or knows how to do may entail an inventiveness with the ordinary apparatus of social science presentation to enable the production of analyzable courses of “exotic” action as concrete features of a presentation. The use of video records in the context of an analysis by one who knows how to do the activities shown is a partial solution, though ultimately one would want to “package” the very material objects through which the competence takes on its properties into the format of a demonstration. The production of such a “museum” as the materializing elements through which the social organiza-

tion of a practice becomes demonstrable would, no doubt, entail some creative violence to what constitutes a social science “paper” presentation.

It is hoped that the reading of this volume will have convincingly pointed to the necessity for practical demonstrations of competent science as a format for proposing and addressing questions on the “social” origins and conditions of scientific work.

Notes

1 Among the many social science accounts which affiliate to such a position as an in-principle way of beginning an analysis of natural scientific inquiry are recent works by Phillips (1977) and Knorr (1977). Phillips’s account has been discussed at length in Chapter 4, and Knorr’s paper is discussed in note 5 below.

2 The phrase, “self explicating” is borrowed from Pollner (1979), where it is used in an account of the social organization of traffic court hearings. Pollner reports an ethnography where he finds issues in the social organization of courtroom hearings to arise in situ via the orientation of prospective defendants to the performative features of prior cases. According to Pollner, the orderliness of a ‘string’ of cases arises as a function of the exemplary character of the details of a case’s action for onlooking novices (defendants). In the above passage I am alluding to the way in which research activities run their course while simultaneously showing how they run their course as a productive feature of how they do so. This is other than a matter of the visibility of actions to an ethnographic observer, as it involves the reflexivity (Garfinkel, 1967) of the observability of actions to the local settings in which they occur. This reflexivity is not identical with “reflection”, or an ability to talk about activities from an “experienced” point of view; instead, it makes reference to an empirically researchable phenomenon, which is the embodied work of doing ordinary activities in the company of others, where the temporalization of the activity (its co-ordinated sequencing, rhythematicity, and identifying features) relies upon the practical-observability and consequentiality of the activity-in-its-course to those who together produce it.

3 What is meant here is that the progression of artifact accounts in Chapter 4, and the uses of “qualified” assertions in Chapter 7 do not nicely “fit” any of the available theories of unified science. “Positive artifacts” were described as discrete, substantive, easily detected, “worldly” phenomena showing distinguishable formal characteristics. “Distortions” implicated the interplay of instrumental “perspectives” or “reality testing” with no ultimate assurance that unrecognized “distortions” did not essentially inhabit the worldly phenomena observed (these might be called, “Kantian artifacts”). Negative artifacts made the “pragmatic” or “existential” character of the work perspicuous; the management of
unknown circumstances with incompletely rationalized programs of action, and an orientation toward “making it work”. Similarly, in laboratory shop talk the introduction of “subjective circumstances” into the format of an object’s conversational assertion had varying effects on the subsequent courses of talk and action.

Holton’s (1978) study of the Millikan-Ehrenhaft controversy over the unit charge on the electron illuminates the way in which artifact accounts were featured in the competent management of research results over the course of repeated “runs” of an experiment. Although Millikan’s accounts of those “runs” that he rejected for presumable “artificial reasons” could easily be taken as ironic evidence of the “biased” or “interested” basis of the accounts, to do so would be to miss how the accounts were necessitated as part of the competent work of detecting the unit charge of the electron. A distinguishing feature of Holton’s historical inquiry into the case is that he examines Millikan’s laboratory notebooks, paying close attention to such details as “failed” experimental runs, recalculation of values of the electrical charge on particular oil drops, and marginal comments by Millikan which note whether particular results were “beautiful” (to be published) or whether they implicated “something wrong” in the performance of the experimental trial (pp. 63–72). Of a series of 140 experimental runs of the oil drop experiment, Millikan selected 58 “good” results for his published demonstration of the unitary electronic charge and accounted for the remaining results, which did not so clearly support his calculation of the unit charge of the electron, as artifacts and errors of various kinds:

To cite some difficulties recorded in Millikan’s notebooks, generally against “failed” runs: The battery voltages have dropped; the manometer is air-locked; convection often interferes; the distance must be kept more constant; stopwatch errors occur; the atomizer is out of order. (p. 69)

Rather than denigrating this practice as a source of experimental “bias,” Holton credits the practice with being part of Millikan’s artful work of preserving his findings from the uncritically examined “empirical results” (p. 71). Millikan’s discernment of which experimental results were to be attributed an accurate relation to “nature” and which were to be held as “suspicious” in light of the contingencies of the technical work of doing the experiment (and in light of the temporal relationships between the single result and a series of previous results) prevented the hypothesis of the unitary electrical quantum from being disconfirmed by discrepancies in the results of specific experimental runs. This work is not available in examinations of Millikan’s published writings, nor is it supported by canons of experimental procedure and logical argument. Nevertheless, Millikan’s achievements would not have been possible in the absence of such ad hoc assessments of the adequacy of each experimental run in relation to his preconceived ideas on the nature of the electronic charge (ideas which were presumably being tested by the selfsame corpus of experimental runs).

In contrast, Ehrenhaft’s research group apparently did not resort to such situated discriminations between “good” and “artificial” results in their experiments, and as a result produced “data” showing a much wider variance to the measured-electrical charge than in Ehrenhaft’s version of the experiment. The range of the values did not show the quantum “jumps” provided by Millikan’s discriminated data, and resulted in claims to the effect that “sub-electrons” of charge were discerned through the more refined experimental techniques of Ehrenhaft’s group. Millikan’s results “survived” the controversy, and showed a greater congruence with contemporary and successive developments in physics. Though the historical vindication of Millikan’s practices does not necessitate the conclusion that his results were more “correct” than Ehrenhaft’s, Holton’s account suggests the extent to which non-formulated practices at managing the possible-artifactuality of results are constitutive of what counts as research competence. Artifact accounts are thereby to be examined in terms of the efficacy of their use in a research situation.

A global use of an argument that scientific work is constructive may be applicable to a debate with philosophical “realists”. However, it fails to address those situated “problems of reality” which distinguish discovering work from philosophical speculation. Knorr (1977) poses the question, “is scientific work descriptive or constructive?”, adopts an “anti-realist” position, and uses field-noted interviews to document the constructive character of laboratory work. Knorr’s approach is relevant as a point of contrast to the research decision used in this study. (Knorr’s account is singled out for such contrastive purposes, not because it is a defective study among other studies in the sociology of science, but because, unlike other studies in that corpus, it addresses itself to issues in the local production of scientific work, creating a condition of affinity with the present study that enables critical dialogue.) In the present study it is concluded that Knorr’s question is addressed anew on each occasion in which laboratory data are produced and reviewed, since the concrete visibility and indefinite possibility of artifact haunts the work in an ongoing fashion, and, in so haunting the work, is essentially part of the finding, testing, announcing, and arguing over results. Although I agree with Knorr that scientific work is visible as embodied crafts or rendering arts (see Knorr, 1978, pp. 674–8) which are inadequately formulated under the imposition to the research practices of concerns for “descriptive adequacy” (p. 673), to speak of these practices as “constructive” is to gloss over the more delimited (and more consequential) meaning of “construction” which inhabits the competent recognition of artifacts in actual research settings. In another sense, the temporal organization of shop work, and the interactive
achievement of agreement in shop talk, can be treated as a "constructive" praxis (as has been elaborated upon in Chapters 3 and 7) in the social organization of inquiries, but such a formulation fails to address the crucial issue (to researchers) of how these practices are competent to the production of adequately (or inadequately) "real" results.

6 "Practical sensibility" refers here to the visibility of the artifact which relies upon and makes continual reference to the experiential circumstances of lab work. Access to the "guided projection" (Gombrich, 1960, p. 203) afforded by participation in extended projects of lab research is not assured by the analysis of the artifact as, for example, an isolated visual object.


8 Ibid., pp. 242-4.


10 Merton (1978, p. 10) states in citing a study by J. Cole and H. Zuckerman:

They distinguish between specialties that are derived from or are at least congruent with the theoretical traditions central to the encompassing discipline and the specialties, such as molecular biology or sociobiology, that are intellectual hybrids, deriving from two or more disciplines and spanning departments in the organization of science. Cole and Zuckerman hypothesize that the first type, central to the parent discipline both cognitively and organizationally, encounters less initial resistance from practitioners in the discipline than the hybrid type. Their analysis locates the sociology of science (at least in the United States) centrally rather than peripherally in the larger discipline.

Note that by locating the sociology of science squarely within the discipline of sociology, and treating the form of its relationship within sociology as a parallel construction to the relationships of specialties within natural science disciplines, the issue of whether or not sociology is to be treated as a science in the first place is never in question. Were it in question, it would be senseless to use both the results of sociological analysis and the exterior form of sociological institutions described through that analysis as bases for demonstrating a "scientific" sociology of science. It is not as though the present study has definitively answered that question of whether sociology can be treated as an adequate instance of science. Indeed, no attempt has been made to operate on such a general level of analysis in this study. As a practical matter, however, the question was treated as an unsettled issue during the conduct of this study.

Rather than taking refuge in an authoritative use of sociological versions of "scientific method" as a principled basis for analyzing natural scientific practices, the alternative was taken, which was to treat "scientific methods" as practices to be come upon, observed, and learned during the study as a basis for demonstrating the social organization of those practices. The difficulties of proceeding in this fashion are compounded by an inability to claim the authority of canonical versions of science as grounds for procedure and findings. The advantage is that, despite the anxieties occasioned by the absence of a standardized "method" as an initial resource, the procedure allows for an interested orientation to those endogenous "methods" which do the work of specifying and exemplifying how intersubjective phenomena are brought into existence by the actions and inter-actions of research colleagues.

11 I am indebted to Professor Duane Metzger, School of Social Sciences, University of California, Irvine, for suggesting to me that an ethnographic study of laboratory work could not take refuge in the presumption that the conceptual resources of the investigator were adequate to the recovery of the "simpler" abstractions of members, but that an investigation of the more "complex" conceptual apparatus of natural scientists entailed a going beyond the available methods of social science inquiry.

12 Holton (1963, 1975) speaks of the thematic elements of scientific thought to be commitments which direct a scientist to his or her subject matter in such a way that the thematic commitment is demonstrated in the successful performance of research but is not derived from the particular results of experiments. The term is used rather loosely in the above passage in this text to suggest that, although it would be an easy matter for this study to document innumerable ways in which the laboratory inquiries observed were similar (that is, showed corresponding analytic features) to social science inquiries, the ongoing pre-occupation of this study has been to pursue those observable aspects of the lab's talk and conduct that were not readily understandable from the outset on the basis of the author's natural language and social science "skills".

13 The possibility that a study of science might attain to an essentializing grasp of the inquiry studied is no more than a conjecture in the present study. However, Garfinkel (1977, pp. 60-71) provides programmatic support for such a conjecture based on ethnmethodological inquiries on queues, conversation, and mathematicians' work. "The maxim of the unique adequacy requirement of methods" is specified by Garfinkel as stating (p. 60), "that a method, to find the phenomenon of interest, will be adequate in the way the method is already a possession of the object that it finds." An implication of this is that an ethnmethodological study of a natural science practice founded in a deep competence with its subject matter could discover ways of embodying the science studied in its own inquiries and demonstrations. This would not necessarily mean that the discovered-methods would be generalizable (to, for example, ethnmethodological or social science inquiries on topics other than the specific natural science inquiries), though it would result in an unprecedented demonstrability of the organizing features of natural science work. The practical effects of such demonstrability on methods used in the social sciences are difficult to imagine at this point.
be the case that the practicable “news” of such studies would be most clearly received by natural scientists. Robillard’s successes at designing a curriculum for medical school personnel at Michigan State University on the basis of ethnomethodological researches suggests as much (cited in Garfinkel, Lecture, York University, Toronto, February 1979).

14 The term, “in-itself,” as used above, is not the expression of a simple realism, connoting that the endogenous features of science are not available as a worldly “thing”. The in-itself, as used here, is a relationally constituted phenomenon, and as such is inseparable from the “approach” of this study (“approach” is taken concretely here rather than theoretically; it means the practical sequences of actions taken in pursuit of a phenomenon, such as, going to a laboratory facility, talking with persons working there, hearing their talk to one another, hearing that their talk is baffling, etc.). The in-itself is a constituted-independence, where, in the manner in which it is experienced, it is exhibited as other than that which attempts to “control” [it], construct [it], or otherwise provide for [it] as a docile feature of “subjectivity”. (The brackets are used to denote the provisional, and possibly non-existent status of the [it].)

15 It does not do these “uncertainties” justice by concluding that they are essential “uncertainties” and ultimately unresolvable, since to conclude this is to turn away from the ongoing inquiry which produced the material contents of the “uncertainties” and specified those “uncertainties” on the basis of a continuing attempt to address them. To say that they are ultimately insoluble is to be much too certain about them.

16 Ethnomethodological studies of work are distinguished by the way in which they topologize embodied practices with “paper,” “pages,” “blackboards,” and “typewriter symbols” as of fundamental importance in studies of “intellectual” activities. Such studies are best exemplified in written works by Garfinkel and Burns on lecturing work (1979), O’Neill (1981) and Morrison (1979) written inquiry in science, Livingston on mathematician’s work (1976, 1983), Pack on conversational transcription (1975), and Weinstein on “logging” in the truck-driving industry (1975).

17 To my knowledge, ongoing researches by Morrison (1979) and O’Neill (1981) are alone among studies of the textual organization of scientific writing in their orientation to the disciplinary specific practices of writing in the natural sciences. This distinguishes their approach from studies which adopt analytic schemata from linguistics or literary criticism as theoretical resources for revealing rhetorical features of scientific texts. Morrison and O’Neill eschew such analytic resources in an attempt to discover how different disciplines of writing identify themselves as distinctive inquiries in the organizing details of reading-and-writing. It should be noted that the orientation in the present study to the extra-literary organizations of scientific practices does not preclude the analysis of written scientific texts, as long as the analysis does not presume to be appropriating “data” about how science is practiced in day-to-day shopwork. An adequate analysis of scientific texts, from the point of view of this study, would need to account for the pragmatics of reading and writing through which research reports and methods recipes are subsumed within courses of action in actual research. Any treatment of such writings in either an ironic or naturalistic reading of them as descriptive accounts can only fail to distinguish their integrity as scientific documents.


19 Garfinkel (lecture, Department of Sociology, UCLA, 1980–2) cites Livingston’s (1983) research on mathematicians’ work as an inquiry based upon Livingston’s extensive study of graduate level mathematics during his graduate career in sociology. The study of mathematics was taken-on as an explicit part of Livingston’s graduate work in sociology, and not as part of a “double career”. The demands of such a program are undoubtedly imposing, though the inquiry which results has the potential of vastly surpassing
CONCLUSION

anything previously done on the sociology of mathematics and the sociology of science, if only because of the intimate way in which the approach to the phenomenon is deeply embedded within the phenomenon's unique course of pedagogy.

Appendix The transcript symbols

(Adapted from unpublished handout by Gail Jefferson)

I Sequencing
(The transcription of sequential features is done with great care.)

The double obliques indicate the point at which a current speaker's talk is overlapped by the talk of another.

A multiple-overlapped utterance is followed, in serial order, by the talk which overlaps it. Thus, C's "Victuh," occurs simultaneously with V's "left," and her "Victuh" occurs simultaneously with his "hallway."

An alternate system is to place a single bracket at the point of overlap, and place the overlapping talk directly beneath the talk it overlaps.

Double brackets placed in front of two serially transcribed utterances indicate that they start simultaneously.

A single right-hand bracket indicates the point at which two overlapping or
II Sound-production

(Matters of sound production are neither conscientiously nor consistently attended to in this transcription system.)

??! V: Becuss the soopuh dint pudda bu:lb on dih sekkin flaw en its burnt ou:t?

V: A dog? enna cat is different.

R: Wuh jeh do:

:: V: So dih guy sez hh

M: Yeh it's all in the chair all th/at junk is in the chair. =

simultaneously started utterances end, if they end simultaneously, or the point at which one of them ends in the course of another, or the point at which one utterance component ends vis-à-vis another.

In general, the equal signs indicate "latching": i.e. no interval between the end of a prior and start of a next piece of talk. It is used for the relationship of a next speaker's talk to a prior speaker's, for the relationship of two parts of a same speaker's talk, and as a transcript convenience for managing long utterances which are overlapped at various points, in which case a through-produced utterance may be more or less arbitrarily broken up.

Punctuation markers are not used as grammatical symbols, but for intonation. Thus, a question may be constructed with "comma" or "period" intonation, and "question-intonation" may occur in association with objects which are not questions.

An equal sign at the end of one speaker's utterance, followed by the combined equal sign and double brackets indicates that the bracketed speakers have started simultaneously, and with no interval between that talk and the preceding talk. This may occur for a speaker followed by two others, or for one "continuing" speaker and one other.

An alternate system, is to place double obliques in the course of what is treated as a single ongoing utterance by a first speaker.
APPENDIX

|= v: y's, well i woulda picked it up.
m: [i mean no no n'no.]
v: [p'tit backup,]
m: [i doesn' make any-] = "latched" onto by a next. in this case, the two prior are latched onto by two simultaneously-starting nexts.
v: ['so dih gu:y] says hh

(0.0) v: ... dih soopuh ul clean it up,
(0.3)
():

v: no kidding.
m: yeh, there's nothin there?
(0.5)
m: quit hassling.
v: she's with somebody y'know hh ennuh,
(0.7) she says wo:w ...

- v: i'm intuh my thing, intuh my: - attitude against othuh pih-hh

'hh v: so i sez, 'hh wa:l whuddiyou goin do

* m: jim wasn' home, // (when y'wen over there)

uc v: en it dint fall OUT!

* v: bu(h)D i'M NO(h)T I(h)NTUH THA(h)*TI* *

a right-hand bracket plus equal sign indicates that two utterances have ended simultaneously and will be "latched" onto by a next. in this case, the two prior are latched onto by two simultaneously-starting nexts.

numbers in parentheses indicate elapsed time in tenths of seconds. the device is used between utterances of adjacent speakers, between two separable parts of a single speaker's talk, and between parts of a single speaker's internally organized utterance.

the dash, rarely used in this transcript, indicates an untimed pause, e.g. a "beat."

the h indicates audible breathing. a dot placed before it indicates an in-breath; no dot indicates out-breath.

the degree sign indicates that the talk it precedes is low in volume.

upper case indicates increased volume.

asterisks indicate non-speech sounds, e.g. thumping fist on table. they may be produced by speaker or another.

III Reader's Guides

( ) m: i'd a' cracked up 'f single parentheses indicate duh friggin (gla-i(h)if transcribers are not sure about y'kno(h)w it) sm(h) the words contained therein.
a(h) heh heh pairs of parentheses, as in the third instance, offer not merely two possible hearings, but (right witchu.) address the equivocality of each. empty parentheses indicate that no "hearing" was achieved. on occasion, nonsense syllables are provided, in an attempt to capture something of the produced sounds.

( ) v: i'll be (back inna minnit.)

():

r: (y'cattuh moo?) the speaker designation column is treated similarly; single parentheses indicating doubt about speaker, pairs indicating equivocal possibilities, and emptyis indicating no achieved identification of speaker.

(()) m: ((whispered)) (now they're gonna, hack it.) materials in double parentheses or double brackets indicate features of the audio materials other than actual verbalization, or verbalizations which are not transcribed. occasionally an attempt is made to transcribe a cough (which might appear as "eh-khoolk!"), or a raspberry (which might appear as "pthrrrrp!")

m: ((cough))

m: ((raspberry))

v: ((dumb slob voice)) "eh-khoolk!" or a raspberry well we use-thuh do dis, (which might appear as "pthrrrrp!")

j: they're fulla sh:it.


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