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THE PROCESS OF KNOWLEDGE CONSTRUCTION:

A Triple Parallel Wrighting of Science, Sociology of Scientific Knowledge and a Candidate PhD Thesis

By

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A Doctoral Thesis

Submitted in partial fulfilment of the requirements for the award of Doctor of Philosophy of Loughborough University

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ABSTRACT

This thesis is an inquiry into the 'process of knowledge construction' in three research domains. These domains are the research into endothelin (a potent vasoconstrictive peptide identified in 1988) in bio-medical science, Sociology of Scientific Knowledge (henceforce, SSK) and the SSK-oriented research of the 'process of knowledge construction' pursued by the Candidate PhD student. The thesis is pursued with a recognition of the self-referential character of itself, that is, reflexivity. Namely, the Candidate's research, including the writing/reading of this thesis, is itself a process of knowledge construction. This recognition is displayed in and through a series of experimental textual forms, 'New Literary Forms (henceforce, NLFs) developed in SSK. Through NLFs, the thesis aims to make itself a wrighting, which connotes "'writing', 'righting' (correcting), and 'wright-ing' (making and working)" (Ashmore 1985, 1989), of the triple parallel process of knowledge construction.

In the first part of the thesis, the 'constructed status' of knowledge is examined by taking up the construction of 'nature' and '(scientific) discoveries' in the three domains. This examination also tries to explicate how sociological analyses involve knowing the research object under relativism, and how an analyst can pursue her research both as an insider member of society and as an outsider sociologist.

The focus of the last part of the thesis is on 'process'. I will examine how it is that knowledge construction in endothelin research is 'in process', how 'being in process' can be studied in SSK, and how such studies can be, self-referentially, in a process of knowledge construction. Then, the text displays itself as being in a process of knowledge construction: the text is displayed as open to being analysed-back by the participants (in science and social science) and itself to be a performance of 're-analysing-back' which asks and waits for further 'analysing-backs', including your reading/writing of this abstract and the rest.
Acknowledgements

First and foremost, I would like to thank Dr. Malcolm Ashmore, my supervisor. Without his continuous support and encouragement my thesis would not have been born to life, and my research life would have lacked something essential. His inspiring supervision has made it possible for me to make my contribution.

For more than ten years, I searched for a place where I would be inspired to express my ideas in a creative way. Starting from the field of Library and Information Science, I tried to find such a place in communication studies, cognitive science, semantics, pragmatics, discourse analysis (albeit more linguistically-driven than in the Loughborough Discourse and Rhetoric Group - DARG). At the beginning of the 1990s, I finally encountered SSK and reflexivity, and Dr. Malcolm Ashmore's The Reflexive Thesis (1989) has thrown a fresh light on my pilgrimage. Then, in 1994, 'a miracle happened'. I wrote a letter to him, and Dr. Ashmore accepted me as his PhD student at Loughborough, which he suggested as one of the best places for me to develop my research interest. This decision totally changed my research life. I am now very happy to have completed my thesis under Malcolm's direction and at least I have been enabled to say what I needed to say with my own voice.

In Loughborough, I have met many brilliant scholars. Amongst others, Prof. Jonathan Potter and Prof. Derek Edwards have inspired me with their frontier research framework and sharp-cut discourse analysis, which I have gradually grasped during our weekly DARG sessions and the training course in qualitative methods. Dr. Katie MacMillan, Dr. Mary Horton-Salway and Dr. Kevin McKenzie have become my best colleagues with whom I have developed my PhD research and creative friendship. I am very grateful for all of them.

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Likewise, not only being supportive colleagues, mentors and friends, but also providing useful feedback, many social and human scientists other than those mentioned above have contributed to the development of my research. I would like to thank Prof. Shunsaku Tamura, Prof. Dick Pels, Prof.
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CHAPTER ONE

Introducing a Triple Parallel Process of Knowledge Construction

Each of these individually meaningful lines fuses with the others in a highly nonrandom way, to make a graceful totality. The art of writing a beautiful fugue lies precisely in this ability, to manufacture several different lines, each one of which gives the illusion of having been written for its own beauty, and yet which when taken together form a whole, which does not feel forced in any way. (Hofstadter 1979: 283)

Reflexivity is not a problem for a sociology of science but a constituent problem of it. (Ashmore 1989: 55)

Reflexions on the Naming of a Thesis

Hello, the author.

Hello, the text. What will you be about?

I am about the process of knowledge construction.

Okay. How will we pursue it?

In and through the work of constructing our knowledge of the process of knowledge construction.

Well, that sounds as if the process of knowledge construction is both our research topic and also the site where we are working.

Well, what's wrong with pursuing it as a research object whilst engaging in it?

1 The idea of 'Reflexion' is provided in the book entitled Knowledge and Reflexivity (Woolgar 1988). In this book, the text of Reflexion tries "to ensure the Editor has the last word" (ibid: vii). Instead, the Reflexion of this thesis tries to display that the Editor has the very first word. But who is the Editor?

A meta-Voice (the Editor) is established in order to reassert the multivocality of the text. But, . . . the assignation of discrete/distinct identities to voices/characters is to be discouraged, because it tends to play down the interpretive work necessary to concretize positions and views, to crystallize them as the production/possession of separate selves. Thus, the meta-Voice either undermines its own assertion or it becomes indistinguishable from the character/voices about which it wishes to make an observation. (Woolgar and Ashmore 1988: 12)

The above Reflexion of this thesis is a display of the interpretive flexibility involved in the author, the reader, the text and its textual conventions. See also the section on 'Introducing Multiple Subjectivity' (p. 52-55). I would like to thank Mary Horton-Salway for her suggestion to name this part of the text 'Reflexion'.

2 This is said because the text hopes to be categorised as SSK, and as social constructionist, relativist and reflexivist.
It sounds... somehow hollow as if we are only engaged in a dialogue with ourselves. And, it may be inappropriate, if not mistaken, as you (the text) will become the result of complex, if not confused, levels of what you represent in terms of content and form, topic and resource, et cetera.

Well, I am not yet convinced that breaking those distinctions is really a serious problem. So, why don't we just name me, the text, 'the process of knowledge construction'?

You are telling me to "relax and stop worrying". But I am still in... sheer perplexity! You are saying that we will study the process of knowledge construction. But surely this can't be the case if we use this strange textual form... it sounds convoluted... and somehow we are...

Oh, don't worry! Let's go on by naming me 'the process of knowledge construction'. You will see how it develops, as we proceed. By the way, what is your name?

About This Thesis

This thesis both studies and constitutes the process of knowledge construction. The process of knowledge construction proceeds in three research domains: science, sociology of scientific knowledge (hereafter SSK) and the very research that a candidate SSKer (hereafter, the Candidate) is pursuing for this PhD thesis. The study simultaneously aims to constitute a process of knowledge construction: it inquires into how knowledge of nature is constructed in process, in various settings including those wherein the interaction between scientists and the Candidate takes place.

3 'The Relax and Stop Worrying Solution' is one of the management strategies for avoiding the reflexivity of reflexivity which Malcolm Ashmore has categorised by taking up a variety of positions taken in ethnomethodology. This position seems to claim that:

[O]ur knowledge and our epistemology have no absolute grounds and do not suffer for their lack. We can then relax and get on with the job without concerning ourselves with insoluble methodological/epistemological/ontological conundrums, secure in the knowledge of a natural limit to our inquiries. (Ashmore 1989: 97)

Ashmore is critical of this solution as it avoids the paradoxical problem of the reflexivity of reflexivity by default.

4 You will see some candidate names in the last section of this chapter (p. 52-55).
The research domain in science, which I will examine, is the one which is currently called 'endothelin research'. Endothelin is a potent vasoconstrictive peptide identified in 1988 (Yanagisawa et. al. 1988). I have chosen endothelin research as an SSK study topic for very practical reasons. I had the opportunity to meet one of the scientists working in The Institute of Clinical Medicine, Tsukuba University, which is situated close to the university where I work, University of Library and Information Science. I asked this scientist whether he could suggest some candidate research area in science which has been initiated with some kind of 'scientific discovery' and was still currently 'hot' in its development. I added that I prefer 'basic' science to 'applied'. The scientist informed me of two research areas in Tsukuba University wherein a big discovery had been achieved. These human networks and local connections\(^5\) that I enabled me to get an introduction to the core members of endothelin research and begin my science case study. In this research, I will try to explicate how knowledge of endothelin is constructed as a process, based on talks and writings collected from interviews, seminars, sessions, conferences, the formal literature, correspondence, field notes, and so on (see Appendix I).

The study is grounded in, and aims to orient itself to SSK, which is a field understood to be currently and ongoingly developed by SSKers who intersubjectively or discursively orient to their knowledge in process. The Candidate has encountered SSK after her five years quest in the research domain where her research interest can be developed, tried to learn the basic knowledge acquired in SSK, and she is now trying to follow its latest and hottest issues. Besides, the Candidate is pursuing her own research. It has been developing for more than ten years (and is still in process). She believes that it will be offered as a contribution to SSK knowledge in the near future.

In sum, this thesis has chose to study the process of knowledge construction in endothelin research, on the basis of SSK which is also in the process of knowledge construction, and it tries its own pursuit to be a process of knowledge construction. In\(^5\) Of course, my research into endothelin research does not mean that I am investigating 'a single case' from a potentially enormous number of science cases. I think that one's choice of any research domain significantly effects how the triple parallel process of knowledge construction proceeds together.

\(^5\) Of course, my research into endothelin research does not mean that I am investigating 'a single case' from a potentially enormous number of science cases. I think that one's choice of any research domain significantly effects how the triple parallel process of knowledge construction proceeds together.
In this sense, the thesis deals with a triple process of knowledge construction. In addition, the emphasis of this thesis is that it does not merely acknowledge the research domain in which it is based (and itself) as processes of knowledge construction. It will simultaneously pursue the processes of knowledge construction in SSK and itself under the rubric of reflexivity (see below, 'Approaching Reflexivity', p. 5-13). Namely, the pursuit of the 'process of knowledge construction' in endothelin research is carried out in tandem with a simultaneous explication of the processes of knowledge construction in SSK and itself: the latter explication will further lead us to acknowledge that it is also proceeding here in process, including the process of your/my reading/writing. For this purpose, the text of this thesis uses some new textual forms and is thereby constituted as being about and a participation in the process of knowledge construction which will be both displayed and performed. To me, such a working-up of the text is called 'wrighting'.

Since I am making the assumption that knowledge in each of the three research domains is constructed in parallel, I prefer not to draw clear borderlines between the materials collected in them, thus categorising them into 'data', 'background' and the Authorised interpretation of this thesis. I will instead treat each of the talks and writings as textual 'bits' of the thesis. This ambition will result in my analysis as a paradoxical pursuit. I will make efforts to analyse the knowledge construction pursued by the participants (including the Candidate), and at the same time, my doing analysis will be taken up as an issue for analysis. I will use, as 'data', transcriptions of talk and tables of listed literature together with its 'summarised content' and 'function'. At the same time, I will explicate what counts as 'data' and how it is used for knowledge construction, including the knowledge construction involved in this explication. Thus the methodology I employ for this thesis is self-referentially applied to itself. Consequently, the thesis takes a reflexive position.

4 'Wrighting' is a neologism connoting 'writing', 'righting' (correcting), and 'wright-ing' (making and working) (Ashmore 1985).
Approaching Reflexivity

The 'reflexivity' developed in SSK (e.g. Mulkay 1985; Woolgar 1988a; Ashmore 1989; MacMillan 1996) is not the main research object for this thesis. I am trying to ascribe the main (and inconclusive) research object here as the 'process of knowledge construction'.

Reflexivity is, I believe, always relevant; and not only when someone makes it a topic. It is always implicated in any kinds of reading/writing. Reflexivity is "a constituent problem of [a sociology]" (Ashmore 1989: 55), and inevitable in any research. Thus, I will deal with the problems, inquiries, findings, proposals, and so on, regarding the process of knowledge construction, with the conviction that any research is to be written/read with reflexivity.

The reflexivity of this thesis is roughly categorised into two kinds. First of all, throughout your and my reading/writing of this thesis, there takes place the ethnomethodologically-informed reflexivity or R-circularity (Ashmore 1989: 32). That is, the thesis is made sense of by the very act of writing/reading it, and as far as you and I make sense of the thesis, both of us are reading/writing it. These are like two sides of a coin. I will call this kind of reflexivity 'reflexive-constitutedness'.

But my particular concern in this thesis is with the second kind of reflexivity: that is, reflexivity which is constructed through self-reference. Namely, the analytical point made in an analysis can be reflected back to itself, by asking "how would what..."
you have just said, be applied to your own formulations?" If this question is only
directed at others, it is likely to be a "tu quoque" (Ashmore 1989: Chapter 3). In
particular, when the tu quoque is used by realists to attack a relativist claim, it has been
taken to be an uninteresting, banal, and irrelevant counter-attack (Edwards, Ashmore
and Potter 1995). My goal in this thesis is to 'wright' "beyond the tu quoque" (Ashmore

To this end, I will try to construct the thesis as a 'celebratory practical reflexive
inquiry'. This has been proposed by Ashmore for dissolving certain problematic
formulations of reflexivity that evoke the potential escalation of meta-level analyses
and drives the analyst into a fear of the 'infinite regress'. Ashmore's prescription is:

It is not a matter of authorial presentation.
It is not a matter of being correct.
It is not a matter of meta-analysis.
It is not a matter of solving a problem.

(ibid.: 99)

Under the above injunctions, this thesis tries to "let speak" (Knorr-Cetina 1981: 18) all
the different voices raised in the writing. That is, a variety of voices explicitly and
implicitly raised in the process of this PhD research are tried to be openly embraced and
(at least potentially) revealed in this thesis, while I fully acknowledge the author's role
that, willingly or not, has control over them and is responsible for all the representations
of their voices. As reflexivity will be taken up as a constituent problem of my inquiry,
the text of this thesis is not orienting to demonstrate its correctness when it faces the
paradoxical features raised in its inquiries. Rather, it tries to reveal the paradoxes
which inherently emerges in one's engagement with SSK research and in one's
textuality when one makes knowledge-claims. The text of the thesis tries its best not to
take the stance of 'Observer-privilege' nor (meta-)analysis which presumes that its
object is subordinate or lower-level than itself. The stance of this text is one which tries
to distance itself from any kind of 'Positionism' (Woolgar 1992). Instead of being afflicted by insoluble problems and falling into an infinite regress, and instead of trying to 'solve those problems', the research object of this thesis will be constructed as thoroughly implicated in such problems, and the instability of its text will be displayed throughout the thesis.

In order to accomplish this, I will construct the text of this thesis in a way that exemplifies its own knowledge-claims. Writing both about and as the process of knowledge construction is to make a kind of simultaneous textual engagement. This effort implicates some contentious issues. On more than a few occasions, the writing will have amusing, if not confusing, textual collapses which could be taken by some readers, to be a jumbling-up of textual (or research) levels. For example, the analyst discusses how her knowledge-claims about (scientific) discovery can themselves become a (sociological) discovery while writing her research on the discovery (see Chapter 3). Those occasions are analogous to Douglas Hofstadter's 'Strange Loop, or Tangled Hierarchies' and 'the Inviolate Level' (Hofstadter 1979). Hofstadter refers to some activities which are concerned with "the consequence of the mixing of subject and objects" in science (ibid.: 699) and contrasts them with 'pure' science:

Science is often criticized as being too "Western" or "dualistic" -- that is, being permeated by the dichotomy between subject and object, or observer and observed. While it is true that up until this century, science was exclusively

11 The stance of non-Positionism and its criticism will be discussed in Chapter 3 (footnote 145) and Chapter 5, 5-4, p. 276-289.
12 One of those occasions is also illustrated in Ashmore's study of the self-referential character of SSK studies, including his own:

At this point, and perhaps not for the first time, the necessary and fascinating confusion of levels involved in [Ashmore's] account provides for what Hofstadter (1979) calls a "strange loop," such that at this point, and perhaps not for the first time, the writing turns back on itself and begins to repeat, coda-like, but perhaps in a different key (on a different level), all that has gone before. Another one of Hofstadter's examples of logicomathematical reflexivity is the "nested structure." These are structures which contain themselves, or versions of themselves, as a constituent part of what they are... This aspect of the text is not simply a matter of its style or form alone. Such stylistic or formal possibilities seem to be predicated on a prior matter of method. Maybe it is the move beyond simple meta-analysis and toward a form of reflexivity that is permanently "part of the very order which it describes" (Brannigan 1981) that a creative and liberating (and above all, deliberate) confusion of levels can provide for such interesting formal textual features. (Ashmore 1989: 158-165)
concerned with things which can be readily distinguished from their human observers . . . this phase of science was a necessary prelude to the more modern phase, in which life itself has come under investigation. (ibid.: 698-699)

'A Strange Loop, or Tangled Hierarchies' is the scheme Hofstadter himself takes to illustrate this ground of mixing. One sample of this scheme is 'the Authorship Triangle'.

There are three authors -- Z, T, and E. Now it happens that Z exists only in a novel by T. Likewise, T exists only in a novel by E. And strangely, E, too, exists only in a novel -- by Z, of course. Now, is such an "authorship triangle" really possible? (ibid.: 688)

This situation is illustrated in Fig. 1-1 (p. 9). Hofstadter's answer to the above question is "of course, it's possible". But it is, in a significant sense, possible only on condition that there is another level: 'the Inviolate Level'.

[T]here's a trick . . . All three authors Z, T, E, are themselves characters in another novel -- by H! You can think of the Z-T-E triangle as a Strange Loop, or Tangled Hierarchy; but author H is outside of the space in which that tangle takes place -- author H is in an inviolate space. Although Z, T, and E all have access -- direct or indirect -- to each other, and can do dastardly things to each other in their various novels, none of them can touch H's life! They can't even imagine him -- no more than you can imagine the author of the book you're a character in. If I were to draw author H, I would represent him somewhere off the page. Of course that would present a problem, since drawing a thing necessarily puts it onto the page . . . Anyway, H is really outside of the world of Z, T, and E, and should be represented as being so. (ibid.: 688-689)

The point in the above example is that it is true that there exists such 'Strange Loop, or Tangled Hierarchies' as we perceive: but "there is always some 'protected' level which is inviolate by the rules on other levels, no matter how tangled their interaction may be among themselves" (ibid.: 688): the strangeness and tangle appear as such on 'the
Fig. 1-1 Hofstadter's 'authorship triangle'
Inviolate Level' (*ibid.*: 689)\(^{13}\) and it is a construction produced on this level. Only with "the Inviolate Level", is this scheme of 'A Strange Loop, or Tangled Hierarchies' intelligible.

In sociological writing, such a jumbling-up that includes itself (i.e., textual form, resource, etc.) in what it is about (i.e., research object, topic, etc.) might be treated as problematic. For instance, Paul Filmer states that sociologists are expected to concern themselves with "what is decidable 'for practical purposes' ", and not to be "interested in" what their task consists of as a practical action (Garfinkel 1967: 7-9). Thus, to engage in the essential reflexivity of sociologists' accounts is:

not doing the practical sociological inquiries constitutive of sociology (-which-is-not-reflexive). That is to say, it would not be doing sociology as it is traditionally understood, precisely because to be doing reflexive sociology is to be rendering problematical, and thus the central topic of inquiry, that very tradition in whose (unexplicated) terms sociology is understood as what it is. The sense, then, in which the essential reflexivity of sociologists' accounts is uninteresting to their formulators lies in this: that sociologists trying to be reflexive are sociologists doing work which is not understandable as sociology; whose sense is not grounded in sociology’s tradition of (scientific) discourse. (Filmer 1975: 155)

Since I am attempting to write both *about* and *as* a process of knowledge construction, my efforts may be regarded as problematic in terms of the traditional sociology that Filmer describes. That is, I am faced with the "problematic paradox" (Filmer 1975: 155) of doing sociology in and by including this 'doing sociology' in this doing sociology. But the recognition of the paradox does not convince me to give up the attempt. As in 'A Strange Loop, or Tangled Hierarchies', the paradox is itself the object which we observe from 'the Inviolate Level': That is, it is also locally constructed as such. More importantly, to attach a negative value onto a certain constructive work by charging it to be paradoxical is itself a contingent practice. It is thus always possible to

\(^{13}\) The location of such an 'Inviolate Level' for this thesis will be suggested in Chapter 3 (p. 165-167).
ask how a certain paradox is observable on that particular level and by whom that paradoxical construction is made to seem negative. Instead of offering the paradox as a reason why I should maintain an indifference towards my own doing of sociology, I take it as an opportunity. It can be transformed into an opportunity for developing a study of both its topic and its own performance, and providing a 'next' turn to explicate the issue from another angle. According to Katie MacMillan, a reflexive performance or display is 'having-one's-cake-and-eating-it' (MacMillan 1996)\(^4\).

Another approach I will employ is to write with a variety of textual forms. There are some forms of reading/writing which have the effect of celebrating and highlighting the textual status of one's own writing. Those called 'reflexivists' in SSK have already experimented with a variety of textual devices, 'New Literary Forms' (hereafter, 'NLFs')\(^5\) for accomplishing such discourse (e.g., Woolgar 1988a, see also

\[\text{therapeutic journey leads us away from the criticism that radical reflexivity has the inevitable danger of slipping into an infinite regress... The spiral... is not only the shape of the reflexive tool which enables more turns at textual deconstruction, it is also the therapy, like hypnotic trance, which enables me, as the hypnotist, to demonstrate that the monster (the awful infinite regress) exists only within our imagination. (MacMillan 1996: 24-5)}\]

Each of the turns engendered with this reflexivity offers:

a shift in focus. Topics may be endlessly rehearsed, but with each rehearsal a different perspective is gained, as the text moves, not back to the beginning, but to the next turn upon the spiral of reflexivity. (ibid.: 26)

MacMillan's study takes the next turn at various stages of her writing. This gives her ways to deconstruct textual methods of knowledge construction and to display this deconstructive and constructive work of her own. Her display invokes that there is always potentially another turn which can be taken in the analysis. Rather than taking this turn as the monster of infinite regress:

there is a healthier textual position, in which the confidence of the writer can lead us into the realms of the unknown, creating a map of the journey there and beyond. (ibid.: 31)

I agree with MacMillan that the regress exists only within textual imagination. A reflexive approach can thus make a therapeutic intervention at each point when the regress is invoked, alerting us "that it is brought into being as a rhetorical device, summoned into the argument, and then banished" (ibid.: 35). A science study with NLFs was initiated in order "to devise ways of accepting, learning from, even enjoying, reflexivity" (Mulkay 1985). This textual form tries to highlight the knowledge construction of its own and others: it tries to:

deconstruct the assumptions implicit within standard texts that there is a clear distinction between fact and fiction... self-consciously display the presence of the analyst/author within the text, and as such demonstrate the way that a writer's claims are shaped by the use of specific textual forms, while retaining an essential subjectivity... thus suggesting that knowledge is a process, implicit within the kind of writing used in standard texts --: imply a multiplicity of
MacMillan 1996). This thesis takes up not only the analysis of the constructed scientific knowledge of nature and its progress, but also how the analyst herself comes to know it and develop her knowledge in process. One of the crucial tasks is to reveal the ground (or the extent to which she takes the realist's ground) of her analytical practice. I will attend to whether and how far my inquiry dissolves the problem of inquiry itself, as it may start becoming "a realist practice of realist writing, even if such a practice is presented as a mere heuristic; and perhaps especially if it is so presented" (Ashmore 1989: 110).

One more aim of this thesis is to ask you to see the reading/writing of this thesis as the performance of a dialogue (Morson and Emerson 1990; Holquist 1990; Morris ed. 1994). If that is possible, the text can open itself to 'the next reflexive turn' (Ashmore 1989; MacMillan 1996). My belief that I am writing both about and as the process of knowledge construction is, of course, only potentially possible. This inquiry into the process of knowledge construction requires to be recognised as such in one's reading/writing. Indeed, your reading/writing is a key to this success here. With these promises, the reflexivity in this thesis is accomplished in and through the reading/writing of it. Thus, let's start writing / reading.

Let me try to negotiate with you more explicitly for "how we would like to read/write this thesis". I am almost certain that you are now reading my thesis, while recently or a long time ago, somewhere far away or right next to you, I am writing my thesis. Would you also try to make clear what you are doing now? Thanks. Right now, you are perhaps in situ constituting what you are reading or what I am writing, that is, this thesis.

Let's start writing / reading that way. All right?

meanings or viewpoints through the use of different textual 'voices': and therefore declare that multiple readings are available within a single text, inviting the reader to step into the text, and partake in the deconstruction of the authority of the author: which allows a further reading, a reflexive spiralling over the last move. (MacMillan 1996)

For further reference to the NLFs, see Ashmore (1989: 66-67).

16 The above concern renders my task of reading/writing this introductory chapter also to be twofold: one is to provide an introduction to what will follow, and the other is to set a reflexive precedent for the writing/reading of what will follow.
I hope that you will raise a lot of issues regarding my writing, while you are aware of your own reading/writing. I hope you will experience a kind of tension between the end of the string you are pulling and the end I am holding here. In the following chapters, I hope that my writing about the process of knowledge construction can be read as itself being in the process of knowledge construction which is in situ taking place and in which you and I are both participating. But now, I will write a review of how the process of knowledge construction has been studied in the relevant research domains, and through this process I will of course be constructing the relevant knowledge as such.

**How the Process of Knowledge Construction Has Been Figured Out**

[The Candidate's Background]

A. Transfer in a Conduit

First of all, let me introduce an approach for studying the process of knowledge construction, with which the Candidate has been most familiar. She originally came from the research domain called 'Library and Information Science'. In this domain, she knows that 'mental models' or 'cognitive models' (e.g. Johnson-Laird, 1983; Aitkenhead and Slack, 1985) have been dominant, particularly in explaining 'how a sender transfers information to a recipient' and 'how scientific information is accumulated and generalised into a body of knowledge'. Here, what may be termed a 'conduit metaphor' (Reddy 1979), 'algorithmical model' (Collins 1975) or 'diffusion model' (Latour 1987) is used. 'Information content' is assumed to be both transported from a sender's mind to a recipient's mind (in such a way that this 'content' runs through a conduit while staying the same); and also transformed into a better-validated, stable and shared content, i.e., a body of knowledge (and thus the content becomes different) (Mushakoji 1994).

To engage with such a model is to understand how we conceptualise our communication and our basis for recognising 'what we know'. Nobody denies its status as a metaphor. It is only when such a model is formulated as the Real World or as Real Human Actions, leaving aside "the messy, indeterminate nature of reality and language,
in pursuit of the rules of thought", that it is inadequate (Edwards 1997: 1-26). This model was the very first target she chose to attack ('Why not?' -- she thought) with her naive hand of SSK. Ironically, her article (Mushakoji 1995), part of which will be presented in this thesis (Chapter 5), reveals how deeply this model convinces her!

[Sociology of Scientific Knowledge]

B. Transition in a Social Process

An approach which looks similar has been taken in early SSK studies which focus on the social processes wherein knowledge-claims develop to become shared knowledge. For example, in his preliminary attempt to explore "the process whereby a scientist's research findings are transformed into accredited factual knowledge", G. Nigel Gilbert examines "the procedures actually used by natural scientists to decide on the validity of claims to scientific knowledge" (Gilbert 1976). The process entails three stages: (1) the construction of knowledge-claims, (2) the evaluation of the knowledge-claims as knowledge, and (3) the evaluation by a research network. The transition of findings is explained as taking place with an individual researcher's 'model', which is defined as "an implicit metaphorical description of how some part of the world is thought to be arranged" (ibid.: 282).

What counts as the process of knowledge construction is explained in this scheme as follows. First of all:

A researcher's model provides an initial formulation of the research problem, indicates the theories and techniques which may be applied appropriately to the problem, and eventually plays a crucial role in giving meaning to reports of the completed research. (Gilbert 1976: 302)

Then, the researcher reaches the stage of constructing knowledge-claims which are then evaluated by the readers. The evaluation is pursued by extracting from the reports
those findings which are compatible with the readers' own models\textsuperscript{17,18}. The evaluation stage is seen in the readers' citing of the initial knowledge-claims which are used for justifying the reader's own findings. The process will then reach the stage of evaluation by a research network, when certain findings start to be repeatedly cited. Gilbert illustrates this final stage as the one at which those findings "fit the majority of the models used by members of the network and can therefore become the basis on which research in the area relies for its justification" \textit{(ibid.: 302)}. He suggests that these findings are then temporarily adopted as scientific knowledge.

The process of knowledge construction in this approach looks similar to the one of 'Transfer in a Conduit', as both of them take knowledge construction to be transitive and more or less linearly and chronologically ordered. However, Gilbert's approach takes a more agnostic or relativistic view of what counts as knowledge. The statement of a research finding is called a 'knowledge-claim', and the temporality and locality of knowledge is emphasised. This I take to be the typical and symbolic tone of SSKers' voices.

C. Enculturation

Another model which is similar to the one above was introduced by Harry Collins in the mid-1970s (Collins 1975). It is called 'the enculturational model'. In his article about replication in science, Collins claims that "the meaning of an 'exact copy of the original' is itself problematical" \textit{(ibid.: 206)}. Rather than presupposing that experimental replication requires exact copying, it is more appropriate to understand it as:

the transmission of a \textit{culture} which legitimates and limits the parameters requiring control in the experimental situation, \textit{without necessarily formulating},

\textsuperscript{17}So, how is my current writing (not) compatible with your model regarding SSK? If you respond to it in your writing, I will be able to see how.

\textsuperscript{18}As a reader, Dr. MacMillan replies to my request in the above footnote:

\textit{But Sumiko, that's not fair, because the reader (a) would be then doing your bidding, (b) in your terms, and thus (c) supporting your SSK perspective or knowledge as constructed! (Sorry! couldn't resist it!!)} (MacMillan 1999, personal communication)
enumerating or understanding them, and which *ipso-facto* generates the set of anomalous experiments (failures which can't be explained by uncontrolled legitimate parameters). (Collins 1975: 207)

The transfer of knowledge is culturally bounded, and what counts as knowledge is culturally legitimated. Accordingly, procedures used in the process of transforming knowledge-claims into knowledge are *enculturated*.

This kind of explanation about the determination of states of knowledge has been, I believe, pervasively adopted in SSK (even if not always explicitly). For example, in what is referred to as the 'interest model explanation' (Barnes 1978, 1983; MacKenzie 1978; Barnes and MacKenzie 1979; see also Woolgar 1981; Barnes 1981; MacKenzie 1981), the knowledge of a competent member is modeled as 'a Hesse net' in which "the acquisition of all that the culture can provide still leaves future concept application underdetermined and open-ended" (Barnes 1983: 25). Given such a model, the adequacy (and inadequacy) of knowledge and the achievement of consensus are the outcomes of successful negotiations. If different cultures are found to have different 'nets', they stand equivalently in relation to 'reality' or to the physical environment (*ibid.*: 33). This is cultural relativism: all systems of culture in this model are equally rationally-held, and as institutions (*ibid.*: 35), restrict the range of inferences. It is changes in associated interests, or in the ability of some subsection of a community to further its interests at the expense of another, which explains the dynamics of institutions. The transfer of knowledge is feasible and thus pursued in a culture which shares and develops its interests.

Unlike the previous 'Transition in a Social Process' approach, this model does not draw a chronological and linear picture of the process of knowledge construction. For instance, Collins states that "the only criterion that the knowledge required to repeat an experiment 'properly', has been transferred, is that the experiment 'works' ", given the dynamic state of knowledge\(^\text{19}\) (Collins 1975: 219). That is, successfully

\(^{19}\) Then, this thesis needs to await its fate of becoming something until you start to speak of or write about it, and support its workings.
replicating experiments is essential in constructing knowledge, but success depends on
scientists' negotiation over the similarity of a repeated experiment to the original (ibid.: 210). I assume that this recognition has led Collins to develop the next approach.

D. Negotiating Procedure

This approach treats the transfer of knowledge as the result of negotiations about both the criteria of replication and the nature of the phenomenon under investigation (Collins 1981a, 1981b, 1985a; Collins and Pinch 1979). The focus has been more clearly shifted to a particular negotiating procedure by which knowledge is constructed as such. Scientists' actions can then be seen as negotiations about the criteria they use. At the same time, the reality and validity of the research objects are also negotiated.

This kind of negotiating procedure is neatly illustrated by Trevor Pinch. Pinch claims that in making knowledge-claims, scientists are faced with, on the one hand, the "externalization of observation", and on the other hand, the "evidential significance of observational reports" (Pinch 1985). In making an observation, a chain of interpretation is involved. For instance, to translate graphical information into a meaningful observation of solar neutrinos is a far from straightforward process. The observation can be merely externalized as "splodges", or further externalized as "Ar$^{37}$ atoms", or even further as "solar neutrinos". The relationship between the degree of externalization of the observational report and the evidential context defines the "evidential significance" of the observation.

Thus, observational reports may take on a different significance in different evidential contexts. It seems that the more externalized the observational report, the more the evidential context is specified and thus the report is likely to become profound. Alternatively, the less externalized the observational report, the less the evidential context is specified and thus the report is likely to become trivial. But the highly externalized report is more likely to be risky, when it is challenged, while the less externalized report is likely to remain unchallenged. In disputes about knowledge-claims, Pinch found that such challenges have an effect of changing the externality of
the observational reports. In reporting observations, experimenters are therefore faced with a dilemma: reports of high externality will stand a greater chance of making a contribution to the wider corpus of knowledge in view of their high evidential specificity, but such reports will also be risky: reports of low externality are less risky and more likely to gain acceptability, but they sacrifice profundity. Thus, the knowledge-claims made in their reports are the consequence of managing a trade-off between the risk of rejection and the risk of triviality.

Understanding science as the domain where this kind of negotiating procedure takes place may give the impression that scientists are shrewd and political. But that misses the point. Those who study the negotiating procedures take the view that in fact any kinds of actions are interactive negotiations\(^{20}\)\(^ {21}\). As any reports on their findings are the consequence of the "simplification processes in the scientific work place" (Star 1983), scientists (and sociologists) are inevitably involved in complex sets of problems, constraints and contingencies to be negotiated in order to construct knowledge.

E. Rhetorical & Textual Achievement

The negotiating procedure has also been studied by specifically focusing on rhetorical and textual devices which can be observed and analysed in a variety of written texts. The analyses are undertaken by taking up such genres of texts as scientific papers (Gusfield 1976; Yearley 1981; Law and Williams 1982; Bazerman 1981, 1988; Myers 1990), grant proposals (Myers 1990), exchanges between scientists (Myers 1990), popular science articles (Myers 1990), writings in economics (McCloskey 1985), and essays in sociology and literary criticism (Bazerman 1981, 1988). The genres and the ranges of academic disciplines investigated has been enormously wide (Dillon 1991).

\(^{20}\) And I am of course negotiating, with you, what we know about 'the process of knowledge construction'.

\(^{21}\) As a reader, Dr. MacMillan again (see footnote 18) responded with the following comment on the above footnote.

Not really though! You are telling the reader that you are negotiating!

(MacMillan 1999, personal communication)

Is there any ground for negotiation about whether a negotiation is taking place or not?
These studies examine, for instance, the textual setting, its action, agent, agency, purpose, voice and viewpoint, the relationship between audience and author, the relationship between author and subjects, metaphor, and social hierarchies (Gusfield 1976). In contrast to the above 'negotiation procedure' approach, the studies of rhetorical and textual devices in scientific texts mainly analyses how knowledge is constructed in authors' practices of writing. Even though most of them maintain that writing is a social action, social interactions are examined in terms of the rhetorical, functions and textual structures in the assumed setting in which the texts are written. Thus, the analyses take place in search of the textual author's achievement: what kinds of constraints the author needs to take account of, what kinds of textual devices are available, and so on. Apparently, this achievement is a social discursive action for constructing knowledge, but this knowledge tends to be analysed in terms of the authors' constructs. In other words, the question of how it is managed and maintained in the interaction between the authors and the readers (or the community) is likely to remain unspoken. The knowledge at issue appears as more or less that which acquired the constructed status, rather than that which is in construction. In addition, by mainly focusing on the published and thus already accepted reports, the focus is on successful cases of negotiation rather than cases of making knowledge-claims whose fates are currently at stake. This additionally gives an impression that the knowledge construction examined in this approach is that they have (at least temporally) a constructed nature, rather than being in a process of construction. In order to frame

22 Like Myers, there are some analysts who pursue a close examination of the negotiation between authors and readers and take not just one-text but a series of texts in order to show how certain knowledge-claims are processed (Myers 1990, 1991).

23 But do you really agree with my opinion here? After all, I am now trying to persuade you by employing similar rhetorical and textual devices in this writing. The author's writing/reading depends on the reader's reading/writing (See footnote 10).

24 However, these studies have provoked the presence of a potential interactive setting of another kind in early time. That is the issue which can again be categorised under the heading of 'reflexivity'. Analysts are asking themselves the following issues:

Am I not also utilizing the devices of Art in my analysis? Is not my critical stance toward the author misplaced since I too might similarly "put down" my own performance? This criticism invites me to provide an analysis and a rationale for the status of the scientific document as something purporting to be accepted as a "true" account. By invoking a description of Science
these studies as those of knowledge which is in construction, we may perhaps need to highlight the interactional setting in which the rhetoric is employed. The next section introduces such an attempt.

F. In Being Discursively Accomplished

One approach for studying the in situ process of knowledge construction has examined interactional discursive works between participants in an interactive setting. This has been originated under one of the SSK programmes, Discourse Analysis (Gilbert and Mulkay 1984; Potter and Wetherell 1987). In analysing a vast amount of interview data in order to study scientists' actions, Gilbert and Mulkay came to open what they term a 'Pandora's Box'. Participant scientists' accounts were found to be context-dependent and variable, and the world under their research was found not to be "composed of a series of discrete, one-dimensional actions which can be more or less accurately represented" (Gilbert and Mulkay 1984: 10). It is thus unsatisfactory for sociologists merely to reproduce the participants' activity of putting all the different accounts through a sieve to find the single correct one. Variability in participants' accounts is observed between different scientists, between different discourses produced by the same scientist, and in the course of a single discourse produced by a single scientist. These observations led Gilbert and Mulkay to propose the Discourse Analysis programme, in which a systematic investigation of participants' discourse is regarded as "methodologically prior to analyst's use of such discourse to characterise and explain social action" (ibid.: 8, stress in original). It has resulted in prolific studies as

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Yes, you are, and we are. The consequence is "an emergence of some disciplines in which the implications of the rhetorical turn are traced (Golinski 1993)". The implication for you is that you need to start analysing the rhetorical and textual devices in this text: specifically, how my critiques of these studies work? Would it successfully render my own approach as occupying the high-ground? It is a 'Pandora's Box' for sociologists. Inside this box, Gilbert and Mulkay found that scientists' accounts of scientific action is context-dependent and variable. Given its context-dependency and variability, how can the analyst use such accounts as descriptions of scientists' actions (Gilbert and Mulkay 1984: 6)? After opening this box, they began to develop an alternative form of analysis — that is, Discourse Analysis.
regarding how texts are organised to represent facts and reality as being 'out there'.

To treat participants' discourse as a topic instead of resource in this way is a major and radical shift.

Discourse Analysis raises some crucial points in studying the process of knowledge construction. First of all, Gilbert and Mulkay introduced two 'interpretative repertoires' that the scientists they interviewed use selectively. It was found that scientists depict action and belief in ways which are appropriate for the different interpretative contexts they are involved in reproducing. More specifically, when scientists write experimental research papers, they make their results meaningful by linking them to formal accounts which are couched in terms of an empiricist representation of scientific action. Gilbert and Mulkay term this mode of discourse an 'empiricist repertoire'. This repertoire is used alongside, a 'contingent repertoire' in which all 'personal' and 'social' factors such as a prejudiced view and excessive adherence to a certain theory are drawn on to explain error and to discredit findings. Such references to the 'social' context tend to be excluded from the realm of formal discourse in scientific report writing.

Secondly, Gilbert and Mulkay repeatedly observe the interpretative inconsistency in scientists' discourse where these incompatible repertoires appear together. When the two repertoires occur together, the potential incompatibility is often signaled by, as well as being resolved by, a specific interpretative pattern or device. This is called the 'Truth Will Out Device' (hereafter, 'TWOD'): the TWOD enables the scientist to re-establish an interpretative disjuncture between the two repertoires after they have been used together in one conversational passage. This TWOD has two

26 How we can observe and talk of facts and reality as being 'out there' is discussed in Derek Edwards, Malcolm Ashmore and Jonathan Potter's 'Death and Furniture' argument (1995). They point out that it is the relativist's perspective which accepts that we are able to observe and talk of them, attend to them, and even explicate how we do it. Realists will not be able to make 'facts and reality' available for us, as they deny our activity of observing and talking of them. This activity of ours employs a form of rhetorical argumentation (Billig 1996). A variety of devices for facts and reality construction have intensively been explored (Edwards and Potter 1992; Potter 1996; Edwards 1997).

27 The 'TWOD' has been found to be employed in a variety of discourses for fact construction (Potter 1997). For example, it is used to manage "the tension between the demands of science-based medicine and the uncertainty of medical practice" (Horton-Salway 1998).
important implications for the study of the process of knowledge construction. First of all, the TWOD clearly works to confirm the inevitable predominance of the theoretical view held by the scientist who employs this device. That is, it is held that the facts, in due course, will not only speak for themselves, but will also speak on behalf of the TWOD-using scientist.

Another implication is that the device works with the scientists' reference to time. That is, although empiricist factors are depicted as operating throughout the temporal sequence during which scientific knowledge is produced, they are treated as becoming increasingly effective over time. In contrast, contingent factors are portrayed as being influential initially, but as dropping away over time. It is important to notice that Discourse Analysis then locates this reference to time in the scientists' use rather than in the analyst's formulation of the transitivity of the process of knowledge construction. Namely, this programme shifts 'time progression' from being the analyst's concern to being the participants': the time line is not considered as a given feature. Consequently, time progression is analysed as a discursive achievement, rather than treated as a presumption of analysis.

Finally, this programme makes a radical shift in what counts as knowledge. Derek Edwards categorises three senses of 'shared knowledge':

(1) **Cultural knowledge** -- things that people generally know about the world or can be expected to know, within a given speech community, and that they use across different occasions of talk.

(2) **Mutual knowledge** -- things that individuals in interaction assume each other knows, and think the other person know they know (and so on), and that they update continuously as the conversation proceeds.

(3) **Pragmatic intersubjectivity** -- shared knowledge, as a participants' practical concern; what their talk treats as shared, and when, and how.

(Edwards 1997: 114)

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28 The TWOD will be considered again in Chapter 4 (p. 184-185).
29 Of course, discourse analysts are inevitably observing some sequences in a (series of) conversations or texts in their analyses, but the time progression in knowledge construction is shifted from the analyst's domain to the participant's domain.
It is this third sense of shared knowledge with which discourse analysts are concerned. They analyse extracts from participants' texts and talks to show how certain issues are treated and managed as known by (some of) them, as in situ discursive practices.

The programme's analytical focus is thus on the organisation of the in situ discursive practices (as participants' concerns) and the discursive devices used to accomplish a certain achievement. The process of knowledge construction turns out to have an open-ended and highly localised character. Consequently, this programme proposes the process of knowledge construction not as a single progressive time-line in which a certain knowledge-claim is made and consolidated or accumulated by being tied with other knowledge-claims through the scientific community's institutional sanctions. Instead, it focuses on a particular discursive process situated within a setting where participants discursively work up 'knowledge'.

G. On Being Constructed in Laboratories

With a view to contributing a detailed understanding of scientific practice to the social studies of science, a series of studies which are categorised under 'laboratory studies' (Latour and Woolgar 1979, 1986; Knorr-Cetina 1981; Law 1994; cf. Lynch 1985) have initiated close examinations of scientific knowledge. Those studies are mainly ethnographic, based on participant observations at the heart of "science in the making" (Latour 1987: 4), that is, laboratories. Some early laboratory studies have advocated the constructivist and/or constructionist programme of studying science. The concern for the process of knowledge construction occupies a prominent position in this approach.

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30 It is obvious that this programme entails not only the examination of a single extract or a few lines of conversation, but also that of a series of extracts or research articles which can thus supply a wider context. Nevertheless, its centre is the on-the-spot constructive work observable in talk which more or less appears to be staked off from, say, the whole institutional procedure regarding a certain discovery, that emerges in a scientific community as a whole or during a certain period.

31 Even though it is occasionally described as one of the initial and representative 'laboratory studies', (e.g., Woolgar 1982), Michael Lynch's study (1985) is better categorised under Subsection J (p. 32-35) for the purpose of this review.
For example, under the rubric of 'constructivist's approach', Karin Knorr-Cetina proposes that the actual process of research "should be seen as constructive rather than descriptive" (Knorr-Cetina, 1981: 5):

[T]he products of science are contextually specific constructions which bear the mark of the situational contingency and interest structure of the process by which they are generated, and which cannot be adequately understood without an analysis of their construction. This means that what happens in the process of production is not irrelevant to the products we obtain. It also means that the products of science have to be seen as highly internally structured though the process of production, independent of the question of their external structuring through some match or mismatch with reality. (ibid.: 5)

For Knorr-Cetina, such a process of knowledge production involves "chains of decisions and negotiations through which their outcomes are derived" (ibid.: 5). This is a process of selection in which:

the selectivity of the selections incorporated into previous scientific work is itself a topic for further scientific investigation. At the same time, the selections of previous work constitute a resource which enables scientific enquiry to proceed: they supply the tools, methods, and interpretations upon which a scientist may draw in the process of her own research. (ibid.: 6)

In such a process, scientists are observed to be "practical reasoners" in constantly orienting their decisions and selections to the expected response of specific members of the scientific community. The producer and the evaluators of knowledge-claims are generally members of this same community.

The process of knowledge construction is neatly illustrated with three findings of Bruno Latour and Steve Woolgar. As one of the initiating laboratory studies, they propose, in Laboratory Life (1979 and 1986), the programme called 'constructionism' for observing scientists' construction of scientific knowledge. First of all, 'facts' are made available in various processes of writing and reading which they call 'literary
inscription'. The 'inscription device' is crucial in fact-building, because it is "the only way [scientists] can talk and not to be undermined by counter-arguments as plausible as their own statements" (Latour 1983: 161).

Secondly, Latour and Woolgar find five different types of statement in scientists' writings, regarding the factual status of knowledge-claims. These types range from the most fact-like, which are treated as the basis for making other knowledge-claims, to the most speculative and evaluative ones. The changes in statement type correspond to changes in fact-like status. Some statements which contain statements *about* statements, Latour and Woolgar call 'modalities'. Latour and Woolgar state that these modalities have no definite correspondence with factual status. That is, the determination of the correct or more appropriate interpretation of the function of a modality will depend critically on the context in each particular case.

But in order to begin the observation of laboratories, it is a useful indicator for calibrating the path whereby scientific facts are constructed. The transformation of modality can reveal how the factual status of knowledge-claims is enhanced (or diminished) as a process. The fate of knowledge-claims is then in the hands of scientists who write successive statements (Latour 1987). Laboratories are in this sense "constantly performing operations on statements" (Latour and Woolgar 1986: 87).

The third finding is that of the stabilisation of statements wherein "the statement becomes a split entity" *(ibid.*: 177). This phase is schematised by Woolgar as 'the splitting and inversion' (five-stage) model of discovery (in this case, the discovery of pulsars):

1. document
2. document *-->* object
3. document object
4. document *<--* object

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32 For the sake of making a literature review, I am writing a(n) *(f)*actual status of the domains which are under review. My writing in this part has thus started by inscribing Latour and Woolgar's writings, and contains fact-like statements in terms of modality. A similar concern with the modalising and 'black boxing' (Latour 1987) of their findings, methods, conclusions and so on as uncontroversial, is neatly raised in Edwards and Potter's 'reflexive box' (Edwards and Potter 1992: 71).
(5) 'deny (or forget about) stages 1-3'

In the first stage, the scientists have documents (traces); in the case of the discovery of pulsars these comprise the charts from the telescope recorder, but might also include other publications, papers, previous results, the telescopes themselves, other apparatus, what Hoyle says and so on. At stage (2), participants use (some of) these documents to project the existence of a particular object ... Importantly, the object is created and constituted out of the documents available to the researchers. At stage (3), splitting occurs. Although the object was initially constituted in virtue of the documents (and more generally of the social network of which they are a part), it is now perceived as a separate entity, distinct from those documents. The object now has a life of its own. Indeed it is just one short step from possessing an infinite history; it is about to acquire the status of antecedent! In stage (4), the relationship between documents and the object is inverted. Whereas the object was constituted on the basis of the document in step (2), it now seems as if the object (which was there all along) had given rise to the documents! It is at this point that the documents most obviously appear to take on the character of 'representations' or 'traces'; they are no longer just documents, they become documents of something. Step (5) is crucial. In order to maintain the inverted relationship of step (4), it is important to play down or minimize all reports which draw attention to the earlier steps (1), (2) and (3). Step (5) thus comprises the minimization, denial or backgrounding of all component parts of the process. Step (5) rewrites history so as to give the discovered object its ontological foundation. (Woolgar 1988b: 68-69)

Woolgar further remarks that the above scheme "is unlikely to sit comfortably with taken-for-granted perceptions about the relationship between observations and objects in the natural world". The reader may react that the above steps (1)-(3) do not occur or are unacceptable. But Woolgar argues that such a reaction "is precisely the function of step (5)". Another possible reaction may be to think that steps (1)-(4) occur very quickly, so that we barely have the chance (or time) to recognise any alternative relationships than that which we see at step (5) (ibid.: 69).

33 But I guess that you are now at steps (1) and (2) for me in your reading.

574.072/LAT
The result of this process is that it (i.e., 'the fact') appears unconstructed by anyone (Latour and Woolgar 1986). But before reaching this state, there is a process of slow, practical craftwork by which inscriptions are superimposed and accounts backed up or dismissed with modalities. From this view, scientific activities are understood as a "a fierce fight to construct reality" (ibid.: 257). Latour and Woolgar observe the laboratory as "the workplace and the set of productive forces, which makes construction possible" (ibid.: 257). This conviction seems to have led one of the two authors further to pursue the way that the strength of fact in laboratories is enforced in society, and to have led the other to inquire how these powerful observations and convictions about knowledge construction in laboratories are themselves constructed by the sociologist and anthropologist.

H. Networking Relationships
The one who pursued the first line is Latour, who has gone on to developing 'the actor-network theory' in collaboration with Michel Callon, John Law and others. For these theorists, it is not only scientists, but all who live in an 'actor-world', which is "the world of entities generated by an actor-network" (Callon, Law and Rip 1986: xvi). They emphasise that for any kind of actor, "there is nothing beyond the network which it has created, which constitutes it, and of which it forms a part" (ibid.: xvi). The actor-network is:

an interrelated set of entities that have been successfully translated ... or enrolled ... by an actor that is thereby able to borrow their force and speak or act on their behalf or with their support. ... The actor who speaks or acts with the support of those others also forms a part of the network ... Hence the term actor-network, for the actor is both the network and a point therein.

(Callon, Law and Rip 1986: xvi)

[T]he texts, the inscriptions, the instruments, the skills, and the nonhumans, are all multiplied: none of them has a decisive weight ... It is true, but all of [them]
mobilized together, woven together, are enough to transform the indefinite pliability of a situation into an irreversible fact. (Callon and Latour 1992: 365)

At a glance, this approach can be seen to depart from a model of knowledge construction that progresses in a linear time-line, from laboratories to the wider society.

The departure is clearly illustrated by two entirely different, but co-constitutive models drawn by Latour: they are 'the diffusion model' and 'the translation model'. The diffusion model holds that "[s]pewed out by a few centres and laboratories, new things and beliefs are emerging, free floating through minds and hands, populating the world with replicas of themselves" (Latour 1987: 133). But the explanation given by this model neglects an important part of what is taking place in science. For example, what counts as novel becomes problematic in the reproductive system of facts in this model, and thus the notion discovery and discoverer have to be invented. But "no matter how carefully [the labels such as 'precursor' or 'unknown genius'] are attributed, the great men and women of science are always a few names in a crowd that cannot be annihilated even by the most enthusiastic advocates of the diffusion model" (ibid.: 134). Furthermore, one critical consequence of this model is as follows:

When a fact is not believed, when an innovation is not taken up, when a theory is put to a completely different use, the diffusion model simply says that 'some groups resist'. . . . [It] traces a dotted line along the path that the 'idea' should have followed, and then, since the idea did not go very far and very fast, they make up groups that resist. (ibid.: 135)

Alternatively, the translation model picks up what is dropped in the diffusion model and re-connects it with other elements in the net-worked science. This model employs the crucial notion -- that is, translation -- in order to explain how some knowledge-claims become parts of solid knowledge and others remain mere claims or artefacts.

Translation takes place when different domains of interests or different worlds are provided for in such a way that they neither do damage to one another nor pass each
other by without some mutually beneficial outcome resulting. It is "[t]he methods by which an actor enrols others" (Callon, Law and Rip 1986: xvii). That is, if one wants to transform a fact-like statement into a fact, or reinforce a fact to be a fact-with-strength, one needs to spread it out in time and space. In order to do this one needs the actions of others. But then, most of these actions are unpredictable.

So we are now in a quandary: either the others will not take up the statement or they will. If they don't, the statement will be limited to a point in time and space. But if they do take it up, they might transform it beyond recognition. To get out of this quandary we need to do two things at once:

- **to enrol others** so that they participate in the construction of the fact;
- **to control their behaviour** in order to make their actions predictable.

At first sight, this solution seems so contradictory as to look unfeasible. If others are enrolled they will transform the claims beyond recognition. Thus the very action of involving them is likely to make control more difficult.

(Latour 1987: 108)

The solution is *translation* between "the interpretation given by the fact-builders of their interests and that of the people they enrol" (*ibid.*: 108). It works "to solidify actor-worlds" which result in the achievement of "the seemingly natural order, where each element relates with the other" (Callon 1986: 28).

What Latour has introduced with these two models suggests two different orientations or 'angles' towards the world. With these two angles, science can be both *ready-made science* and *science in the making*34. On the one hand, established facts and machines can be seen with the language of *diffusion*. On the other hand, still undecided controversies can be seen in terms of *translation* (Latour 1987: 123). What we take to be knowledge in the diffusion model is then the consequence of some elements' gaining strength in the translation model35. With the latter model, the status

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34 How the process of knowledge construction can be grasped with the angles of *retrospection* and *projection* will be an issue in Chapter 4.

35 Then, the strength of my knowledge-claims about these SSKers' approaches are extremely weak, unless you start speaking of, and writing about this thesis as if it were under my control.
of knowledge is always a matter of its relation with other things, and is always in the making.

In the above approach, it is particularly noticeable that the synchronicity of the state of knowledge is emphasised far more than the diachronic progression. That is, the process of knowledge construction envisaged in the above approach is concerned with relations to other entities in the actor-network, rather than chronological progression. This is not to say that the actor-network theorists ignore historicity in the process of knowledge construction. On the contrary, they frequently use historical cases. But we can no longer use a model of one-way linear time with a starting point (or the Origin or laboratory negotiations) and an ending point (or what counts as knowledge, or the social recognition in the wider society).

I. **On Being 'Mangled'**

A similar kind of non-linear process is maintained in Andrew Pickering's claim that science is 'mangled' in a temporally emergent practice in a "posthumanistic" mode (Pickering 1995). The 'mangle of practice' is defined as "[t]he practical, goal-oriented and goal-revising dialectic of resistance and accommodation" in scientific practice (ibid.: 22-23). In this idea, scientific culture involves a dialectic of accommodation and resistance between human agency and material agency. From this viewpoint, speaking of knowledge, as if it is some describable entity, is itself problematic.

In a departure from the traditional SSK approach, Pickering tries to initiate "a real-time understanding of practice" (ibid.: 3) in scientific culture. What he seeks is:

The work of cultural extension in science as it happens in time. This is to be contrasted with retrospective approaches that look backward from some terminus of cultural extension and explain practice in terms of the substance of that terminus. The exemplary instance of the latter is what I call "the scientist's account" . . . in which accepted scientific knowledge functions as an interpretive yardstick in reconstructing the history of its own production. . . . [F]or my purpose, to indulge in retrospection would be circularly self-defeating and must be eschewed. (ibid.: 3)
Pickering claims that SSKers uphold science with the 'representational idiom' which "casts science as, above all, an activity that seeks to represent nature, to produce knowledge that maps, mirrors, or corresponds to how the world really is" (ibid.: 5). In contrast, his attempt is to move to the 'performative idiom' in which "science is regarded as a field of powers, capacities, and performances, situated in machinic captures of material agency. Knowledge is not excluded from this idiom but "the specific contents of scientific knowledge are always immediately tied to specific and very precisely formed fields of machines and disciplines" (ibid.: 146). The process of knowledge construction is a temporally emergent process where multiple material and human agencies are 'mangled' for stabilisation.

There are two concepts that appear to configure how the process of knowledge construction proceeds from this view: 'tuning' and 'modelling'.

Tuning in goal-oriented practice takes the form, I think, of a dance of agency. As active, intentional beings, scientists tentatively construct some new machine. They then adopt a passive role, monitoring the performance of the machine to see whatever capture of material agency it might effect. Symmetrically, this period of human passivity is the period in which material agency actively manifests itself. ... The dance of agency, seen asymmetrically from the human end, thus takes the form of a dialectic of resistance and accommodation, where resistance denotes the failure to achieve an intended capture of agency in practice, and accommodation an active human strategy of response to resistance, which can include revisions to goals and intentions as well as to the material form of the machine in question and to the human frame of gestures and social relations that surround it. (ibid.: 21-22)

36 It is hard for me to make a clear distinction between the representational idiom and performative idiom in the Candidate's research, which will be represented solely by performance. Clearly, there should be a continuous awareness that we are now engaging in reading/writing. It is inevitably an act of representing objects, but it is simultaneously a performance. This issue will be elaborated in Chapter 4.
In the above explanation, the 'intentionality' of scientific practice is defined as "extended temporality" in a process of modelling. Scientific practice has goals which "are imaginatively transformed versions of its present". The process of modelling ensures that "the future states of scientific culture at which practice aims are constructed from existing culture" (ibid.: 19). Modelling is an open-ended process with no determinate destination: human intentions are bound up and intertwined with prior captures of material agency in the reciprocal tuning of machines and disciplined human performances. The goals of scientific practice are open to successive transformation (ibid.: 20).

As stated in the above extract, scientific practice which proceeds by the tuning of human and material agencies is seen "asymmetrically" (ibid.: 22) for us humans. It is for humans that a process of modelling is a way to extend scientific culture. For humans, scientific practice proceeds towards stabilisation. But as the practice is temporally emergent, what is stabilised is open to be destabilised and restabilised. Reciprocally, what scientists (and we) know is in an open-ended process.

J. The In Situ Constitution of Cultural Objects through Embodied Practice and Temporal Historicity

I will introduce another approach in science studies which represents a radical departure from SSK. This approach is proposed in the domain of 'ethnomethodological studies of work' in the sciences (Lynch 1982, 1985, 1993; Garfinkel, Lynch and)

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37 In denying the exclusive role of humans in the practice of science, Pickering takes a stance similar to that of the actor network theorists. For critical arguments on this denial, see Collins and Yearley (1992a, 1992b) and Callon and Latour (1992). For the very issue of making a distinction between the human and the non-human, see Ashmore (1993a), Lynch (1993), Ashmore, Woffitt and Harding (1994), Edwards (1994), and Rachel (1994).

38 Likewise, this thesis is 'a display of my human struggle to extend my own 'culture', so to speak. This struggle is attempted within every act of my writing/reading. But I am not sure whether I would like to have any kind of stabilisation in your human reading of it.

39 Lynch categorises two programmes in ethnomethodology: one is 'conversational analysis' and the other is 'ethnomethodological studies of work'. Ethnomethodological studies of work focus on "how practitioners in various settings make their activities, along with their 'settinged' features, objectively accountable for all practical purposes". Science studies in this programme attend to "the ordinary, day-to-day production of scientific methods" (Lynch 1993: 22-25).

The trajectory of this programme is itself, as Garfinkel claims, "a systematically produced feature of ordinary society and accompanies ordinary society's locally produced ordinariness and their natural accountability as identifying details of ordinary society's production and accountability. That the
Livingston 1981; Lynch, Livingston and Garfinkel 1983). The programme proposes that instead of pursuing 'studies about scientists' work' (like other social sciences do), it will engage in 'studies of scientists' work' (Garfinkel, Lynch and Livingston 1981). The studies in this programme investigate scientists' work as a naturally organised ordinary activity. Like many SSKers, these ethnomethodologists treat science as practical action. But their emphasis is on the production of scientific objects through scientists' embodied practice and temporal historicity (Garfinkel, Lynch and Livingston 1981; Lynch, Livingston and Garfinkel 1983):

"Ethnomethodological studies attempt to discover and to demonstrate the ways in which various scientific practices compose themselves through vernacular conversations and the ordinariness of embodied disciplinary activities."

(Lynch, Livingston and Garfinkel 1983: 208)

This focus makes ethnomethodology, they claim, "a foundational discipline" (ibid.: 208): the methods by which the origins of science are demonstrable is the very topic of investigation. Historical development in science is not that which is specified through domain is ignored is a systematically produced feature of ordinary society's practical objectivity, its observability, its recognition, its understanding, or its analysis" (Garfinkel 1986: vii).

Because ethnomethodologists declare a departure from making 'explanations' of scientific activities and analysing scientific achievements in terms of 'accounts', and because their sociology consists of producing a sociological description (Sacks 1963) of members' work, how ethnomethodologists can take up their research object (i.e., the origin of science in the above case) in the first place is an interesting question:

[S]omething more is involved in actually engaging in a practice than can be formulated in even the most detailed of instructions. However carefully one attempts to follow, say, a manual of lab procedures, much more will need to be taken account of than is anticipated in the instructions. It is this ubiquitous 'something more' that delimits a field of investigable phenomena which is not schematized in formal accounts of scientific method. ... To mention this is not to make an issue about idiosyncratic origins in science, but to note instead that despite the absence of specific accounts of scientific methods, when scientists are at work they evidently are not at a loss over what to do. They find their ways through singular troubles, vernacularly organized discussions, and embodied routines of inquiry, and they do so as an unremarkable competency with 'the facts of daily life'. In other words, much of what evidently makes up the orderliness, and ordinariness, of scientific activities is not worth talking about in the idiom of 'scientific method'. Paradoxically, that is what supplies the motive for ethnomethodological studies; that there is no motive for generic methods accounts to make an issue of the ordinary practices through which scientists produce the evidently scientific character of their day's work.
chronologically linked 'events', but that which is embodied in work and vernacular 'shop talk' (ibid: 208).

I assume that this programme would be reticent to discuss 'the process of knowledge construction'. For ethnomethodologists, the locus for 'knowledge construction' (my concern) is located in the in situ local practice of 'competent' scientists with their embodied practice and its temporal historicity. 'What is known' (my description) is 'constructed' (my description) in "the intertwining of worldly objects and embodied practices" (Garfinkel, Lynch and Livingston 1981: 137). These

(Lynch, Livingston and Garfinkel 1983: 207-208)

In the above account, it seems that "something more" is identified by the ethnomethodologists. The above just-there-ness in scientists' routine work seems to be recognised and described in these ethnomethodologists' writings.

Then, we can ask how they have developed their own work of doing science studies. That is, the ethnomethodologists' activities, embodied routines of inquiry and 'the facts of daily life' can also be an interesting inquiry in terms of self-reference. It is interesting to see whether and how the vernacularity of the scientists' activities can be developed with the selfsame vernacularity of the ethnomethodologists' activities including their writings within the sociological domain.

I am not claiming that the just-there-ness of science written by ethnomethodologists is not genuine. My point is whether it can not be perceived and perceivable as such by us, unless the ethnomethodologists' writings achieve "something more" than talking about science in the idiom of ethnomethodology -- that is, unless they are read with "the orderliness and ordinariness" of ethnomethodologists' 'daily life'.

If the above two footnotes are read as engaging in 'tu quoque' (Ashmore 1989: Chapter 3) and being critical of ethnomethodology, please hold that thought until you read Chapter 3. I feel some difficulty in writing a literature review without engaging in a kind of 'tu-quoque'. How sociologists (including the Candidate) can know the inside members' activity and analyse it as an outsider will be the very topic I will deal with in Chapter 3.

What counts as 'competence' is indicated in ethnomethodologists' writings. Scientists' practices:

are occasioned; they are 'hidden' in and as their apt and familiar efficacy; they are only available to practitioners; and only to their vulgar competence, they are done unwittingly; they are developingly objective and 'account-able', i.e., observable-and-discover-able; they are unavailable to reasoned reflection, to introspection, to ethnographic reportage, to the analysis of ethnographic documentation, or to documented argument except, and at best, as documented conjectures; they are done in detail; they are real worldly, and they consist of all that detail can be in technical, material contents; they are only discoverable and cannot be imagined; and they are naturally accountable. (ibid.: 140)
ethnomethodologists would not describe this practice as the process of knowledge construction. For they prefer to formulate the issue not as how scientists construct knowledge about their research object, but as how the scientists in situ produce a "cultural object, not a 'physical' or a 'natural' object" (ibid.: 141). For them, the foundation of scientific objects, events, or demonstrations is "exhibited in the temporal 'building', and 'building-up' of those phenomena in actual courses of activity" (Lynch, Livingston and Garfinkel 1983: 208). Paraphrasing such courses of activity as a 'process of knowledge construction' is treated as a sociological 'gloss' (Garfinkel and Sacks 1970) and not as a description of scientists' activity.

K. So, Are Our Texts Bringing Knowledge about Knowledge into Being?
Can the 'reflexivist' approach in SSK provide an account of the process of knowledge construction? Ashmore introduces three kinds of reflexivity relevant for SSK in his 'Encyclopedia of Reflexivity and Knowledge' (Ashmore 1989, Chapter 3; see also Lynch 1996 and the section 'Approaching Reflexivity' in this chapter, p. 5-13). They are reflexivity as (1) self-reference, (2) self-awareness, and (3) the constitutive circularity of accounts. The first reflexivity is the turn of one's arguments onto themselves. The second reflexivity is the advocacy of thinking more deeply about what we are doing in a benign introspective fashion. The third one is a technical term in ethnomethodology: a document and its underlying pattern reciprocally define each other, i.e., they are co-constitutive.

The reflexivists' programme in SSK originated with the first and the third kinds of reflexivity. The roots of reflexivity studies can be, I believe, traced back to, on the one hand, one of the four tenets in David Bloor's 'Strong Programme' (Bloor 1976), and on the other, to Steve Woolgar's introduction of ethnomethodology into SSK (Woolgar 1978, 1981a). Whereas Bloor seems to envisage reflexivity on the 'in-principle' level (Ashmore 1989), Woolgar has continued to advocate the inclusion of reflexivity into SSK studies. What Woolgar incorporates under the rubric of reflexivity is, however, not only the relationship between practical actions and indexical expressions which
"remains programmatic in every particular case and in every actual occasion in which the distinction and substitutability must be demonstrated" (Garfinkel 1976: 6). For instance, Woolgar is critical of his own attempt at presenting an ethnographic description of science (Latour and Woolgar 1979, 1986) for selectively applying epistemological relativism (i.e., 'ontological gerrymandering') and thus for not being reflexive (Woolgar 1982).

Reflexivity is pursued in SSK as a programme that proposes to study and study with reflexivity. The inquiry into reflexivity takes various forms. Mulkay's exploration (1985) originates in the emergence of Discourse Analysis, and Ashmore's explication is uniquely targeted at the self-referential implications in SSK discourse and the development of experimental textual practice which tries to make itself more consistent with SSK relativism (1989). Knowledge and Reflexivity, which is a compilation of reflexive studies (Woolgar 1988b) is a kind of reflexivities' melting pot. Consequently, the different kinds of reflexivities mentioned above are often blurred or appear to overlap. Indeed, "to declare oneself to be in favour (or, for that matter against) reflexivity, is to beg the question of what brand of reflexivity one has in mind" (Lynch 1996).

My own view is that with respect to studies of the process of knowledge construction, the kind of reflexivity that attends to the self-referential nature of its own argument is especially relevant and interesting. This kind of reflexivity is treated as idiosyncratic to SSK. Its relevance comes from the rhetorical situation of SSK, conceived as a sociological study of scientific knowledge. For SSK is a discourse which "is routinely treated as inherently and supremely critical of its object" whose object (science) "is routinely treated as uniquely capable of producing valid

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45 "Ontological gerrymandering" is the central strategy characteristic of sociological arguments in the "social constructionist" approach. It works by "making problematic the truth status of certain states of affairs selected for analysis and explanation, while backgrounding or minimising the possibility that the same problems apply to assumptions upon which the analysis depends" (Woolgar and Pawluch 1985: 216). This strategy is used in SSK studies when relativism is only upheld for the research object, science, but not for their own resources. Woolgar and Pawluch's dis-solution (as it is an unavoidable feature in any sociological argument) is to search for a form of discourse which is free from the tension engendered by espousals of relativism within the conventions of an objectivist form of presentation, and a close examination of the rhetorical strategies which constitute social problems explanation.
Thus, it is not surprising that scientists "who wish to defend the status of science respond to its critique by turning the critical discourse of the sociology of scientific knowledge onto itself": the reflexive move is therefore "double-edged" (Ashmore 1989: xxviii).

Reflexivity as self-reference thus yields and stimulates a challenging range of activities for SSK. Ashmore's attempt is not to take it as a threat, but "to defuse its problematic reputation, and to show how a positive assessment of the phenomenon can open up new areas for scholarly inquiry" (ibid.: xxviii). In other words, reflexivists are trying to transform reflexivity from the negative self-reference which may result in an attack against their own knowledge-claims, to that which celebrates self-reference as the constituent problem of sociology and which attempts to address textual practices as self-exemplifying. Moreover, reflexivists display their own texts not only as "one of the constructions", but also as being open and ready for the next reflexive turn, most of the time by using 'NLFs' (Ashmore 1989; MacMillan 1996, this chapter, p. 11-12). This is a simultaneous constructing, deconstructing, and re-constructing practice of writing one's own text.

The process of knowledge construction in the reflexivists' approach is concerned with how to deconstruct (and reconstruct) constructed knowledge while simultaneously opening the very work of deconstruction (and reconstruction) to further deconstruction (and reconstruction) on the very spot where the work is pursued. For instance, in constructing the so-called state of the art of SSK, reflexivists simultaneously deconstruct it (Woolgar and Ashmore 1988). The state of knowledge is then not only in a dynamic process of construction as other programmes envisage: once one takes up reflexivity, one's own activity of knowing objects becomes unstable: one knows them while knowing that one's knowledge is also deconstructable and reconstructable. Accordingly, I am writing this review of studies of the process of knowledge construction, as deconstructable. Moreover, in a self-exemplifying way, I am trying to
engage in a kind of knowledge construction regarding the previous studies of the
process of knowledge construction as a literature review.\textsuperscript{46}

For the concerns of this thesis, it is not enough to review the approaches for studying
the process of knowledge construction in SSK and closely related research domains.
There are, of course, more approaches outside these domains. Some of them will be
discussed now.

[The Ordinary View]

L. Mundanely Reasoned

In sociological texts, a distinction between ordinary people and sociologists (or
analysts) is often made. Given that we accept this distinction (cf. Chapter 3, this
thesis), there are two questions regarding writing a review of the ordinary
understanding of the process of knowledge construction. The first question is, whether
I can write how ordinary people understand it in this thesis. How ordinary people
understand the process of knowledge construction may become, when it is analysed in
sociological texts, an 'ironic' version (Woolgar 1983). The second problem relates to
my own status: I am a candidate SSKer: whether I am a 'competent\textsuperscript{47} SSKer (or
whether I am more or less close to ordinary members of society than other SSKers)
remains to be decided. But either way, I will stay agnostic regarding the matter of my
status.\textsuperscript{48}

Regardless of whether the following account will be read as one of an ordinary
member of society,\textsuperscript{49} I shall continue to write how the 'process of knowledge

\textsuperscript{46}And I guess it is time to stop putting all my reflexive remarks into footnotes, and to start writing in a
way that textures my research object with reflexivity in all parts of this thesis (including this part of the
footnotes) now. (But then, what is \textit{this} footnote?)

\textsuperscript{47}See footnote 43.

\textsuperscript{48}See Chapter 3 for a further examination of the status of being inside-and-outside.

\textsuperscript{49}The legitimacy of my account as an ordinary member of society wholly depends upon how I report my
identity (Sacks 1984): if the report can announce the \textit{ordinariness} of 'being an ordinary member', the
account will be a mundane (and not analytical) account of the process of knowledge construction. Do
you think I am reported/reporting as an ordinary member of society in this text? But then, how can \textit{you}
judge it (as an ordinary member or as an outsider)?
construction' is mundanely understood. In an ordinary situation, I know something in a solid and stable manner. But I occasionally feel what I know is changing or advancing. To use a sociologically informed language, the way to describe knowledge is, for the ordinary-me, contingent. In displaying some kind of knowledge in sociology, the change and advance of what counts as knowledge in the ordinary world of mine is mundanely reasoned (Pollner 1987). That is, when different groups of people disagree, this is managed by ascribing the dynamically changing state of knowledge to contingencies.

It is clear, however, that the above bid for my membership of 'ordinary people' is not unproblematic, and that these knowledge claims being made right here regarding the process of knowledge construction for ordinary people have at most a candidate status as and in a sociological text.

[Endothelin Research]

M. Progressively Established

Another group whose approach is obviously important is the group of participant scientists in endothelin research.

In the interviews with them, I found that there is a divide between the known and the unknown regarding endothelin in these scientists' accounts. Furthermore, these scientists make a distinction between a corpus of texts in which the issue of whether it is known or unknown is a significant matter, and a corpus in which this does not matter. Most of them describe endothelin research as progressing, and what is known about endothelin as becoming clearer. These uptakes can be compared with a variety of "modalised" (Latour and Woolgar 1979 and 1986) statements in the endothelin literature. For example, in the original article which initially reports endothelin (Yanagisawa et al. 1988), 'endothelin' is given its name:

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50 Dr. Mary Horton-Salway, who inspiringly helped me in proof-reading the English of this chapter, has scribbled on this line: "but are you?"
We have now isolated a potent vasoconstrictor peptide from the culture supernatant of porcine aortic EC, determined its amino-acid sequence, and molecularly cloned the peptide precursor. This peptide, endothelin, does not belong to any previously known peptide family. (ibid.: 411)

But one of the interviewed scientists commented, in 1994, that the first figure in this article, which elaborates the method of extracting endothelin, has ceased to be cited, and the second figure which demonstrates the results of this extraction, has become less cited as the research has further progressed:

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751. Prof. Kimura This is- I mean that recently this graph ((taps Figure 1)) does not appear, too,
752. and only a line is drawn in this figure- ((points to Figure 2)) by saying
753. something like that the peptide was- it was isolated at this point.
754. Mushakoji Oh. Oh, is that- is it changing
755. [into that kind of way of omitted writing?] [Yeah. That's right. It's already so.] Yes.
756. Prof. Kimura (Prof. Kimura, 1 July, 1994)
757. Prof. Kimura

Prof. Kimura's indication of the omission of details of the verification system for the existence of endothelin suggests that what first counts as important in verifying the discovery lapses into technologies "once nature is produced in particular forms" (Fujimura 1996: 67). Apparently, the modality for fortifying (Latour 1987) the factual status of . . .

Hang on, hang on.

Who are you?51

This text, author. Let me speak once more, independently from you.

Oh, no, that makes me independent from the text. Well, I cannot accept . . .

I know. But let me just suggest one thing. It appears that you have started an analysis of the participant scientists' accounts in the wrong place. What you have just

51 I have stolen this utterance from MacMillan's Trance-Scripts: the Poetics of a Reflexive Guide to Hypnosis and Trance Talk (1996: 41) in which this utterance is raised at a point where one becomes aware of reflexivity.

40
written is your interpretation of their accounts, which should go in your analytical chapters.

What do you mean? I just want to include their approach here in my review.

I know. But you are writing this literature review within the SSK domain. You cannot treat the accounts and literature of endothelin research in the same way as you have treated the SSK literature.

Why not? As I have already written, this thesis will proceed along the lines of a triple parallel knowledge construction in endothelin research, SSK and the Candidate's research. I thought that we had agreed to let each of them speak.

Because, as far as I can see from the above text, you seem to have no other way of approaching them than an analysis in the SSK idiom. You are not letting the endothelin researcher speak.

Well, okay... yeah. I admit that they are all introduced in my own terms — my own idiom. But I have also applied a kind of analysis to the SSK literature. On this account, I haven't drawn any distinctions between the texts of SSK and those of endothelin research. I have tried to make it clear that just in the way that endothelin researchers hold their research object, endothelin, ontologically constant on all occasions, SSKers themselves hold their research objects as ontologically constant on each occasion. Likewise, you, the text, are discussing our research object in just the same way!

Are you suggesting that I am ontologically committed to it? Fair enough! It is you, the author, who needs to write a literature review as a requirement for the thesis. To make it a review, it is necessary to pin down 'the process of knowledge construction' as the single research object, but if you start intervening in this way, I will never...

Okay, calm down. But may I ask who started the intervention here?

[The Framework which is in the Process of Being Constructed in This Thesis]
N. 'A Becoming' which is Ongoingly and Potentially Projected and which Awaits its Fate in Retrospective Construction

Of course, how the process of knowledge construction is approached in this thesis has not yet been reviewed. But this is what will be made available in your reading of this thesis. More importantly, the approach will be introduced not just in its writing about the process of knowledge construction, but also in its participation in it.

Introduction of the Chapters

The chapter you are/am reading/writing is Chapter 1, which is an introduction to the thesis. The naming of the thesis has been pursued and the reflexivity in writing the thesis has been introduced. I have also illustrated how 'the process of knowledge construction' has been treated in the Candidate's background research domain, SSK and related domains, the society for ordinary members, endothelin research, and in this thesis. The introduction of this thesis (as self-referentially introduced) will be followed by a brief comment on the way the Candidate transcribed and translated the participants' talk. Finally, I will explain how the subject is constructed in this thesis.

Chapter 1 will be followed by the body of this thesis which has two parts: Part I, Construction, and Part II, Process. I will explain how they are structured as follows:

PART I CONSTRUCTION

In Part I, I choose three research domains and examine the constructed status of knowledge in each of these domains. The three domains are endothelin research, SSK and the research the Candidate student is pursuing as her PhD thesis.

In Chapter 2, I deal with the construction of 'nature' in the three research domains. For endothelin research and in the Candidate's research, endothelin will specifically be brought into focus as a case of (or rather, a part of) nature. Accounts of nature taken as case studies in SSK will also be examined.

52 What this heading refers to will be elaborated in Chapter 4.
Along with this examination, the chapter is concerned with the following issue: for any analyses, the activity of reading the materials is an inevitable feature; and the reading itself involves knowing what the text is about. This issue is not only topicalised but also displayed in this chapter. In order to examine accounts of endothelin, two genres of scientific texts -- that is, an original article and the extract taken from the transcription of an interview with an endothelin researcher -- are analysed. The analysis shows how textual devices, such as sequential paraphrasing and naming, make the recognition of endothelin available, and how the generation of intertextuality plays a role in establishing, configuring and consolidating endothelin. It also shows how one particular scientist's account of endothelin renders in situ the generation of intertextuality in a particular setting, where a dialogue\(^5\) between himself and the participant interviewer (Mushakoji) is taking place. In this analysis, however, the activity of knowing endothelin is simultaneously taking place in the reading of this article and the extract. I try to explicate the intermingled and paradoxical relationship between analysing the accounts of endothelin (from a social constructionist perspective) and knowing endothelin (for oneself). The text of the analysis is compared with SSKers' writing of their cases, and the extent to which SSKers are committing themselves to nature in their writings is demonstrated.

This analysis begs the question of the Candidate's own involvement in the work of knowledge construction of endothelin in writing/reading this thesis. Moreover, there is another side to this question: the question of knowing endothelin for yourself.

\(^5\) This notion of dialogue is grounded in one of the Bakhtinian notions of dialogues that "all social and psychological entities are processual in nature. . . . [F]or any individual or social entity we cannot properly separate existence from the ongoing process of communication" (Morson and Emerson 1990: 50). The notion is opposed to the "monologized" or "finalised" worldview where the openness of dialogue is reduced to a closed systematicity and potentials are completely overlooked:

If we transform dialogue into one continuous text, that is, erase the divisions between voices (changes of speaking subjects), which is possible at the extreme (Hegel's monological dialectic), then the deep-seated (infinite) contextual meaning disappears (we hit the bottom, reach a standstill).

(Bakhtin 1986a: 162)

Real dialogism will incarnate a world whose unity is essentially one of multiple voices, whose conversations never reach finality and cannot be transcribed in monologic form. The unity of the world will then appear as it really is polyphonic.

(Morson and Emerson 1990: 60)

I will try to perform the dialogue in this sense with the Candidate's own case in Chapter 5.
through your reading of this thesis. Just as endothelin is observed to be co-constituted by the participant scientist and Mushakoji, it is also being co-constituted in the activity of reading/writing this thesis.

In Chapter 3, I deal with the constructed status of '(scientific) discoveries' in the three research domains. In particular, my focus is on the uptake in which (scientific) discoveries are 'socially attributed' (Brannigan 1981). First of all, I examine how the work of attributing the initial reporting of endothelin in 1988 is organised. This is analysed by taking up those texts which are concerned with the recognition of endothelin and its discovery. Throughout the Candidate's research, the 1988 achievement of a Japanese research group, called 'the Tsukuba Group', has consistently been called 'the discovery'. However, another interesting possibility emerges when we examine an alternative claim by scientist Robert F. Highsmith. In the historical review published by Highsmith (1992), he claims that his group had published a hypothesis about the existence of "endotensin" in about 1982, and that endotensin is endothelin's former name. This claim invites us to interrogate our own work of attributing a discovery status to the Tsukuba Group's achievement. I focus on Highsmith's explanation of his group's failure to formally name "endotensin", and examine the issues regarding his use of these two names, in order to discuss the relationship between the act of naming and the establishment of discovery and its originality.

In the last half of this chapter, I further examine a problem which has been raised during the above analysis. That is, the problem of how we can both know, as members of society, that a certain achievement or event is a discovery, and at the same time, analyse the attributional work of members. This will be examined in terms of 'the problem of being inside-and-outside'. For this examination, I will take up an SSK - and ethnomethodologically - informed discovery study, conducted by Augustin Brannigan (1981); Harold Garfinkel, Michael Lynch and Eric Livingston's ethnomethodological study of work which treats scientists' work on "the optically discovered pulsar" (Garfinkel, Lynch and Livingston 1981); the participant endothelin researchers' sociological accounts of 'the discovery of endothelin', and the Candidate's own account.
In sociologists' sociology, the problem of being inside-and-outside is managed by separating the members' domain and the analyst's domain (methodological relativism), or by concentrating on describing the members' work and taking a policy of 'ethnomethodological indifference' to any evaluative activities. In terms of the Candidate's own research, she is faced with neither a clear separation between the participants' domain and the analyst's domain, nor an avoidance of her participation in knowledge construction as a participant/analyst. The Candidate is inevitably participating in the attributional work of the discovery of endothelin, and attempting to analyse it. Her research proceeds in a process wherein both activities are interlinked. Thus, she obviously takes inside-and-outside roles. By introducing the sociology of scientific discoveries pursued by the participant endothelin researchers, I show that scientists are also insiders and outsiders: they discuss the contingency of the attributed status of their own discovery and thus problematise it. However, they also establish this discovery status, presuming and consolidating it by projecting how its potentiality

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54 'Ethnomethodological indifference' is one of the essential policies taken in ethnomethodology:

Ethnomethodological studies of formal structures are directed to the study of [members' phenomenon where the mastery of natural language is involved], seeking to describe members' accounts of formal structures wherever and by whomever they are done, while abstaining from all judgements of their adequacy, value, importance, necessity, practicality, success, or consequentiality. We refer to this procedural policy as "ethnomethodological indifference". (Garfinkel and Sacks 1970: 345)

I am convinced by the usefulness of this policy for studies which espouse a Wittgensteinian understanding of our mastery of language (Lynch 1992). Because I do not exempt my own knowledge construction from my analysis of knowledge construction, however, I am not able to ignore my own "judgements of their adequacy, value, importance, necessity, practicality, success, or consequentiality" in knowing members' objects and analysing them (which is consequentially to know how members come to know them. See Chapter 2 and 3). Thus, my approach takes issue with this policy.

55 Ashmore has explicated the problem of inside-and-outside in the relationship between analysis and meta-analysis. He claims that how these two analyses are established as distinct analyses pursued in separated levels, or pursued by an analyst and a Superior other analyst. His critique is that such separation is unsustainable, and when used for a 'tu quoque', it generates sterility. In the Candidate's case, in order to pursue a reflexive study of the discovery of endothelin, she has to face the question of how she can simultaneously pursue the tasks of knowing the discovery as an inside member of society and analysing it within an SSK framework. For reflexivists, when an analyst claims that she retains an ontological commitment to her object despite her analysis of it, such a claim is regarded as "disingenuous": "To show how objectivities become objectivities is to show that they are not objectivities" (Ashmore 1989: 98, Woolgar 1983). But it seems that an analyst does not so easily discard her object-for-members (in this case, discovery) by taking up her sociological object (in this case, the construction of the same discovery). Rather, she is creating a sociological text in which both objects (the one for members and the one for sociologists) are interlinked. She is showing how an objectivity (e.g., discovery) becomes objective, while retaining her knowledge of this objectivity (which becomes yet remains objective). On such an occasion, perhaps knowing 'it' relatively (and still making it the object on which one stands) is possible in a relativist epistemology.
becomes greater as the research proceeds. They thus engage in attributional work on their discovery. If such an inside-and-outside role is possible for scientists, how does this double role apply to sociologists who pursue the sociology of discoveries? Sociologists are not only analysing members' attributional work, through describing scientists' practice, but also participating in this attributional work through writing their sociology.

Finally, I illustrate the paradox of being inside-and-outside with the Hofstadteresque scheme of 'A Strange Loop, or Tangled Hierarchies' (Hofstadter 1979): in sociological texts, the account as an insider and the one as an outsider appear to be entangled: the paradox is a construct generated on 'the Inviolate Level' (ibid.: 689). But where is this level located? It is precisely in your/my reading/writing where a variety of intertextual ties are generated in process.

The examination of discovery status shifts its concern to a further implication56: whether knowledge-claims which are in the process of being made in the writing/reading of this thesis can be treated as self-exemplary. In other words, will the Candidate's research itself be treated as a discovery in SSK?

PART II PROCESS

This final concern is further developed in Part II. The process of knowledge construction is taken up both as its own research object and as the textual form of this thesis. That is, it envisages that writing/reading about the process of knowledge construction is synonymous with its attempt to construct itself as a PhD thesis. In other words, I try to display the process of knowledge construction both as what the thesis is about and as how the reading/writing of the thesis is performed. My concluding proposal is that the writing/reading of this thesis is the Candidate's wrighting of triple parallel knowledge construction.

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56 This is a case of "a jumbling-up of textual levels" (this chapter, p. 7) which occasionally appears in this thesis.
The focus in Chapter 4 is on 'process' itself in the process of knowledge construction. I try to study it by examining the following three questions: first, how it is that knowledge construction in endothelin research is 'in process'; second, how what is observed to be 'in process' can be studied in the research programmes proposed in SSK, and third, how such SSK studies can be engaged, self-referentially, in a process of knowledge construction. For this task, I employ two different angles: the angle of retrospection and the angle of projection.

Employing these two angles, I consider how far the process can be analysed as a process (and not a produced construction). First, the process of knowledge construction in endothelin research is analysed in terms of how it is constructed by the participant endothelin researchers, the interviewer Mushakoji, and myself (and the Candidate). My own story of the research development and the three components of 'process' observed in interview talk are introduced. I proceed to analyse how the process of knowledge construction is constructed by the participants as follows: (1) how claims about the significance of endothelin bring the process of endothelin research into existence, and (2) how the variations in participants' claims that "endothelin is further known" show that such 'further knowledge' cannot be reduced to a single formulation.

However, the above analysis is not enough if one tries to study 'the process of knowledge construction', not as one of constructions, but as a process. In order to be able to see it as being in a process which is ongoing and potentially proceeding, I need a further explication of the status of endothelin researchers' knowledge-claims. This explication is pursued by analysing how the endothelin researchers' strategic actions on the one hand, and endothelin and endothelin research as becomings on the other, are "intertwined" (Pickering 1995) and can be regarded as the two vectors of the process. Employing the angles of retrospection and projection suggested above, I analyse the endothelin researchers' accounts with respect to how the nature of endothelin, the state of endothelin research, and the scientists' actions are both established in retrospect, and
projected as future concerns, so that these issues are made currently and ongoingly relevant.

To bring the above two angles into the analysis converts the analysed object from one which has already been constructed into one which has the currency of ongoingness and potentiality. However, I again emphasise that this analysis cannot successfully construct its object as being ongoingly and potentially proceeding unless we reflect on the status of my analysis itself. Namely, the knowledge construction in any research in SSK and the Candidate's own research which analyses the process of knowledge construction in science, are ongoingly and potentially proceeding in process, and thus can be analysed in terms of the angles of retrospection and projection. I thus first briefly illustrate the status of analyses in SSK from the two angles. This consideration also allows us to attend to the pivotal dimensions of the process -- i.e., the creativity and nonsense in knowledge construction at the angle of projection -- which have escaped from the retrospective analysis.

Finally I focus on the retrospective and projective angles of the Candidate's research. This chapter is concluded by discussing the implications of the above analysis and also by a performance of this implication. With extracts taken from interview transcriptions, I display how the Candidate's own knowledge of endothelin is established and how the process of knowledge construction in her research is aligned with that in endothelin research. I discuss (at the angle of retrospection) and make a claim (at the angle of projection) about the status of her research. Her research, which takes the angle of retrospection in the analysis, is simultaneously a performance of making her own knowledge-claims. Moreover, this performance at the angle of projection has its counterpart -- i.e., what will be described as her research object in retrospect.

The final Chapter 5 is the Candidate's performance of wrighting the triple process of knowledge construction. In order to show that her research is in process, I show how the analysis of the process of knowledge construction is being opened up to be (re-)analysed-back by the participants. The text of this chapter is not only a display
of analysing-back to their 'analysing-backs' again, but also invites further analysing-backs. In other words, this chapter searches for a form in which a series of analysing-backs are able to proceed potentially and ongoingly.

First of all, I shall examine how the Candidate's research can (not) be self-exemplary by taking up her own earlier writing (Mushakoji 1995). I will display how this writing/reading has been re-written/re-read by both the participant endothelin researchers and her social scientist colleagues. This display will employ a form of oral presentation in which a successive achievement is in situ reported at an STS conference (Mushakoji 1996). It has 'a nested structure' where the text of the article written in the Candidate's early research is included in her oral presentation which is in turn included in my writing, as if accumulated like a snowball, and where my writing is itself seen as again being included and potentially accumulated into her research. In addition, it is shown that on each occasion the Candidate's ongoing research is temporally presented, the presentation takes up responses from participants in endothelin research and in social sciences and that her research stays in an open dialogue.

This dialogue of course works up controversies. In those controversies, the criticisms and inquiries regarding her approach have been raised as its responses. Among these controversies, I deal with the following 'analysing-backs', which result in some pivotal issues for any 'celebratory practical reflexive pursuit':

1. The textual form of this thesis represents the Candidate as a multiple subjectivity (such as "the Candidate", "I", "Mushakoji" and "Sumiko") and this is criticised, since it works to avoid what Descartes calls 'self-reflection' (this criticism will result in raising issues regarding the construction of Author, the ownership of Authority, and so on),

2. The Candidate's approach of reflexive-constitutedness and self-referentiality is precisely taken up, for a next reflexive turn, by the participants. (And this taking-up will result in revealing the difficulty-and-potentiality in not taking this criticism as undermining one's own research, but rather as 'celebratory practical reflexive inquiry' [Ashmore 1989]).
(3) Her approach is criticised for being in an infinite regress of self-absorption. (The dilemma she faces is how to respond to a response that commands her not to include it as a response.)

My analysing-back to each of the above cases is performed by making the following claims: first, in the triple parallel process of knowledge construction displayed and performed in this thesis, even the activity of constructing the knowledge, the subject of this construction, and the linguistic competence for this construction are constructed in process by the participants: second, this knowledge construction is not one of encapsulating the first two processes (i.e., those in endothelin research and in SSK) into the last process (i.e., the one in the Candidate' research): it is not leading to an 'infinite regress'. The approach employed in this thesis is far from a mere collection of all the responses into the triple parallel knowledge construction without any evaluations: it is instead an attempt to deconstruct them to the utmost extent of including one's own constructive activity, i.e., the constructing subject and the language for this construction. Such an approach is never easy to pursue for the analyst.

As long as the knowledge construction claimed above is potentially attempted in the reading/writing of this thesis, it will be a process of knowledge construction, in which a dialogical space will be opened such that our knowledge is mutually constructed through our continuous activities. Then, these claims will not only be said 'without doing' (Ashmore 1989), but they will be precisely performed in writing/reading this thesis. In this writing/reading, knowing the relevant issues is taking place in situ, and this knowledge is ongoingly developing in a process. Furthermore, this writing/reading is not only a matter of your practice of constructing knowledge about the research object, but it will also become your participation in the process of triple parallel knowledge construction. It is of course possible for you to respond to this attempt in three ways -- that is, by agreement, silence or refusal. Whichever kind of response is made, however, your reading/writing, as well as my reading/writing, is in a
dialogue with the Candidate's wrighting. In this wrighting, I am asking you to be ongoingly and potentially constructing our knowledge.

The Problem of Transcription in This Thesis

There is a particularly significant side-issue in this thesis. It relates to the way that participants' accounts are presented. As the outset (this chapter, p. 4, 6-7), I have promised to make an effort to allow the voices to speak for themselves in this thesis, rather than merely treating participants' talk as 'data'. But obviously, anything presented as 'talk' here is constructed by the Candidate herself. This construction involves, in the Candidate's case, making recordings in particular settings, extracting them from the settings, processing them under the rubric of social science methods, and selecting relevant pieces for a specific analytical and interpretative purpose.

In the Candidate's research, however, such a constructive procedure has been more of a critical issue than usual, due to her non-nativeness. In particular, most of the interviews were conducted and transcribed in Japanese. The transcriptions were then translated into English. In the course of this construction, a few native English-speaking colleagues have helped her to make the translated versions more naturally Anglified. Throughout this procedure, she has been able to clarify such issues as what was said, what was meant and how it was originally said. Even though being aware of this conduct of pre-interpretation, the task has been done out of necessity: her interpretation (and authorisation) have thus, in being witnessed, preempted the 'standard' analysis. When particular parts of the extracts were selected for inclusion in the thesis, the constructive procedure has even been repeated by going back to the recordings and transcriptions. This repetition often resulted in alterations to the translation. Although I believe that the analysts' construction is pervasive and

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57 The transcription symbols used in this thesis are explained in Appendix II.
58 My particular and profound acknowledgements to Dr. Kevin McKenzie, Dr. Mary Horton-Salway and Dr. Katie MacMillan for their rigorous and reflexive support.
59 And writing this thesis has been very much supported by my supervisor, Dr. Malcolm Ashmore, and the colleagues above mentioned (footnote 58) (Many thanks!) by making it fluentish in English. Here, I firmly claim that the originality for this PhD research belongs to myself, but such a claim is risky when you insist that originality is to be ascribed to a text written in a certain language.
unavoidable for any kind of analysis, the constructive operations involved in this thesis are an issue that cannot be neglected.

Although I consider the explication of this issue to be important, it will be left for some future discussion\(^6\), since it is beyond the scope of what can be accomplished here in this thesis.

**Introducing Multiple Subjectivity**

*Author?*

...  

*Author?!*  

Are you calling... me?

Yes. You. You are again separated from me, the text. I would like to suggest one more issue here.

Oh. Okay. What is it?

Let us refer to what can be counted as 'the author' in a multiple way. As we have done thus far, we will employ 'I', the Candidate, you (when what can be counted as 'the author' gets splits into two and starts a dialogue between its parts), the author, Mushakoji and Sumiko.

You mean... I will not be the only person who is authorised.

Neither am I. Perhaps, none of our readers are, either. It's all right, though, since multiple subjectivity can be observed in some SSK writings, and of course, in some reflexivists' writings with NLFs (e.g. Mulkay 1985, 1988; Ashmore 1989; Woolgar and Ashmore 1988).

But one drawback of employing these kinds of unconventional textual forms is, it might "run the risk of irritating the reader" (Pinch and Pinch 1988: 191). I am reluctant to take such a risk.

Really? Try to be a bit more consistent with how we have been (to be unconventional)! You have already let me speak independently and unconventionally.

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\(^6\) Some crucial problematics will, however, be raised in Chapter 5, 5-3, p. 261-276.
Moreover, there are inevitably some reasons why multiple subjectivity is relevant for this thesis. Now, you need to specify them.

No. It is not my task, you do it, text!

No. You do it, author!

SOMEONE CAN DO IT!61

First of all, I want to attend to how I am constructing the variety of settings which I claim to have taken place and in which I have participated. I am displaying how Mushakoji interviewed the participant endothelin researchers in the extracts taken from the transcriptions. In analysing her talks with the participant scientists, I am displaying that Mushakoji is also one of the participants. Likewise, I am displaying that it is Sumiko (and not Mushakoji) who is engaged in discussion in the extracts with her supervisor and colleagues. In the interviews, the interviewer is identified with her surname and the participant scientists are identified with their titles and surnames (as I feel appropriate as a matter of [Japanese?] courtesy). In the discussions with her supervisor and colleagues (in the U. K.), everyone is called by their first name (as I feel most comfortable). They are, as are the author of this thesis and myself, constituted as varied and variable identities.

Secondly, it calls attention to the self-referential nature of this study. To unify the various identities who appear in this thesis as being the same clearly attends to the sameness-and-differences issue: they are established as being the same person appearing differently. It is this text, or rather your textual practice of reading this thesis, which constitutes all of them as having the same identity. In other words, construing her/them as being the same and different, is a textual practice. But in a study of one's knowledge construction, what is being accomplished by this form of constituting the self? The writing itself becomes both writing about the process of knowledge construction in someone's research, and this same person's writing. This is, as I might conclude, not two distinct tasks but a single task. 'A display of' a distinction

61 She/he/it is the one recognised by you, the reader/writer.
between the identities, however, alerts us that this task *is being construed* as different: the object of writing and the writing itself cannot be recognisable as two distinct issues unless this distinction is first made. That is, *I* am writing about how the process of knowledge construction in *the Candidate's* research proceeds, while attending to how *my own* writing is a process of knowledge construction. The process of knowledge construction in *her* and *my* writing actually becomes the one and only process of knowledge construction, if *I* do not make a distinction between the Candidate and myself.

Finally, *I* will raise the question of how it is possible to unify these distinct entities under the rubric of a single Author. While *I* am writing, *I* have ceaselessly been aware of some need for taking a distance from the *Candidate's* research, just for the sake of analysing it. Namely, *I* can more easily write about how she, rather than *I*, constructs the knowledge regarding *her* research object. In analysing the data, *I* am also alerted to the issue of how *I* am authorising what the Candidate, Mushakoji or Sumiko said, what she meant, and what kind of setting she participated in. This issue has to do, not only with the authoritative force of the text, but also with the self as a concept which seems "to be disengaged from the Self in the act of representation" (Woolgar 1995). What is not clear in following the textual convention of unifying a multiple subjectivity is how far *I* would authorise, or rather, how far *I* am participating in the process of knowledge construction (taking place in *the Candidate's* research) which *I* will analyse, what can be observed in the displayed texts and talks (where Mushakoji and Sumiko are participants), and the ownership of this thesis (written by the author). This is an important aspect of the insider/outsider issue (Chapter 3), *my own* performance for knowledge construction at the angles of retrospection and projection (Chapter 4), and *my* dialogical critique of the criticisms raised against the Candidate's approach to the reflexivity in her research (Chapter 5).

Obviously, *you* and *I* will have no trouble in identifying those multiple persons as a single author in reading/writing this thesis. The multiplicity of these

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62 You with your multiplicity, of course.
persons is of course my own display and thus the potential authority will not be diffused. But more radically, you may think that 'the author' is "an effect or product of your own" (McHoul 1982: 5). Then, how far the I in this text can be traced back depends on how far "we look for a sentient being to attach the authorship to" (Hofstadter 1979). It is not necessary for you to take for granted that they are single in the first place. In making a connection between them explicit, the text will try to open ways to see our work of reading/writing as contingent methods for making them unified.  

**Introducing the Triple Parallel Process of Knowledge Construction**

Now, finally, I can really introduce the triple parallel process of knowledge construction into this text. I mean, after trying to make this text fulfil the requirements as an introduction of a PhD thesis (Loughborough University 1983; cf. Ashmore 1989: Chapter 7; MacMillan 1996: Chapter 1), I can now really start to make it proceed in the following way.

Each of the chapters which will follow tackles the triple process at a slightly different angle. At the beginning of each of the chapters, I will present what kind of angle will be taken with a chart more or less similar to the following:

<table>
<thead>
<tr>
<th>Nature of endothelin</th>
<th>pursued by</th>
<th>Endothelin research</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Scientific Knowledge</strong></td>
<td>pursued by</td>
<td>SSK (Sociology of Scientific Knowledge)</td>
</tr>
<tr>
<td><strong>The Processes of Knowledge Construction</strong></td>
<td>pursued and performed in</td>
<td>The Candidate's PhD research</td>
</tr>
</tbody>
</table>

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This point will further be elaborated in Chapter 5, 5-2, p. 254-260.
Part I

CONSTRUCTION

Part I proceeds with the development of the triple parallel knowledge construction in three domains (that is, endothelin research, SSK and the Candidate's research) regarding the recognition of nature and scientific activities. In Chapter 2, the main concern will be the nature of endothelin. In Chapter 3, I will focus on (scientific) discoveries.
CHAPTER TWO

Epistemological Checkin

Sociology, like the rest of the sciences, and, for that matter, like all modes of inquiry, is directed to the explication and analysis of a world whose 'thereness' is essentially non-problematic. It is precisely in and through inquiry's preoccupation with a world already there that the network of assumptions and practices through which the thereness of the world is created is disattended. Inquiry gains a world but loses the work of worldlying. For radical inquiry, by contrast, the phenomenon par excellence is not the world per se but worldlying, the work whereby a world per se and the attendant concerns which derive from a world per se -- truth and error, to mention two -- are constructed and sustained. (Pollner 1987: 7)

<table>
<thead>
<tr>
<th>Nature</th>
<th>Knowing and Analysing Endothelin in Endothelin Research</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Knowing and Analysing Nature in SSK</td>
</tr>
<tr>
<td></td>
<td>Knowing and Analysing Endothelin in the Candidate's Research</td>
</tr>
</tbody>
</table>

Introduction

This chapter takes up the issue of nature in endothelin research, SSK and the Candidate's own research. For endothelin research, and for the Candidate research, endothelin will specifically be brought into focus as a case of (or rather, a part of) nature. For SSK, the focus of study will be how nature itself is constructed.

The aim of this chapter is three-fold. First of all, it will explain how endothelin is constructed in participants' accounts of its discovery. This concern is analytical. I will inquire how endothelin came to exist sometime around 1988, as a result of its discovery in basic medical science. This inquiry can itself be formulated as informing the construction of endothelin in the SSK framework.

The second concern is related to the way the first concern is achieved: that is, our reading for the above analysis. It is obvious that endothelin is also established in

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64 This title is an allusion to Collins and Yearley's Epistemological Chicken (1992).
our readings of endothelin-related materials and in this thesis, precisely for the sake of explicating the first concern. The Candidate acquainted herself with a variety of relevant materials in order to pursue her PhD research. She read, listened, discussed, transcribed, and analysed the corpus of texts that she regarded as relevant to her research. Those texts are not only analytical objects, but are also readable for knowing what endothelin is. In other words, in the course of becoming acquainted with her research object, she has also come to know endothelin. In order to highlight this dimension of knowing it, particularly that of the knowing which unavoidably occurs in the activity of reading, I would like to present the texts of these materials not only as analytical objects, but also as those which inform us of what endothelin is. I will thus try to grope for what it means for the Candidate to read about the issues regarding endothelin. This will be compared with other SSKers' readings of nature in their case studies of science.

So, I take it that what endothelin amounts to in our reading and writing is one of the issues here. But that leads us to a further question. Whose reading will be treated as authoritative here?

Who are you?\(^{65}\)

Reader A reader of this chapter.

Oh. Oh, I see. Yes, of course, "you" will be the first one to read what you write. But, as far as I see it, you are in fact writing your reading.

Writer Yes, this "I" here, who writes, is actually interchangeable with the "I" who reads. But the fact that it is the author who occupies these two roles does not necessarily preclude her from having an argument generated from these two positions.

\(^{65}\) See footnote 51.
We must also mention that there may be other kinds of readers for this candidate thesis. Now, I have a question to put to them as well.

So, how do you know about endothelin, Reader? Has it come to exist with your own "inscription device" (Latour and Woolgar 1986) in your laboratory or through your talks with colleagues? Or, have you brought it into existence by reading an article in journals such as *Nature* or *Proceedings of the Royal Academy of Science*? Perhaps, you have started to give it existence when you picked up this thesis and skimmed some part where an expository account of endothelin is written and then with that you...

Pursuing these two concerns simultaneously, this chapter will attempt both to read the literature of endothelin research (and the transcripts of interviews with endothelin researchers), and to analyse them. In other words, it tries to inform both about endothelin and also about the construction of endothelin. Is it not the case, however, that engaging in both these tasks appears to be contradictory and thus problematic? Or, alternatively, are these tasks intimately interlinked? These questions will be discussed further; indeed, the chapter's third concern is the problematics inherent in the work of both reading about endothelin and analysing how it is established.

2-1. Reading Nature and Analysing Texts

The materials I will read and analyse in the next two sections are of two genres. The first item consists of the opening two pages of an article published in *Nature*, 1988, entitled 'A novel potent vasoconstrictor peptide produced by vascular endothelial cells'

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"Inscribing" is what is done, in laboratory settings, with "inscription devices" (Latour and Woolgar 1986; Akrich and Latour 1992). The devices:

... transform pieces of matter into written documents. More exactly, an inscription device is any item of apparatus or particular configuration of such items which can transform a material substance into a figure or diagram which is directly usable by one of the members of the office space.

(Latour and Woolgar 1986: 51)
(Yanagisawa et. al. 1988) which I will call 'the original 1988 article'. Its publication raises a number of interesting issues in that this article is generally regarded as having initially reported the existence of endothelin. This issue will be discussed further in the next chapter. Some extracts have also been taken from the transcription of an interview between one of the participant scientists and the Candidate. The particular sequence I will draw upon is where this scientist accounts for how endothelin was extracted with a method, known as HPLC, established prior to the extraction of endothelin which was mastered by him and his colleagues. This account is treated as a dialogue between the scientist and the Candidate.

Before reading and analysing them, I will first discuss how my analysis will be pursued. My analysis will be based on Woolgar's claims of an "isomorphism" between presentational context and scientific concepts (Woolgar 1980). As Woolgar puts it:

"[O]n each and every occasion that participants refer to a fact they do so in such a way that the facticity of the phenomenon is re-established. In referring to a thing, work is done to sustain its fact-like nature, or to say it differently, to pass off the thing as being a fact. . . . [T]he practical expression of, or reference to, a phenomenon both recreates and establishes anew the existence of the phenomenon. In describing a phenomenon, participants simultaneously render its out-there-ness."

(Woolgar 1980: 246)

Woolgar develops Dorothy Smith's ethnomethodological work (Smith 1978) into two implications for analysts: Firstly, any analysis which "arbitrates on the actual existence or otherwise of phenomena" (ibid.: 246) is to be avoided: Secondly,

"[T]he only way to recover the character of a phenomenon is to examine the work which is carried out by participants in effecting or bringing off its existence."

(ibid.: 246-7)

These implications are crucial for the analysis of a particular phenomenon. For, given that the out-there-ness of a phenomenon is accomplished in establishing its properties within a certain vernacular setting, whether the phenomenon is ontologically true or not,
or exists or not, will not be recoverable from participants’ accounts. The analytical focus, then, should be shifted onto the organisation of the presentational context in which participants make the phenomenon available to themselves in and through their accounts in order to analyse "what for participants counts as a phenomenon" (ibid.: 248).

Woolgar analyses the transcript of a Nobel Lecture in which he shows how the notion of 'discovery' is made available and practically managed through a demonstration of the interpretative procedures involved in the scientist's writing. While Woolgar's concern is to examine "the writing of scientists" (ibid.: 245), I would again like to emphasise that my focus will be on the reading. My interest is in the form of reading by which nature comes to be known for participants, such as scientists, SSKers and the Candidate herself, including the readers and author of the analysis in this thesis. I believe that it is crucial to attend to whose reading is authoritative in this kind of analysis (of the text of the original 1988 article and the participants' accounts of their reading) and in the reading of this chapter. Therefore, the work of reading needs to be examined multi-dimensionally.

Another reason for focusing on the state of reading is that this issue directly relates to the research object of this thesis — that is, the process of knowledge construction. It should be remembered that the Candidate's own state of knowledge about endothelin has been established increasingly firmly during the course of her research. That is, the more she has proceeded with the research, the more the participants establish that she knows about endothelin (see Chapter 4, p. 212-214). Moreover, during the course of research, the reading of relevant materials has not been seen as a single task. She reads scientific articles in order to be able to talk about endothelin with the scientists, while she also reads them in order to pursue an SSK.

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67 This concern is more or less shared with discourse analysts and ethnomethodologists (see Chapter 1, p.20-23, 32-35).

68 Reading itself turns out to be writing and vice versa (see footnote 10). Writing down what it is that has been read can never fully capture what it tries to write down, since what it tries to write down is established in the very reading of that writing, which will be generated as text in the very reading of that writing, and so on. The impossibility of capturing the actuality of this reading is articulated in a variety of ways (e.g., Derrida 1988; Deleuze 1990, 1994).
analysis. In other words, this raises the potential for a variety of readings, including that of knowing endothelin, that of finding out how it comes to exist, and that of investigating a way to display such readings. Whilst this might seem to be an overly complicated process, my aim is to involve you, the readers, in a multiphasic dimension of reading.

With regard to the process of knowledge construction, I believe that knowing something is closely tied with reading and writing. The image of scientists working in a laboratory without exchanging a word provides the impression that the process of knowing does not necessarily have to relate to reading and writing. But writing and talking are both prominent ways to display one’s knowledge. The construction of knowledge depends on "a complicated variety of factors, including our reading or listening to the accounts of others, our susceptibility to persuasion by authoritative sources, our willingness to credit claims to expertise, and so on" (Grint and Woolgar 1992: 371). Silent laboratory scientists are "inscribing" nature (Latour and Woolgar 1986), and it is the activity of reading and writing that makes nature available and demonstrable (Edwards, Ashmore and Potter 1995).

Reading of Scientific Materials versus Analysing the Rhetoric?

Reader Hold on! You have erased a paragraph which was previously here (and I will undo your erasure)!

[In the reading which I am now going to write, I do not want to assume that the scientists who authored the writing are only keen on the rhetoric they used while they were writing. Nor do I want to be keen only on exploring that rhetoric. I would rather, in the first place, read that writing in order to know about endothelin and how it came to be extracted.

(the first draft by Mushakoji, 19 January, 1996)

69 See footnote 66.
This claim was pointed out as problematic by our supervisor, Dr. Ashmore. Specifically, if both the scientists and 'we' are not working on rhetoric, then what is it other than rhetoric that we can generate within texts in the first place? Clearly, I was (un)convinced by this bit of supervisory advice (or rhetoric?)

Writer Well, okay, so, what's wrong with concentrating our study on the rhetoric of scientific texts? Any studies which try to demonstrate how a knowledge-claim is made, or how a certain object or phenomenon is established in a text, are thereby studies of rhetoric (e.g., Billig 1996; Edwards and Potter 1992). There are also numerous studies of rhetoric used in science (e.g. Bazerman 1981, 1988; Brown 1994a; Brown 1994b; Gusfield 1976; Myers 1990). They are exciting in their demonstration of how reality is produced in and through a text and in revealing how eloquently that text is organised. Actually, most of SSK's empirical analyses can be regarded as dealing with rhetoric in science in their analyses, even if they do not promote themselves under the rubric of 'analyses of rhetoric in science'. You, and I, are thus supposed to read things in just that way if we are (mainly) to attend to SSK analyses.

Reader However, it is an SSKer's reading that these analyses are showing. SSK's reading of scientific texts is pre-oriented to reading for the analysis of rhetoric, of how nature is established to exist in science. The crux of such studies is, thus, that of deconstructing this existence.

Writer You read those SSK analyses that way.

Reader But is their reading the only reading? I mean, do scientists read scientific materials in order to observe how rhetoric is used?

Writer Well, maybe not, or maybe not always, but SSKers may say that reading itself is a practice which endogenously involves rhetoric and readers' interpretative procedures.

Reader But in spite of this, there can be a reading of scientific articles which is not for analysing the rhetoric used in them, but for knowing what is reported therein70. I

70 If SSKers are reading scientific literature as their research material only to discover the rhetoric, and if scientists are reading scientific literature to know about their research objects, we may have to conclude that each of them is engaging in a totally different activity. However, I disagree with such an
should mention that if we single out such a reading, we would keep at bay an essential point of the reading which has an implication for SSK studies as well.

Writer Does it not mean that while we are to analyse how a certain scientific object is believed to exist, we are also to accept the existence of this object in a similar way to non-SSKers? The task you impose on us appears to be extremely difficult, if not contradictory. As an SSKer, we will show how a certain scientific object is established or believed to exist, in our analyses, and we need to keep in our minds that "to show how objectivities become objectivities is to show that they are not objectivities" (Ashmore 1989: 98-99, stress original; Woolgar 1983:253-54).

Reader Maybe. But let me point out a more interesting issue. Are SSKers indeed reading scientific materials merely in order to extract how rhetoric is used in them? That is, are SSKers reading only for the purpose of analysing how knowledge-claims are rhetorically made? Do they not need also to construct what it is that they take such texts to be claiming in the first place? Is it not the case that even for their own analytic purposes, they need to know about nature in order to start their analysis? In order to show how something is constructed, that is, in order to undertake a deconstructive work, they must first construct something to deconstruct. I suppose that in this work of construction, nobody can observe herself as 'Here I am, merely and temporarily, believing endothelin (that is, constructing an object, just for the sake of a planned deconstruction as the next step!); but that she is trying to know endothelin.

Writer Are you now talking about SSKers' knowledge?

Reader Or, to put it in more SSK-oriented language, I am talking about SSKers' display of their own knowledge. Of course, for reading scientific articles, an analytical distinction can be made between sociological analysis and other kinds of readings such as a reading for knowledge, reading for entertainment, etc. These readings are, however, more or less spontaneously intermingled. Read (or write) the following section.

understanding. As long as we can and do intermesh our discourses, and as long as we describe our observations with a language which is common to both scientists and SSKers, the borders between these research communities overlap (cf. Lynch 1993).
SSKers' Own Accounting Practices of Their Nature (or Their Cases)

A reading that aims to examine the discursive work of a text does not preclude reading with the purpose of knowing the object. Such a reading establishes what the text is about. This point can be further applied to the work of writing a sociological analysis (and, of course, writing is reading).

SSKers need to introduce scientific cases as a sociological object in the first place. That is, only by first introducing their case as such, can they analyse how a certain scientific activity is constituted, and what it is as a sociological referent. In SSK, the work of "first introducing the case as such" (and then giving it a sociological turn) occurs when nature is introduced as the research object of a certain scientific activity, as well as when this scientific activity is introduced as such. Some salient examples of the work of introducing nature include Gilbert and Mulkay's "Layman's gloss" for an original scientific text in their initial discourse analytic work (Gilbert and Mulkay 1980, 1984). Other interesting cases include Collins' expositions of the TEA-laser, gravitational radiation and parapsychology in his study of the socially negotiated nature of replication in science (Collins 1985). Let us examine their accounts of nature.

71 See footnote 10.
72 This kind of SSK work is described by Woolgar as 'doing irony'. "To do irony is to say of something that appears one way, that it is in fact something other than it appears" (Woolgar 1983: 249).

In constructivists' analyses, Woolgar identifies three kinds of move: first, a certain account is selected to be ironised; secondly, accounts quite different from this first account are asserted; thirdly, "[these] accounts are not only supposed to be different, they are taken as alternative versions of the same reality" (ibid.: 250-51, stress original). But on what ground can the constructivist assume that those accounts relate to the same reality? Woolgar sees these ironical moves as being troublesome. This kind of irony undertaken in sociological analysis -- i.e., 'instrumental irony' -- is "an instrument whereby alternative accounts are contrasted but where the business of accounting and contrasting is passed over". Thus, the irony in this case is "not only a methodological convenience" but "a way of doing sociological analysis without attending to the difficult problems involved in description and explanation" (ibid.: 258).

Instead, Woolgar proposes 'dynamic irony', which is not stable, and which has "the effect of making the reader take more seriously the deep flexibility of accounting procedures. Not only does he come to realize the variety of different ways in which descriptions can be received; he also comes to see the sense in which one can never know for sure" (ibid.: 260). With this kind of irony, "the assumption that members of a fixed recognizable community of fellow practitioners have equal access to irony" is radically withdrawn: the irony then becomes "irony as project" (ibid.: 260).

I am convinced that this latter irony contains far more joy as a project: it provides for how your reading-writing and mine proceed with each individual creative act. The joy lies in "the constant change in what the reader sees", and not in any single change used for a preconceived sociological end (ibid.: 260). For a discussion of projection in sociological analyses, see Chapter 4 (p. 195-196, 200-211).

73 How scientific activities are introduced as such will be explicated in Chapter 3, by taking up scientific discoveries.
Table 2-1 shows the first few sentences of the text of a scientific article and its "Layman's gloss", as well as Collins' descriptions of his three cases. Although their discussion soon proceeds along the lines of how the knowledge-claims are made and how they are (un-) successful, Gilbert and Mulkay, and Collins, inevitably provide some statements about nature, in order to share such knowledge with their readers.

Gilbert and Mulkay's "Layman's gloss A" is a paraphrase of "Introduction A" (the extract from the original article). In the activity of paraphrasing it, Gilbert and Mulkay are demonstrating that they can calibrate what the reader needs to "understand completely the quotation from an introduction to a research paper" in biochemistry (Gilbert and Mulkay 1981: 21), and that they themselves can translate it into a language suitable for a non-technical readership. By labelling this translation "Layman's gloss", it may well be that Gilbert and Mulkay are 'dynamically ironising' (Woolgar 1983) their own accounts of nature. The task is nevertheless one of giving their own formulation of nature (i.e., ATP) to the readers.

A similar task is undertaken in Collins' descriptions of his cases: and interestingly, Collins seems to have formulated nature rather differently in these three cases. As shown in Table 2-1, in each case nature is introduced with a radically different degree of facticity. The distinction demonstrates an asymmetry in Collins' degree of 'ontological commitment' to the scientific objects discussed. In the first case, the TEA-laser itself is articulated; in the second case, the 'story-so-far' of the pursuit of gravitational radiation is recounted; and in the third case, the events surrounding a knowledge-claim made about the responses of plants to remote stimuli are narrated as 'the story'. Notice how the account of the TEA-laser is more 'solid' than the account of gravitational radiation, and how the account of the responses of plants is even less so. This hierarchy of conviction parallels the acknowledged hierarchy of normality in
Gilbert and Mulkay 1980, 1984

**Introduction A**

A long held assumption concerning oxidative phosphorylation has been that the energy available from oxidation-reduction reactions is used to drive the formation of the terminal covalent anhydride bound in ATP. Contrary to this view, recent results from several laboratories suggest that energy is used primarily to promote the binding of ADP forms at the catalytic site from bound ADP and phosphate with little change in free energy.

A critical test of this proposal would be to measure energy-dependent changes in binding affinities at the catalytic site for adenine nucleotides.

**Layman's gloss A**

ATP is one of a class of complex molecules called nucleotides. It is biologically important because it is a major source of energy in living organisms. ATP is formed by the combination of ADP and inorganic phosphate. The overall process whereby ATP is formed is called oxidative phosphorylation.


Collins 1985

**TEA-laser**

A laser produces a beam of powerful 'coherent' radiation, often visible light, that can be focused very finely and can therefore damage the small spot upon which it impinges. The radiation is generated by putting energy into the molecules of the lasing substance -- it might be a piece of ruby, or a gas -- and then releasing all this energy in a synchronized way. The TEA-laser uses a gas as the lasting medium and produces infra-red radiation rather than visible light.

(Collins 1985: 51)

gravitational radiation

Gravitational radiation can be thought of as the invisible gravitational equivalent of light or other electromagnetic radiation (see, for example, Davies, 1980). Most scientists agree that Einstein's general theory predicts that moving massive bodies will produce gravity waves: however, they are so weak that their detection is difficult. For example,

( Ibid. : 79)

Responses of plants to remote stimuli (parapsychology)

Toward the end of the 1960s Cleve Backster, a lie detector expert in New York, attracted considerable publicity for discovering, as the newspaper put it, that plants have feelings. Lie detectors work by registering changes in the electrical conductivity of the human skin and are widely used in the United States. Becker was a sufficiently well established expert to be asked to present a statement to a congressional hearing on the subject in June 1974.

The story of his orthodox work began, as he reports, when, in an idle moment, he attached the electrodes of a lie detector...

( Ibid. : 113)
science which Collins explicates in his analysis. This asymmetry clearly implicates us in the degree of SSKers' commitment to acknowledging nature when they take up the analysis of cases in science.

When SSKers account for their analytical task, they are usually regarded as having explained, explicated, or deconstructed the knowledge-claims made in science. This task seems to be one of analysing the knowledge-claims under a particular sociological framework. This task is separated from the task of knowing about nature, which is attributed to the domain of participants (scientists or lay people), and which is SSKers' analytical focus.

The tasks of knowing an object and analysing how people construct such knowledge are separated in SSK. Most notably, such separation is the ground of methodological relativism (Bloor 1976; see also Barnes 1981). This methodological

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74 This finding can be interestingly compared with Edwards, Ashmore and Potter's explication of Collins' distinction between symbols and a rock. For Collins, while the former need to be learned and trained, the latter is an example of:

obstinate features of the natural world that provide causal constraints on our physical movement. As we stumble against a rock we do not seem to have to think about obeying its instructions. It will give us guidance about where to walk in its vicinity whether we think about it or not. We may walk beside it or away from it, but not through it. We do not have to decide not to walk through it. Our actions are caused directly by the rock rather than by our interpretations of what the rock is.

(Collins 1990: 50)

In this passage, Collins takes into consideration how rocks work within cultural frameworks. But, a rock is able to instruct "everyone equally and obtains uniform results". For Collins, "rocks are cultural universals", and different from those symbols which affect only "those who have gone through the training" (ibid.: 50).

For Edwards, Ashmore and Potter, this position is strange for an SSKer. The rock is best understood as not an actual but a prototypical one. The rock and the stumbling are offered as just the obvious, common or garden (where the rocks and trees are), bog standard, ordinary and typical sort; not the fuzzy, borderline, silly examples like Ayers rock, crumbly chalk, or diamonds. They work by deploying semantic prototypes to represent an idealized and realistic general knowledge and by having these representations masquerade as what everyone would have to agree to say about a specific event. Once we accept that Collins is offering us not reality but a short story, the only ground for accepting the story as merely real is because we are told to. Gravity waves have to be constructed as discoveries, but rocks are just there. It is a kind of trickery when writers introduce reality in the form of specific descriptions of it, and then kick away the textual ladder and ask us to consider the thus-described reality as out-there.

(Edwards, Ashmore and Potter 1995).

To take their point that Collins' introduction of nature is pursued with a trick, it may well be that Collins is best understood as a narrator who changes his ways of narrating nature in each of the above three cases of nature, rather than that he knows them differently. But I think that to narrate individual cases of nature differently is to commit oneself to knowing them differently.
relativism is also upheld in the empirical programme of relativism which Collins advocates (Collins 1983; Collins and Yearley 1992a, 1992b):

Sociologists don't know anything in quite [the same way as other people with ontological commitment do]; they only know how it is to know. The sociologist is promiscuous, experiencing many loves without ever falling in love. . . . [Though] promiscuity is not a recipe for love, it is for education. A well-educated person is not just a faithful specialist but one who knows how to take another's point of view -- even to invade another's world of knowledge.

The achievement of [SSK] can be understood as an extension of this ability to "alternate". . . . Our argument here is that social studies of science ought to erect meta-alternation as a principle, not treat it as a failing. To treat it as a failing is to invite participation in an escalation of skepticism which we liken to the game of chicken; in this case the game is epistemological chicken.

(Collins and Yearley 1992a: 301-302)

For Collins and Yearley, two approaches in SSK are problematic in terms of "epistemological escalation": reflexivity and the actor-network theory (see Chapter 1, p. 27-30, 35-38). Methodological relativism is a remedial stance for not falling into such escalation. Its proposal is to use relativism as a methodology for the study of science, while holding to the necessity of epistemological agnosticism as an analysts' resource. An SSKers' task is to demonstrate how reality is constructed in scientists' justificatory procedures and negotiations. Thus, this relativist stance only applies to 'nature' -- that is, the reality for scientists. In the world of SSK, on the other hand, 'scientists' activity of constructing nature' is the reality.

This stance whereby one's ascription to relativism serves to separate oneself from the rest is criticised by Woolgar, as one of two "insensitivities":

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75 This excerpt is taken from the article which raised "the so-called chicken debate" (Pickering 1995: 10-14). The epistemological escalation is illustrated as the game of "chicken":

The game of "chicken" involves dashing across the road in front of speeding cars. The object of the game is to be the last person to cross. Only this person can avoid the charge of being cowardly. An early crosser is a "chicken" (noun), that is, a person who is "chicken" (adjective).

(Collins and Yearley 1992a: 301)

76 This position is critically termed "social realism" by Callon and Latour (1992). It is also critically described by Evelleen Richards as "the methodological and political necessity of compartmentalization" (1996: 328).
First we are told that the cosmic status of SSK derives from the fact that its practitioners do not know in the same way that other disciplines know. . . . Sociologists are said to be the only beings capable of seeing things from more than one point of view. (ibid.: 330)

Woolgar points out that to presume any structured reality of what it is to know in the discipline of sociology (and other disciplines) is not only irrelevant but erroneous, as any version of such a structured reality is the "(occasioned) products of [sociologists'] own efforts at knowing" (ibid.: 331). For Woolgar, sociologists are not authorities who can dominate through their knowledge of other people's knowledge and their ways of knowing things. Thus, the second "insensitivity":

follows from this glib characterization of the world in terms of different ways of knowing. Given that there are different epistemological natural attitudes, [Collins and Yearley] say we are free to use whatever version is appropriate for the purpose at hand. But once we recognize the constitutive function of language, the strength of the argument that we are immersed in our language games, this idea of freedom of choice is laughable. The notion that we should then decide (how) to choose an epistemological stance is ludicrous. (ibid.: 331)

As we are not free from "the constraints of conventions of language" (ibid.: 331), Woolgar claims that one cannot possess "different ways of knowing" and have control over them.

This point seems to be sustained throughout Woolgar's explication of sociologists' explanations of other people's accounts. For Woolgar, this explanatory work unavoidably engages in 'ironising' other people's versions of reality (Woolgar 1983). Furthermore, the process of transforming their object of reference into a

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77 See footnote 72.
78 Sociologists' activity of 'ironising' is, when it is instrumental, valued negatively by Woolgar. In contrast, what Collins and Yearley describe as 'alternation' is a notion close to Woolgar's 'ironising' but is valued positively by them. The latter notion is borrowed from Peter Berger's Invitation to Sociology (1966). It is the sociologists' "ability to switch between different frames of reference" (Collins and Yearley 1992: 301).
sociological object involves 'ontological gerrymandering'\textsuperscript{79} (Woolgar and Pawluch 1985). Moreover, for Woolgar, as well as for Ashmore, one who \textit{analyses} other people's versions of reality cannot at the same time \textit{preserve} such versions. If sociologists assume that they can engage in these two ways of knowing simultaneously, this is "disingenuous" (Woolgar 1981, 1983): "To show how objectivities \textit{become} objectivities is to show that they are \textit{not} objectivities" (Ashmore 1989: 98-99, stress original): Analysis is not separable from criticism (\textit{ibid.}: 178).

However, as we have seen, the task of analysing scientists' accounts of nature goes hand in hand with SSKers' own accounts of this very nature. Examining (and reading) some SSKer's accounts of nature and their asymmetrical commitment to such cases, the SSKers' task of analysing knowledge-claims about nature in scientific texts seems to be inseparable from the task of knowing nature (and disseminating this knowledge to their readers). I therefore find it difficult to see SSK analyses as 'purely' analytical. In other words, in the analytical process, one may still know the object -- which is to be analysed -- in the way that 'other' people do\textsuperscript{80}.

\textsuperscript{79} See footnote 45.

\textsuperscript{80} For the case of the Candidate's research, it seems that she has been engaging in \textit{knowing} (as an ordinary member of society) and \textit{analysing} (as a candidate SSKer) her research object. Although she has not upheld an imperative separation between these two tasks and has believed that she has control over the choice between different ways of knowing, she does look as though she has different ways of knowing. At this point, then, her \textit{position} -- which is \textit{non-positionism} -- has resulted in a departure from that advocated by reflexivists such as Ashmore and Woolgar: the Candidate appears to be "promiscuous" (Collins and Yearley 1992a: 301): a promiscuity that in this case is not a recipe for education -- a consequence of love? (cf. \textit{ibid.}: 301-2, see also p. 69).

I think that the departure comes from the nature of her research object -- the process of knowledge construction, including her own. In this process, she experienced \textit{a series of loves}: the latest love seems not a brand new one happening once and for all (although it sometimes seems to be so). It is enriched with the intertextuality (see footnote 82) of love. In other words, it is not that she can choose ways of knowing things, but she seems to know things, including the alternatives -- \textit{knowing relatively} (See footnote 55).

I believe that it is 'to know relatively' that animates the Candidate's research, and love of the \textit{primacy of denial, and of argument} (Edwards, Ashmore and Potter 1995: 42). As Edwards, Ashmore and Potter claim, relativism is \textit{not}, itself, a positive statement; but then:

\begin{quote}
\textit{[a]ll} positive statements orientate to the otherwise possible nature of things. Assertions are denials and \textit{vice versa}. Denial is not an upsetting of reality, a 'things fall apart' threat to any possibility of order, but an essential feature of knowledge's orderliness. (\textit{ibid.}: 42)
\end{quote}

They present this kind of relativism as "a non-position, as critique or scepticism, not as a positive statement opposed to realism" (\textit{ibid.}: 41). It can include and analyse realism and relativism alike, viewed as \textit{rhetorical practices}.  

71
Okay. So, you are proceeding further with this muddling of your analysis. But there is also another obstacle here! Your reading is distinct from that of readers who engage the original 1988 article without taking up this text.

My Analysis Involves Readings But Captures Them

So, far, I have suggested that analysing how knowledge-claims about endothelin are made involves reading the scientific materials and knowing endothelin. However, in undertaking an analysis of scientific texts, even at the very outset, what is agreed upon regarding the reading of scientific materials for analysis, and what we will agree upon regarding the readings of this chapter are undecided.\(^{81}\)

As regards the reading for an analysis, to display a certain reading of scientific materials is inevitable but intractable. I understand that each reading is locally situated, but it is not enough to confess that the reading is done for a certain analytical purpose and is thus a situated achievement. It is also always becoming different in an ongoing and multiphasic fashion. That is, whatever the purpose of one's reading is claimed to be, a text does not just arrive at one's empty hand. There must be an enormous amount of things achieved before a reader can take it up: we have already done a great deal of work in order to bring it in front of us to read. We generate the text which we have so far acquired with all its intertextuality.\(^{82}\)

The most immediate criticism against SSKers' reading of scientific materials may be that their reading is accomplished without enough competence. In other words, their reading is different from that of the scientists who are experts in the relevant research areas. In making a distinction between analysis which involves such reading and the ethnomethodological framework, Lynch states that for the latter, texts are to be

\(^{81}\) And whether we can agree upon invoking such an agreement in the first place, is undecided.

\(^{82}\) Intertextuality is defined by Julia Kristeva as "the transposition of one (or several) sign system(s) into another" (Kristeva 1984: 59-60) which involves the destruction of the old position and the formation of a new one.

If we grant that every signifying practice is a field of transpositions of various signifying systems (an inter-textuality), one then understands that its "place" of enunciation and its denoted "object" are never single, complete, and identical to themselves, but always plural, shattered, capable of being tabulated. (Kristeva 1984: 60)
analyzed for how they are employed within occupationally specific competencies of "reading" and "writing." [The ethnomethodological 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 use of the texts within the specific occupational activities which produced them. (Lynch 1985: 8)

However, the analytical task of not isolating the texts from the practical state of affairs in science and still analysing them as an adequately performed reading by the scientists, is almost impossible (even if a sociologist somehow becomes an incumbent member of science). In fact, in describing laboratory work as a sociologist, Lynch is also aware of his own "disengagement" from "an embodied access to the practices of scientific work" (ibid.: 280).

As Lynch claims, it may well be that the scientists' own way of reading and writing, is "disengaged" from the way of reading that will be taken by, say, SSK readers. However, it appears that even among endothelin researchers who share "occupationally specific competencies" (Lynch 1985: 8), there might not be a single reading. As the following extracts 2A and 2B show, what is accomplished in the reading is described in a radically different manner:

2A
517. Mushakoji On first being published, people all over the world read it [and] I suppose
518. Prof. Goto [Yeah.]
519. Mushakoji there must have been various responses [[to it and]] eh-eh-which of the
520. Prof. Goto [[Yeah. That's right.]]
521. Mushakoji responses was for you, sensei ((honorific indicating higher academic status than that of speaker)), the most directly-impressive-I mean, with regard to
522. the discovery, what sort of responses- from whom and in what way were they
523. uh-
524.
525. Prof. Goto Among others, what surprised us most was, you see, eh-there is someone
526. named John Vane in England, the guy who won the Nobel Prize [for]
527. Mushakoji [Yes.]
528. Prof. Goto prostaglandin,

83 For example, we can assume that it is highly possible that the latter are not familiar with the authors of the original 1988 article and their affiliations. Readers may perhaps infer a certain image from such terms as 'Tsukuba, Japan', etc. and may be familiar with the significance that the journal Nature holds in some circles (in various ways); etc. But it may still be said that the relevant reading can be taken only inside the scientific community.
In about mid-April 1988, a colleague informed me that there was an article in a recent issue of *Nature* (March 31, 1988) by a large group of investigators from Japan that I *might* be interested in. Later that evening I read the paper: "A novel potent vasoconstrictor peptide produced by vascular endothelial cells."

Although the title was simple enough, the content of the article was overwhelming. (Highsmith 1992: 29; stress original)

In the first extract (2A), Prof. Goto's account describes how other scientists came to know about endothelin by reading the original 1988 article. The story that this article immediately provided the impetus for the other scientists *to obtain endothelin* informs us that endothelin had been unknown to them, and was immediately recognised as an existent object which was highly relevant for their investigations. The second extract (2B) is from a historical review written by an American scientist who is not a member of the Tsukuba Group but whose work is particularly close to that reported in the original 1988 article (See Chapter 3). This scientist's account indicates that a reading immediately evoked the establishment of endothelin and the significance of the Tsukuba Group's report.
In this way, I claim that my analysis inevitably includes the reading of scientific materials and knowing endothelin and the related scientific activities, but I need to remind you that this reading is not the one pursued *in situ* by any (other) scientists. The texts of scientific materials are solely *your reading*.

Reading Rhetoric

*Writer*  
Okay. Before ending this section, let me add one more thing to your remarks on rhetoric! Not only SSKers, but also scientists themselves raise the rhetorical elements in discourse as noteworthy. They are aware of the rhetoric in their discourses. Take a look at how the very scientists who are the authors of the article and interview talk address this issue:

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2C  
21. Prof. Goto  
You know it certainly is easy to make up a story eh- after the event, but in reality we did not begin with such an admirable idea to start out with. By happenstance, eh- there was a paper ((refers to an unspecified research paper)) 84 which revealed something interesting. We were like "okay, then", so, "let's just go ahead and isolate (in English)- isolate and purify it". We were all, "come on, let's identify this object". At the beginning, it was really like that.  
(Prof. Goto 14 March, 1994)

2D  
215. Dr. H. Kurihara  
Yeah, right. It's really like- When you look at it from uh- the outside, it seems that these things have proceeded [in a uh-] smooth way-  
217. Mushakoji  
[Yes. ]

---

84 The paper is Hickey, Rubanyi, Paul and Highsmith (1985) which is relevant for the next chapter with respect to the attribution of scientific discoveries. At this point of the interview, however, the paper Prof. Goto referred to was not identified by Mushakoji. It was only later that Mushakoji could guess which paper he was talking about by conglomerating other accounts obtained by a variety of researchers. This guess was confirmed with Prof. Goto retrospectively. For the 'chronological' trajectory of the Candidate's research, see Chapter 3, p. 116. For the retrospective angle in the knowledge construction of her research, see Chapter 4, p. 212-214.
when we look back in retrospect, we can then finally [start] seeing things
[Um.]

[[very much]] like "oh, now I see, it had been this kind of a story all along."

[[Oh.]]

(Dr. Y. Kurihara and Dr. H. Kurihara 8 November, 1995)

Writer  You see? They are talking about the contingent nature of their accounts. They are aware that this nature cannot be avoided. So, as I said, I am not wrong to see their texts as rhetorical. Fair enough?

Reader  Okay. But you are now reading what these scientists inform us, and not analysing the way they rhetorically construct the issue.

Writer  Oh, well, yeah. Of course, I should start analysing how they here assert a certain version of reality by claiming, "but in reality" (2C, line 21-22).

Reader  Moreover, there is a self-referential question here: are you claiming that we ourselves have so far merely used rhetoric in our arguments, "but in reality" what is taking place in our analysis is something else?

Writer  Oh, well . . . but I still hope that readers will read this writing in order to know our point regarding reading and analysing, and will not merely analyse our rhetorical moves.

Reader  (Oh, rhetoric!)


In a 'serious' journal, . . . readers are asked to orient to a range of vetting procedures which they could imagine having taken place before the story was allowed to appear in its current setting. Tacitly, the judgement of (presumably expert and hence reliable) others has preceded the reader's own assessment of the authority of the text. Before even beginning to read the text, the reader is faced with the choice of concurring with this expert evaluation or dismissing its authority out of hand.  

(Woolgar 1980: 251)
Sequential Paraphrasing and the Naming of Endothelin

Now, please read the first page of the original 1988 article (Fig. 2-1). Its title is:

2E

   (Yanagisawa et. al. 1988: 411)

The title can be recognised as referring to "a novel potent vasoconstrictor peptide" and thus as indicating its existence. This recognition is relevant at the very start of reading the first page, and is underwritten by "the setting" which establishes it as a claim to be taken seriously (Woolgar 1980: 251).

Oh. Okay, seriously, and then . . .

2F

2. An endothelium-derived 21-residue vasoconstrictor peptide, endothelin, has been isolated, and shown to be one of the most potent vasoconstrictors known.
3. Cloning and sequencing of preproendothelin complementary DNA shows that mature endothelin is generated through an unusual proteolytic processing, and regional homologies to a group of neurotoxins suggest that endothelin is an endogenous modulator of voltage-dependent ion channels. Expression of the endothelin gene is regulated by several vasoactive agents, indicating the existence of a novel cardiovascular control system.
   (Yanagisawa et. al. 1988: 411)

Comparing the above two texts (i.e., those of the title [line 1] and abstract [lines 2-9]), we can observe that what has been referred to in the title (i.e. "a novel potent vasoconstrictor peptide") is to be related to "[a]n endothelium-derived 21-residue vasoconstrictor peptide" in the abstract (line 2). This latter phrase "[a]n endothelium-derived. . ." can be recognised as referring to what has already been referred to by the phrase, "[a] novel . . ." in the title. Thus, "[a] novel . . . peptide" can be read as a paraphrased version of "[a]n endothelium-derived . . . peptide". This recognition seems
A novel potent vasoconstrictor peptide produced by vascular endothelial cells

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An endothelin-derived 21-residue vasoconstrictor peptide, endothelin, has been isolated, and shown to be one of the most potent vasoconstrictors known. Cloning and sequencing of preproendothelin complementary DNA that mature endothelin is generated through an unusual proteolytic processing, and regional homologies to a group of neurotoxins suggest that endothelin is an endogenous modulator of polypeptide ion channels. Expression of the endothelin gene is regulated by several vasoactive agents, indicating the existence of a novel cardiovascular control system.

Following the discovery in 1980 of endothelin-dependent vasodilation by Furchgott and Zawadzki1, vascular endothelium has been recognized as an important functional unit involved in the regulation of vascular smooth muscle tonus. It has been hypothesized that, when stimulated by vasoactive agents such as norepinephrine, thrombin and hypoxia, as well as to a closely related substance, vasodilation by Furchgott and Zawadzki as nitric oxide or a closely related factor(s) which mediate these constrictive responses. Endothelin is generated through an unusual proteolytic processing, and regional homologies to a group of neurotoxins suggest that endothelin is an endogenous modulator of polypeptide ion channels. Expression of the endothelin gene is regulated by several vasoactive agents, indicating the existence of a novel cardiovascular control system.

However, shows local homologies to a certain group of peptide neurotoxins that act on voltage-dependent Na+ channels, suggesting that endothelin also acts directly on membrane channels. Studies with preproendothelin cDNA reveal that the precursor peptide is proteolytically processed in an unusual way and that its biosynthesis in cultured EC is regulated at the transcriptional level in response to various chemical and mechanical stimuli.

Purification and structure

The supernatant from confluent monolayer cultures of porcine aortic EC causes an endothelium-independent, slow-onset contraction when added to porcine coronary artery strips at a final concentration of 10% v/v or more (data not shown). Unconditioned medium, or medium conditioned with human fibroblast IMR-90 cells, has no effect. Pretreatment of the conditioned medium with 1 μg/ml trypsin at 37°C for 2 h abolishes the endothelin activity. The activity is also found in the serum-free conditioned medium, and no significant decrease of the activity was observed after a long-term (4-5 weeks) serum-free maintenance of the EC culture. These observations indicate that endothelin is not a derivative of a serum component.

That the confluent EC monolayer can be maintained for several weeks even in protein-free culture medium greatly facilited its purification. We purified the vasorculin from concentrated serum-free conditioned medium, by collecting active fractions after anion-exchange chromatography and two steps of reversed-phase HPLC (Fig. 1a-c). The final endothelin fraction, corresponding to the absorbance peak at the arrow in Fig. 1b, was eluted as a single peak on analytical anion-exchange and reversed-phase HPLCs. In one of the experiments, 2.9 nmol purified endothelin was obtained from 9.61 of medium conditioned for 5 days with approximately 4 x 10^7 cells. The amino-acid composition, determined by acid hydrolysis, was: Asx, 2.05; Ser, 2.76; Gln, 1.08; Cys, 1.37; (as cysteine) Val,
to be the most crucial activity for the establishment of endothelin as an existing substance in the reading. Here, the paraphrase is produced in the manner that Lynch has observed across utterances: that is, "successively modified assertions 'somehow' retain their referent" (Lynch 1985: 267). The way the 'identity' of reference is perceived, is described as follows:

[T]he accountable character of the object changes over the course of the sequence of reassertions. The "identity" of reference is located with interior (though largely unspecified) accomplishments of successive assertions. That is, it is an accomplishment of the production of serial assertions that allows one to hear these assertions as referring to the "same thing." (Lynch 1985: 267-268)

I observe that such identification also appears relevant in the reading of extract 2E and 2F. I will refer to this function as *sequential paraphrasing*. In the above case, such sequential paraphrasing involves movement from "peptide", the existence of which has been established in the title, to "peptide" that one sequentially comes to know as a topic through successive formulations. The task involved in this reading may be one of recognising the successive phrases as the development of a topic in a series of paraphrases upon the same object, "peptide", which thereby comes to be represented differently. In this way, the reading ongoingly establishes the intratextuality in its movement of what is initially represented as taking place throughout the series it develops in the reading.

Immediately following the reference to the "peptide" in the abstract, this "peptide" comes in turn to be paraphrased by being *named* "endothelin" (line 2) (Fig. 2-2). What is referred to by the first two "peptide[s]" is here perceived as having been given a name. The word "endothelin" is perceived as a name which refers to the same object that has already been referred to as a topic. This is the very point, at which the *naming of endothelin* takes place. I consider this act of naming to be a crucial stage in reading about the initial reporting of endothelin. The referent thereby acquires a proper name, which lets us refer to a particular object more precisely with a proper name. In
An endothelium-derived 21-residue vasoconstrictor peptide, (endothelin), has been isolated.

(Yanagisawa et al., 1988: 411, stress and parenthetical notation added)

Fig. 2-2. The act of naming endothelin
short, the reading has proceeded, first of all, with the sequential paraphrasing in which what is established as a topic develops sequentially through a series of paraphrases. Then, after the topic is given a name, this sequential paraphrasing proceeds in such a way as to unambiguously identify the object and specify 'it'.

This way of recognising the sequential paraphrasing and the act of naming, as providing the movement of the sameness (of a research object) through their differences (in its representations), gives us some understanding of how it is that endothelin is constituted in the text. Although one certainly needs all sorts of competences, including reading English and being able to invoke what is 'semiotically' represented in such an academic text, I assume that this trajectory of sequential paraphrasing and naming in the reading are available independently from any professional knowledge of biochemistry and basic medical science.

Reading Triple Text

There is a condition that makes the above sequence of reading available for us, in a linear fashion proceeding from the title to the abstract. On re-examining the front page of this article, however, one's reading of the text may not take place in quite such a linear sequential progression. Rather, the reading is more likely to occur in such a way that the front page is considered in its entirety. That is, what we see in Fig. 2-1 is a 'global' view where the title and abstract are seen as encapsulated together.

This overview takes in both the bold typed title and the first few words in the corner of the abstract. The phrase "A novel . . . peptide" in the bold typed title and "An endothelium-derived . . . peptide" in the italicised abstract stand out and resonate with one another in the running of one's glance to the bottom of the page. Or, if one views the top two or three lines of the abstract together in parallel with the title, it may well be that both what the title is about and what the (first few lines of) the abstract are about will immediately be established as referring to the same object. With this global view of the title-and-abstract, the reading can proceed not word by word, but chunk-of-words by chunk-of-words.
Prior to this subsection, we have discussed the movement of reading as a matter of the sequential paraphrasing of words and phrases. It is possible that this movement-as-paraphrasing also takes place between texts. If our reading involves such processes as are stipulated in style manuals (e.g. Style Manual Committee, Council of Biology Editors 1994: 580-591), then the texts of the title and that of the abstract are anticipated to be 'summarised versions' of the text of a full-length article. In other words, the texts of title, abstract, and full-length article are regarded as having the same content, written in different formats with different lengths.

This triple paraphrase is understood as the sameness in different texts. This kind of sameness identified between different texts has been explored in studies that focus on the relation between headings and their subsequent texts (McHoul 1982; Woolgar 1980: 251-252). These studies show that in the very process of reading, the recognition of sameness between headings and bodies of full-text is achieved in situ and is based on the condition that headings are simultaneously recognised as constituting a summary of their succeeding texts. Such a reading is experimentally examined and reported by Alex McHoul in his ethnographic reading of newspaper headline and article:

We can also say, upon inspection of the ethnography, that the headline is a [preliminary guide] ... to the article. It is 'preliminary' in that it falls sequentially prior to the article and it is a 'guide' in that it gives (or will be seen to have given) clues as to 'what the article is about'. But it can, and hence the cautious parentheses, only after this article has been read and, further, read as the 'key' to the headline. Strangely here, 'preliminary' is a matter of gradual and for-the-future decidability. With respect to the headline, then, our topicalised object becomes its [becoming a preliminary guide in reading]. (McHoul 1982: 124)

The sameness between different texts is an achievement in the reading. Its establishment is suggested to have its basis in how "[t]extual items can be read as connected or tied together if they can be found to belong to a common category or collection of items" (Woolgar 1980; 251-252). With such texts as titles, abstracts, summaries, synopses, digests, etc., the reading process is one that involves discursively
resolving the adequacy of the texts' 'claim to be the same' in their particular contexts (Edwards and Potter 1992: 37 and 39). Furthermore, given our reading that such different texts represent the same object or phenomenon, the sameness of what is represented in these different texts is constituted between them in such a reading. This is thus a reflexive process, as the establishment of the sameness in different texts takes place in the reading, and conversely, the reading itself can proceed precisely to make this establishment available.

In this way, we may gradually and reflexively constitute the recognition of the title as a name and the abstract as a summary of the full-length article as a whole. Such a constitution then establishes the sameness of what is represented in these texts: in other words, these three texts may be read as a triple text of 'what is reported in the article'. We can make sense that each text is reporting endothelin. Conversely, each text seems to make what is reported, endothelin, as foreseeably established in our reading, when the triple is recognised as holding the same content.

**Reflecting the Sameness-and-Differences**

**Reader** Is this really what takes place in your reading? I'm still not clear about the state of the reading you're presenting in the above section. If you are to analyse a text, is your task not to problematise the sameness of the triple text more radically? Is it not that your analytical focus is to show how the different texts are actually constructed as the same by ordinary readers? In other words, you need to draw the sharpest contrast between the reading of ordinary people and that of an SSK candidate. But in doing so, your presentation appears to have resulted in not successfully problematising the existence of endothelin.

**Writer** I am ready to admit that. Whilst analysing those scientific materials, I am also reading them to know what endothelin is. Even the analyst cannot help doing what ordinary people do.
Reader But the opposite is also true! Your reading cannot escape from being analytic. For example, in ordinary reading, how to read the title, abstract and full-length article is "a matter for the participants themselves to resolve" (Edwards and Potter 1992: 37-39). The sameness of the triple text pointed out in the above section is analytic, and that is not what people ordinarily pay attention to in their reading. An attempt to describe the reading of ordinary people can never be the same as the reading they actually pursue (Woolgar 1983: 253-54; Ashmore 1989: 98-99).

Writer I take your point. Whilst claiming to show ordinary reading, the work of showing it is itself inevitably analytic. It is true that I am actually reading for the analysis of non-analytical reading, which is paradoxical. But I still hope that my analytical work will be read in the ordinary, as well as the analytic, manner. For example, I hope that your reading of the above section has itself been an ordinary one, taking in such a device as its heading. Are you aware of that?

Reader Yes. I am very aware of that self-referential aspect. It is your own presentation of 'the sameness in different texts'.

Writer Thank you for clarifying that. I was hoping to attend to the 'next turn' in the reading of our analysis.

Reader Still, you cannot be sure that the reading will proceed with dynamic irony! It is a contradiction both to presume the way your analysis will be read, and at the same time to create an opportunity for dynamic irony. Dynamic irony renders any reading insecure, and so you cannot assume how your analysis will be read. Indeed, as Latour claims, the readers "seem to be much more devious, much harder to take in, much cleverer at deconstruction, much faster in fiction-making than is assumed by those writers who, with some arrogance, believe that others believe" (Latour 1988: 168).

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85 Reflexivists in SSK propose "celebrating" (Mulkay 1985, Ashmore 1989) their own constructive work. The opportunity for such celebration can be taken in what MacMillan terms a 'spiralling' next turn (MacMillan 1996: 25-27, see footnote 14).

86 See footnote 72.
Okay. But I'm not sure whether you should believe Latour in your reading...

Configuring the Work of Initial Reporting

So far, we have observed that intratextual relationships such as sequential paraphrasing, naming, and the sameness constituted in the triple text, are significant in the reading of the original 1988 article. Let us now start reading the introductory part of the full-length article:

2G
10. FOLLOWING the discovery in 1980 of endothelium-dependent vasodilation by Furchgott and Zawadzki, vascular endothelium has been recognised as an important functional unit involved in the regulation of vascular smooth muscle tonus. . . . Vasconstriction dependent on or enhanced by intact endothelium has also been observed in response to various chemical and physical stimuli such as . . . Augmentation of noradrenaline-induced vasoconstriction by anoxia$^{5,10}$ and neuropeptide Y 11 has also been found to be endothelin-dependent. Diffusible factor(s) which mediate these constrictive reactions have yet to be identified. Recent reports have described a protease-sensitive vasoconstrictor activity in supernatants of cultured EC 12-14. This activity is . . . (Yanagisawa et al. 1988: 411)

This is the very beginning of the introductory part. In lines 21-22, the phrase "a protease-sensitive vasoconstrictor activity" refers to something which has already been reported in other scientists' papers published beforehand. What it refers to has some relation to "a novel potent vasoconstrictor peptide" (line 1) as written in the title. But it is clearly distinguished from this "novel . . . peptide". The term "[f]ollowing" (line 10), in the first sentence, and the past perfect tense in this whole paragraph, can be read to indicate the potentiality of what is going to be reported from now on, and to connect this potentiality with that which has been reported in the previous papers, which are 'yet to become' the background of the reporting. The specifics of this background then
sequentially appear: "[v]asoconstriction" (line 13), "[a]ugmentation" (line 15), "[d]iffusible factor(s)" (lines 16-17), and so on.

We can observe that a connection is further made between the phenomena which have already been reported in these other scientific papers, and that which will be reported here: that is, "a novel potent vasoconstictor peptide" (line 1) in the title, and/or "[a]n endothelium-derived 21-residue vasoconstictor peptide, endothelin" (line 2) in the abstract.

The connection seems to establish the grounds on which the original 1988 article is recognised as being the initial report of endothelin. Again, we can consider this establishment, in terms of the sameness-and-differences in the reading. When the "novel" peptide is reported, it is reported in relation to things which have already been established. In order to have an idea of what kind of thing endothelin is, endothelin needs to be reported in this relational way. At the same time, endothelin must be different from these other phenomena in order to be newly reported. The difference must be of sufficient significance to render the initial-ness of reporting intelligible. In other words, in the reading, something initial must appear as different from what are claimed to exist before, yet must still appear similar enough to them to be recognised as 'a new one'. This form of sameness-and-difference in the recognition of claimed originals is pointed out by Ashmore as follows:

The perception of originality is the perception of a form of difference, constructed . . . on the basis of a constructed similarity. For something to be seen as completely original would require that it was entirely dissimilar to anything -- which would mean that it would be totally unrecognisable as anything. On the other hand, nothing is ever wholly unoriginal; that would require it to be absolutely identical to something else -- which is literally impossible.

(Ashmore 1989: 213)

Ashmore conjectures that "perception of similarity and difference" is "at the root of the construction of all knowledge" (ibid.). Given that the construction of scientific knowledge is intrinsically grounded in our accounts of nature and the way these
accounts interactively establish the facticity of knowledge-claims, whether someone's account is an 'original' or a 'copy' is an achievement of negotiation. Likewise, the question of whether (any accounts of) nature are rational, empirically adequate, or intelligible for the members of society is a consequence of a negotiating procedure (Collins 1975). Here, the grounds of such a negotiation lies in the recognition of sameness-and-difference: moreover, the recognition of sameness depends upon that of difference, and vice versa. It is thus that the work of calibration between 'being the same as' and 'being different from' what is known can be seen to take place in the reading of text 2G. Given that the negotiation of such sameness-and-differences is successfully managed, the paragraph presents an object similar to and different from endothelin, which is then newly introduced:

2H
20. We have now isolated a potent vasoconstrictor peptide from the culture supernatant of porcine aortic EC, determined its amino-acid sequence, and molecularly cloned the peptide precursor. This peptide, endothelin, does not belong to any previously known peptide family.

What is referred to as "a potent vasoconstrictor peptide" (line 20) is to be worked up as the same object as those of the "peptide" and "endothelin" that have already appeared in the title and abstract, but also to be evidently different from that referred to as "a protease-sensitive vasoconstrictor activity" (line 18-19). In the next sentence, what is referred to in the former set of phrases is again paraphrased in a clear manner as "this peptide", whereupon the naming of 'endothelin' again takes place (lines 22-23).

In this way, it seems that the initial report of endothelin is configured, at this point, with an intertextuality produced in the reading of this article.
A section following the introductory text is entitled "[p]urification and structure". It is a detailed exposition of how endothelin is isolated, with a figure (Fig. 1, reproduced in page 90, Fig. 2-3) which illustrates a laboratory experiment in which endothelin is extracted using a method called HPLC.  

The supematant from confluent monolayer cultures of porcine aortic EC causes an endothelium-independent, slow-onset contraction when added to porcine coronary artery strips at a final concentration of 10% v/v or more (data not shown). . . . Pretreatment of the conditioned medium with 1 μg ml-1 trypsin at 37°C for 2h abolishes the endothelin activity. The activity is also found . . . These observations indicate that endothelin is not a derivative of a serum component. (Yanagisawa 1988: 411)

The phrase "slow-onset contraction" (line 25) tells us that the experiment resulted in "inscribing" something which is measurable. It is then paraphrased as "the endothelin activity" (line 28), further followed by "[t]he activity" (line 28). Finally, the sentence "[t]hese observations indicate that endothelin is not a derivative of a serum component" (lines 29-30) can be read as indicating that the experiment was conducted and consequently endothelin, which is the topic sequentially paraphrased from the previous texts, was successfully extracted.

What is this movement from "contraction" (which indicates an experimental measurement), to "the activity" (which refers to the endothelin observable in the experiment), and then to the "endothelin" (which is stipulated as an object)? It is a sequential paraphrasing which results in a connection between (a) what was measured with detailed experimental procedures in the laboratory setting, (b) some function which began to unfold itself in the laboratory experiment, and (c) what has already been topicalised as an existing object, a peptide named 'endothelin'. This connection makes . . .

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87 Have you noticed that I have begun to paraphrase the text of the original 1988 article myself, immediately following my analysis? I realised that if I wish to write my analysis there is no escape from paraphrasing the texts and participating in the practice of producing an additional series of texts. Since I have made up my mind that I will still want to share the interesting issues raised in this analysis with you (if you are happy with that), I will continue paraphrasing.  
88 See footnote 66.  

88
endothelin intelligible as an element in the interwoven network of the experimental setting, as a part of nature, and as a categorical substance in biochemistry. Notice that the name "endothelin" has now been far more enriched than since we first started reading this section.\textsuperscript{89, 90}

This enrichment of endothelin by virtue of sequential paraphrasing now advances further as the reading interweaves all the different paraphrasable expressions of the same object.

In this second paragraph, the two sentences sequentially continue from "([w]e purified) the vasoconstrictor" (line 31-32) to "[t]he final endothelin fraction" (line 34). Here, endothelin is also sequentially paraphrased as what is displayed in Fig. 1, which can be seen on the next page (Fig. 2-3).

The movement from "[t]he final endothelin fraction" (line 34) to Fig. 1c -- following what is indicated by "corresponding to the absorbance peak at the arrow in Fig. 1c" (lines 34-35) -- is a particularly significant paraphrase in the text. There is not only a movement from this phrase to the arrow indicated in Fig. 1c, but also from what is referred to by the authors as what was observed in their laboratory. Any competent

\textsuperscript{89} And here as well\textsuperscript{90}.
\textsuperscript{90} And here as well\textsuperscript{89}. 

(Yanagisawa et al. 1988: 411-412)
Fig. 1 Purification and sequence analysis of porcine endothelin. α, Anion-exchange chromatogram of concentrated serum-free EC-conditioned medium. Concentrated medium (20 ml) was directly loaded onto a DEAE-Toyopearl 650 M column (2.2 x 20 cm; Tosoh) connected to a Beckman model 342 HPLC system and equilibrated with 20 mM Tris-HCl (pH 7.0), and a linear gradient of NaCl (broken lines) was applied at a flow rate of 4 ml min⁻¹. The eluate absorbance at 210 and 280 nm was recorded and the active fraction designated by a solid bar was collected. β, Reverse-phase HPLC. The active fraction from the anion-exchange chromatography was directly applied on a Unisil-Q C₁₈ column (7.6 x 250 mm; Gasukuro-Kogyo) equilibrated with 0.1% trifluoroacetic acid (TFA). A gradient of acetonitrile was used at a flow rate of 3 ml min⁻¹. Endothelin activity was col­luded with the absorbance peak designated with an arrow. c, Second reverse-phase HPLC. The active fraction in b was further purified on a Chemicosorb S-ODS-H column (4.6 x 250 mm; Chemco), with a gradient of acetonitrile at a flow rate of 1 ml min⁻¹ in the presence of 0.1% TFA. Arrow, final endothelin fraction. d, Peptide sequence analysis. The yield of phenylthiohydantoin (PTH)-amino acids at each cycle of Edman degradation is shown with the one-letter amino acid notation. Methods Endothelial cells (EC) were isolated from adult porcine thoracic aor­ta by a confluent monolayer in Eagle's minimum, essential medium (MEM), containing 10% horse serum at 37°C in 5% CO₂/95% air. Cells were identified by typical 'cobble-stone' morphology and by immuno­fluorescence to factor VIII antigen. Smooth-muscle cell contamination was less than 0.1%. After passing 10-15 times, cells were grown to confluence in a multi-layered tissue culture flask on a surface area of 12,000 cm² (Cellfactio, Nunc). Cells were washed twice with phosphate buffered saline and fed with 3.21 of serum-free MEM. Medium was changed every 5 days and the conditioned medium was pooled and stored at -20°C. Pooled conditioned medium was centrifuged at 1,000 g for 30 min, the supernatant loaded on a 3 x 18 cm C₁₈ reverse-phase column (SP-C-ODS, Chemco) equilibrated with 0.1% TFA, and adsorbed material eluted with 30 ml 0.1% TFA/70% acetonitrile. Eluate was extracted twice with 200 ml diethyl ether and the aqueous phase adjusted to pH 7.0 with Tris base. For bioassay of endothelin activity, dilutions of conditioned medium or the HPLC fractions were added directly into the muscle chamber (see Fig. 2 legend). For purification of endothelin, peptide was reduced in a solution of 0.2 M N-methylmorpholine acetate (pH 8.0), 20 mM 2-mercaptoethanol at 22°C for 30 min. The reaction was incubated at 22°C for a further 90 min after adding 4-vinylpyridine to 40 mM. The pyridylthylated endothelin was purified on a C₁₈ reverse-phase HPLC and applied to a gas-phase protein sequencer (Model 470A/120A, Applied Biosystems).

0.91; Met, 0.90; Ile, 1.03; Leu, 1.97; Tyr, 0.64; Phe, 1.11; His, 0.92; Lys, 1.09; Thr, 0.98; Ala, 1.36 and Arg, undetected. Furthermore, automated gas-phase peptide sequencing (Fig. 1d), and carboxyterimal analysis by hydrazinolysis (data not shown) showed that porcine endothelin is of relative molecular mass (Mr), 2,492, comprised of 21 amino acid residues with free amino- and carboxy-termini. The four cystine residues of endothelin were found to form two intrachain disulfide bonds (Fig. 5). Synthetic endothelin was prepared by liquid-phase chemistry, crosslinking the four cystine residues according to the analytically determined structure. The synthetic endothelin showed complete biological activity, and retention times identical to those of the natural peptide on a C₁₈ reverse-phase HPLC and an anion-exchange HPLC (data not shown). Technical details for the determination of the primary structure and the disulfide-bond topology, together with the liquid-phase synthesis of endothelin will be described elsewhere.

Vasoconstrictor/pressor activities

A typical example and the dose-response relationship of the vasoconstrictor effect of endothelin on porcine right coronary artery strips is shown in Fig. 2. Similar results were obtained with strips of rat aortas, cat basilar arteries, rabbit mesenteric arteries, dog mesenteric, femoral and renal arteries, and human mesenteric and pulmonary artery branches (data not shown). The maximum tensions are comparable to those of KCl-induced contraction. The estimated concentration at which endothelin was 50% effective (EC₅₀) in this assay was 4.0 ± 2.2 x 10⁻¹⁰ M. This figure is at least one order of magnitude lower than the reported values for angiotensin II[14], vasopressin[17] or neuropeptide Y[14], indicating that endothelin is the most potent mammalian vasoconstrictor peptide known to date. In vivo, intravenous bolus injection of endothelin causes a markedly sustained rise in arterial pressure in anesthetized, chemically denervated rats (Fig. 2c). The pressor effect is more prominent in diastole. Typically, more than 40-60 min is required for return of arterial pressure to the base-line levels.

The endothelin-induced contraction of porcine coronary artery strips is long-lasting andcharacteristically difficult to wash out, although completely reversed by the addition of 2 x 10⁻⁷ M isoproterenol, or 10⁻⁶ M glycercyl trinitrate (Fig. 2a). The constrictive response is resistant to the following antagonists: adrenergic (phenolamine, 10⁻⁴ M), H₂-histaminergic (diphe­nylhydramine, 10⁻⁴ M), serotoninergic (methysergid, 10⁻⁴ M), cyclooctoxygenase (indomethacin, 10⁻⁵ M), and lipooxygenase (nordihydroguaiaretic acid, 10⁻⁴ M). This suggests that endothelin acts directly on the smooth muscle cells. The endothelin-induced contraction is completely inhibited when bathing solution was substituted with Ca²⁺-free Krebs–Ringer solution containing 1 mM EGTA immediately before the addition of 10⁻⁴ M endothelin. Furthermore, the vasoconstrictor effect was markedly attenuated in the presence of the Ca²⁺-channel blocker, nicardipine (10⁻⁵ M). These findings agree with previous observations[13] and suggest that influx of extracellular Ca²⁺ is required for the action of endothelin.
reader can experience and witness Fig. 1c as the data inscribed in the laboratory, judge whether the marking of the arrow is appropriate, understand what the arrow indicates, and infer its relevant implications. What is referred to as what exists and what took place in the laboratory is translated into what it is that the reader can recognise in the text before them. This movement progresses further with the paraphrase "2.9 n mol purified endothelin" (lines 37), which shifts us back from what any reader can see in the figure at hand to the experimentally-produced and quantified substance. Then, there is a further shift to "porcine endothelin" (line 40), a paraphrase of the material taken from the experimental animal, and finally to "[s]ynthetic endothelin" (line 41), a biochemically manufactured substance. This textual interweaving in which the same object is referred to in all these different ways allows, in one's reading, a sort of transmigration of the object between the experimental setting, the natural world, and the science of biochemistry.

Tripling the Material, Inscribed Matter and the Scientific Concept

How this sort of transmigration is accomplished in reading is even more pronounced in the reading of Fig. 1 and its caption. Here the sequence takes in "porcine endothelin", "medium", "[t]he elute absorbance at 210 and 280 nm", "the active fraction designated by a solid bar", "[t]he active fraction from the anion-exchange chromatography", "[e]ndothelin activity", "the absorbance peak designated with an arrow", "[t]he active fraction in b", "[a]row", "final endothelin fraction", and so on, each of which spirally\(^1\) relates to the visual representation of Fig. 1 as well. Given that they all relate to "endothelin", such a reading allows us to make sense of an identity which is sequentially unfolding: on the other hand, if they are read in a sequence, that sequence involves the

\(^1\) When the object of reference is seen to be constituted by its representation and the representation is recognised as referring to this object, such constitution can be described as reflexive and related to the ethnomethodological understanding of social construction (Garfinkel 1967). This constitution is imaged in various ways: "back and forth" (Woolgar 1981), circular (Ashmore 1989: 32), "spiral" (MacMillan 1996: 26). My image of endothelin's constitution in Fig. 1 and its caption is closest to the spiral (see footnote 14).
maintenance and reinforcement of endothelin as its relevant topic. The sequence includes what was extracted in the scientists' laboratory, i.e. endothelin, that which can be observed as an indication of a procedural experimental method in the visual representation of the figure, i.e. endothelin, and the name of a 'scientific substance', a novel peptide, i.e. endothelin.

This reading-in-sequence allows the reader to assume the identity of (1) material substances (a certain animal's blood, a mass of medium, a pharmacological substance, etc.), (2) biochemical activities, measurable and readable with "inscription devices" (Latour and Woolgar 1986), and (3) theoretical concepts of the science of biochemistry. The first category is material, and collects the referent of expressions that indicate the physicality of endothelin (cf. Pickering 1995). The second category, which I call inscribed matter, includes those expressions which describe the activity of endothelin as measured or calibrated with the experimental method, and verbally and/or visually represented. In the third category of scientific concept go the theoretical terms of the relevant science.

As we have seen, when endothelin is configured in the initial report, the text of this report appears to be recognised as such in and through its intertextual relation with the texts of previous reports. And now, endothelin seems to be further enriched through these triple constitutional dimensions, each of which is also intertextual; though not in the sense of being tied with the texts of a specific literature. Instead, endothelin is tied with the material world and the world of laboratory practice: furthermore, it is tied with all the potentially interwoven texts of this research domain. That is, the references to endothelin seem to constitute it in relation to the corpus of texts in science, and the simultaneous multi-recognition of this corpus reinforces the name of endothelin.

Now, I would like to add one more thing to the notion of intertextuality: intertextuality is not just an additional feature of the reading. Rather, to be able to read this original 1988 article (that is, to perceive it as a text), is itself no different from the intertextuality generated through reading. I believe that the existence of nature is made

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92 See footnote 66.
available for us by bringing it in and tying it with knowledge-claims. As Latour points out, a lot of resources need to be brought in and linked together when science is "in the making" in order to make a scientific fact more robust (Latour 1987). Such work is, however, not so much to do with how many things from outside we add to our own text. Even though the strategic adding-up of 'external' resources may be indispensable for success, it is rather, from my point of view, the richness and elaboration of the intertextuality spontaneously generated in situ in one's own reading, which decides, initially and for us, the validity of a knowledge-claim.

Text Working on Textual Asymmetry

Reader  It seems that you are trying to construct the concept of 'intertextuality'. Is the presence of this concept, after all, what you are committing yourself in this text?

Writer  Well yes, and why not. Now I am talking about 'intertextuality' and that very work of talking is itself establishing intertextuality. Is it not? As Woolgar says, "the practical expression of, or reference to, a phenomenon both recreates and establishes anew the existence of the phenomenon" (Woolgar 1980: 246).

Reader  "In describing a phenomenon, participants simultaneously render its out-there-ness" (ibid.: 246). What Woolgar says in this sentence of course also applies to itself. Even reflexivists actually establish something as soon as they open their mouths. Okay. But I still protest against your position. You have claimed that the scientists are making 'endothelin' available in the scientific materials, but on the other hand you seem to be using your own concepts such as sameness-and-difference, intertextuality, etc. without bracketing. I mean, your concepts are held so constant that you are using them unproblematically for the analysis of the other participants' construction. Don't you think that this text is getting a bit asymmetrical? Here speaks Mulkay:

Textual asymmetry is not only evident in the discourse of scientists and politicians. . . . Sociologists work on the assumption that only they can see the real meaning of the raw data provided by their partner (the participant). The crucial difficulty arising from sociologists' endorsement of this interpretative
asymmetry is that the social world of actions, texts and other interpretative forms is thereby treated as a realm of fixed meanings which can find expression exclusively through the analyst's voice. (Mulkay 1985: 101-2)

**Writer** 
You are saying that the text is asymmetrical, but if that is the only truth, why are we brought into the text to argue over it? Let me show you Mulkay's next paragraph:

I take it as a basic premise that the social world is open to an indefinite variety of alternative interpretations and that forms of analysis should be devised which allow us to deal with this interpretative fecundity. (ibid: 101-2)

We are, here in this text, trying to establish the potentiality of opening this text to a variety of readings. Mulkay has made a wonderful recommendation that texts can invite other voices (which here include those of endothelin, scientific activities, sociologically constructed scientific worlds, etc.) to engage in a range of different interpretative practices in the text. These voices may be conflicting, but none of them are almighty and thus can never entirely supplant each other. Mulkay states that such openness is demonstrable in some forms of writing.

**Reader** 
I follow what you are saying. It's what you referred to at the beginning of Chapter 1, about writing in a Bakhtinian style. So, for example, if Mulkay only states the above issue in his text, he is still more or less monologic. But, in fact, he invites scientists to speak in his text. Thus, if you use a dialogical textual form and let it speak for itself (Knorr-Cetina 1981), as well as letting the endothelin researchers' texts speak for themselves, we shall not have engaged in a monologic form. That looks like some kind of solution for engaging in reflexivity rather than merely talking about it (in an unreflexive way) (Woolgar and Ashmore 1988: 4).

**Writer** 
Oh, yes. That is certainly what we are aiming for! After all, this notion of intertextuality would save us from being unreflexive. We . . .
Reader Wait a minute! Just remember that for this sense of reflexivity, I should not to be too far in agreement with your position, you know. In fact, I am not convinced by your position. You have claimed that in this thesis, the text such as the original 1988 article is to be regarded to have a parallel status to the text of your analysis (this thesis, p. 3). But if the former text is regarded as being encapsulated within your analytical text, is it not that your text is still monologic? And are you not actually a single dominant story-teller?

Writer True. As well as showing the Candidate's own process of knowing endothelin, I am of course trying to organise the whole text. But I am trying not to make my analysis dominant. I am trying to reveal how I have managed to write the analysis, and then later, in Chapter 5, I will display how I am trying to make my analysis open to the opportunities of being 'analysed-back'. My position is to try not to appropriate the other voices solely for the sake of my analysis.

Reader Can you really do that? I doubt whether the readers would be happy if you succeeded, since the text would be so inconsistent. If your voice is not dominant, the text will not function well, and the reader will regard it as a scattered pastiche where your own standpoint doesn't exist or where you are just indulgently switching between textual modes.

Writer But I believe that it is important to allow each of the flowing voices in all the different texts, such as the original 1988 article and the reading which I am presenting, to be heard.

Reader That's too naive an image of writing! Any author must own her responsibility for the text (and) to the readers.

Writer I believe that the author's role is in a sense already a dominant one: even though she tries to let other voices speak, she still inescapably speaks as and for herself. On the other hand, however, what constitutes this 'author' has not remained the same through the reading/writing of this thesis. She is becoming different in the process of her own knowledge construction (see Chapter 5). For example, in the course of the Candidate's research, the way she makes sense of the original 1988 article has changed.
All these aspects of 'becoming different' cannot be denied in this research which has been pursued through her active interaction with (other) texts and (other) participants. Writing 'about' this process is not just a matter of describing the final product, but of writing within this ongoing and potential process as a whole.

Reader But in the end, you are presenting the final product monologically!

Writer No, I am not!

Reader and Writer So, it's you who are wrong!

Writer You have just pointed out the constructed nature of 'intertextuality' in my analysis. But I have been, in a self-exemplifying manner, achieving the intertextuality here -- in the process. You see, given that the state of this text is asymmetrical, as you critically identified, such an identification is also "a matter of contingency" (Ashmore 1996). I mean, my writing is to be read with the intertextuality achieved in the course of its reading: in the readers' process of knowledge construction. This process itself has been dialogical, where a variety of discussions, confrontations and negotiations have been going on, such as these we are now having.

Reader Given that the intertextuality of this text exists, and will be further enriched, the outcome of the authorship (and the authorship of the outcome) is yet to be decided/negotiated!

2-4. Extraction of Endothelin by HPLC and the Interview Talk between a Scientist and the Candidate

Following on from the reading of the original 1988 article, I will now pursue a similar approach by turning to a different kind of text. This time, the material is taken from extracts of a transcript of the interview talk between an endothelin researcher and the Candidate. The main topic of this interview is the work of the Tsukuba Group in extracting endothelin.
Connecting Different Settings

In the following extracts for analysis, Prof. Goto provides an account of how he pursued one of his experiments using the method of chromatography, HPLC, which is presented in Fig. 1 of the original 1988 article (p. 90). In the extracts, this experiment is demonstrated in situ by Prof. Goto's explication of Fig. 1 and his making of a pointing gesture during the interview. In the first few lines of the following extract (2L), the explication and gesture involves a sequential paraphrasing of the kind we saw in our analysis of the text of the original 1988 article:

2L
160. Prof. Goto "(points to Fig. 1) This is the one that I just mentioned as involving, eh-
[the separation] method of chromatography, [[you see. So, right. Okay.]]
161. Mushakoji [Oh! ] 
162. Mushakoji "(points to Fig. 1)"
163. Mushakoji [Yes. Oh, so this is the- ]
164. Prof. Goto Yeah.
165. Mushakoji and-
166. Prof. Goto the structure ((of endothelin)) [at any rate] was then identified- yeah-
167. Mushakoji [The structure was-]
168. Mushakoji Oh. So it was after doing this that the structure was then-
(Prof. Goto, 14 March, 1994)

Fig. 1, pointed to by Prof. Goto (line 160), was in a copy of the original 1988 article which was then placed on the desk between Prof. Goto and Mushakoji.

The gestural practice of pointing (line 160) was accomplished when Prof. Goto opened the second page of the original 1988 article and turned it around so that Mushakoji could view it properly (that is, right-side-up from her perspective). It was referred to as "[t]his" (line 160) and then linked to "the one that I just mentioned" (line 160), so that "[t]his" (line 160) could be recognised as a paraphrase of the referent in the previous talk. Furthermore, by pointing to the copy of the original 1988 article, what is referred to is associated with what is written in this article, which is an experiment in a particular laboratory setting, and thus with this scientist's activity of extracting endothelin in his laboratory. The association between these things is accomplished by another reference, "[t]he structure" (line 166), to the structure of endothelin. In this way, a figure and its caption (Fig. 1 and "[t]his" [line 160]) in the article, and what had been mentioned previously ("[t]he one that I just . . .") become tied with a method of biochemistry ("the separation method of chromatography" [line 161]).
The most beautiful aspect of this talk is the paraphrasing which occurs when Prof. Goto establishes a connection between (a) something that had already been referred to in his prior talk, (b) the figure of the original 1988 article that he points to during the interview, and (c) what took place in the conduct of the experiment. This connection makes it possible not only for him but for both of the participants to point to the figure and talk about the method used for extracting endothelin. That is, during the activity of uttering her statements recorded in lines 162 and 168, Mushakoji was simultaneously pointing to the figure as well. In other words, references to the method for extracting endothelin are attained by means of the participants' interactional accomplishment of these referencings. Here, we can note that Prof. Goto's reference to things is sequentially accomplished in the interview talk, "in utterances that make a point vis à vis a local context of utterances and activities" (Lynch 1993: 191-95). The beauty lies in the display of a connection between different settings, each of which is local and situated. Moreover, it is this connection which renders the scientific practice of extracting endothelin viable and approachable to non-scientists.

In the following extract, Prof. Goto continues to talk of the particular method used for the extraction of endothelin. The extract exhibits the work of making a connection between the experimental toils of the laboratory, Fig. 1, and the interview talk that proceeds on the spot. At this point, Prof. Goto's explanation became clearer due to the physical availability of a copy of the article on the desk between Prof. Goto and Mushakoji: moreover, he rolled paper into a tube and pointed to the top end of it.

93 In this sense, the connection made in the interview talk addresses the critical issue which Lynch calls the "mastery" of science and its lack:

[L]he 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. (Lynch 1985: 285)

The upshot is that the above talk of scientific activity cannot but be unlike the vernacular mastered by insiders to endothelin research. But leaving aside the contentious issue of the Authenticity and Purity of what constitutes scientific practice (and of how such Authenticity and Purity are constituted in sociological analyses), it is my contention that Prof. Goto informs Mushakoji about his science. Moreover, this connection between endothelin constituted in the laboratory setting and the interview setting seems to be achieved in the talk.

In addition, this connection itself can be further tied to the setting in which this chapter is read/written right now.
This action takes place in a rather animated fashion, as if the experiment is being demonstrated *in situ*.

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In this extract, Mushakoji appears to be learning like a school child who does not participate in actual laboratory work yet who is witness to an impromptu modelling of the process (cf. Atkinson and Delamont 1977). We can also see this talk and its accompanying gestural activity as working to accomplish the distribution of knowledge concerning the method of chromatography both in terms of what can be recognised by the participants reading Fig. 1, and in terms of what it is that is worked on in the laboratory setting itself.
Then, that sort of thing, uh- will be done on each one of them. And then, this-
(points to Fig. 1) that one will be indicated at this 'bar' (in English),
(points to plotting bar on graph), you see?

Mushakoji: Mm hm.

Prof. Goto: In this (refers to something that exhibits activity in the sample portion indicated) ['fraction' (in English)].

[At that point-]

Mushakoji: the one which permeated [[in that- at that point-]]

Prof. Goto: [[The particular]] substance which shows activity is likely to exist in that sample.

Mushakoji: Oh.

Prof. Goto: So that's basically how it works. And both of these axes ((the two plotting bars adjacent to the one indicated as containing the substance))

[will all be ] discarded-

Mushakoji: [This one will-]

Mushakoji: Uh huh.

Prof. Goto: Only [this here] ((indicating the axis of the Fig. 1a))

Mushakoji: [this one-]

Prof. Goto: will actually be analysed yet a [[further]] time.

Mushakoji: [[Oh.]]

Prof. Goto: ((just managing to view figure 1 from the, relative to him, upside down position where it had been set in order to show it to Mushakoji)) Uh, let's see, yeah, right, that's the one.

Mushakoji: Yes.

Prof. Goto: So, it will get done once again. And then for the 'column' (in English) on both this side and the 'column' (in English) on that side ((pointing to paper tube referred to previously)) the one that further looks a little- the one that's different in that sample will be used. The nature of the-

Mushakoji: Mm.

Prof. Goto: resin-

Mushakoji: Mm.

Prof. Goto: Well, you see, because there are various resins,

Mushakoji: Oh.

Prof. Goto: And, this particular one will be used in a similar way.

Mushakoji: Yes.

(Prof. Goto, 14 March, 1994)

Backgrounding some further interactive talk on the sequences of the method, what the "bar" (line 256) in the figure indicates, and what is referred to by "the particular substance" (line 263) are accounted in the following interaction as something coming out in a solid and coherent way. Prof. Goto repeatedly points to the figure (lines 192, 256, 257, 259-60, 271). He also models some experimental actions during his accounting work (lines 202-203, 207, 279-82).

The linking between the experiment that took place in Prof. Goto's laboratory, what is represented and interpretable in the figure, and Prof. Goto's impromptu demonstration which the participant Mushakoji witnessed in the context of the interview setting, continues in the following lines (lines 290-94):
290. Prof. Goto And so we pour this in (refers to the solution which contains the substance exhibiting activity with the motion) and will gradually flow through it- with time- through a process of elimination- in this way- (points to Fig. 1) this is referred to as a fraction.

294. Mushakoji Mm.

(Prof. Goto, 14 March, 1994)

Mm.

In this extract, the task of introducing a technical term -- that is, "this- this is referred to as a fraction" (line 293) -- is pursued. This task seems to have worked out the interlocutor's agreement on the proper usage of a term for referring to some part of the figure, to something which is relevant to Prof. Goto's experiment, and to a specific concept authored by Prof. Goto. The term "fraction" also appears in the original 1988 article. This interaction then works to allow Mushakoji, supported by Prof. Goto who is entitled to know the meaning of this term, to make sense of this figure in the article. It engenders the experimental event wherein something was called a "fraction".

And it will then be further divided and the same procedure will again be repeated.

And then, the next time- well, like look. This one- oh wait, the indicator doesn't seem to be recorded. Now, isn't it written here somewhere? Oh, here we go. This is it. Right here.

Oh, yeah...

Yeah. [So, yeah. The arrow. Really only- pretty much-] [It's this arrow here, right? Yeah. It's at 50- um- at 60.] right here- it has already reached a point where the peak is indicative of a single entity (refers to the active substance).

Yeah.

Nevertheless, it actually indicates that there are of several different [kinds-] (of peaks). That is, that there are still more.

That is, that there are still more.

Oh, is that so?

This will again- again, this time around, taking only this sample portion, this procedure will once again be done.

Mm.

And then it will be done under a different set of conditions.

Oh. This time it would appear to be rather [prominent, is that right?]

[In the end-]

Yeah. In the end, in this peak- here where it looks like only one peak can be observed- well actually, when the procedure gets done a number of times, there will still appear yet more peaks in it, you see?

Mm.

So, when we arrive at this point, this will again get done- up to the point at which nothing more can be further divided.

Yes.

101
At that stage- right at- finally, in the very end, a single entity which actually has an activity will then have been found.

Oh, well. That's how it works. In short, that's basically uh- well, the method- Mm. Yes. you see? This is a method which we- only ten years ago- which Kimura-sensei and I established. And we have been working with it eh- since then, [I as you can see. I] because that was obtained, [2 so-] [2 That's why-] [2] our work was possible. Like "pat" ((a mimetic expression descriptive of a swift motion, here analogous to "boom" in English)) (laughter) As "pat" only if we have an object.

(Prof. Goto, 14 March, 1994)

Here, Prof. Goto accounts for the experiment in detail: what was at first experimentally extracted with less determinate accuracy was later sifted out with greater determinative accuracy using the method of HPLC: finally, it is identified definitively enough (line 321-322) to be called "a single entity" (line 324) that has an activity (line 325). In this sequence of explanation, it is interesting that Prof. Goto remarks that "only if we have an object" (line 342), could his group extract it. The discovery of endothelin is here constituted in such a way that a hypothetical object is treated as existing prior to the experimental extraction of an object from the physical material.

How Endothelin is Made Known

Thus far, we have read how the particular scientific method, HPLC, was used in a particular laboratory setting, and how that constitutes the very activity of extracting endothelin. In the previous section, we started to see how endothelin is established and how its existence is consolidated in terms of sequential paraphrasing and naming in the reading. We observed a spatio-temporal connection between the corpus of scientific literature relevant to the existence of endothelin, the experimental laboratory setting, and the reading of this article. In the above analysis, a spatio-temporal extension is observed between the article in which endothelin is initially reported, the events occurring in the
participant scientist's laboratory, and Mushakoji's learning of the scientists' activity in the interview talk.

Now, I would like to suggest two points that are significant for establishing this reading. First of all, the text of the interview talk is intrinsically connected with other texts such as talks that have previously taken place, the original 1988 article, and so on. This notion of a connection among texts within a single text is what I have been calling *intertextuality*. In these extracts, it is not so much that the text of someone's talk is *accompanied* by intertextuality; rather, the intertextuality is generated and recognised *in situ* in the text. That is, intertextuality is constituted in the course of the ongoing process of talk and occurs in such a way that the participants prompt one another to perceive the sameness of reference in their talk. As I have mentioned in the analysis of the original 1988 article (this chapter, p. 92-93), to perceive the text is to perceive this intertextuality.

Secondly, what took place in the above talk is not just Prof. Goto's account of activities related to a particular scientific method used for the extraction of endothelin in his laboratory, but also the recognition of this activity for *all* the participants. Prof. Goto's account is immediately agreed upon by Mushakoji (line 275-7 followed by 278, 298-300 followed by 301). The gradual sequencing of Mushakoji's response to Prof. Goto's account displays this situated accomplishment oriented toward that which he is explaining. In other words, the way endothelin was extracted by HPLC and inscribed in Fig. 1 of the original 1988 article is then discovered by Mushakoji. This is what Atkinson and Delamont refer to as a student's "guided discovery" in science education, where "the socially agreed nature of 'science' . . . and the natural world" is "recapitulated" (Atkinson and Delamont 1977). In tandem with this, Mushakoji also displays her understanding that the experiment took place in his laboratory, through their shared reading of Fig. 1 illustrated by Prof. Goto's gestures. Thus it is not only Prof. Goto's account, but also, and quite powerfully, Mushakoji's *participation* in this account which renders the described activity "the extraction of endothelin". In other words, Mushakoji's participation results in the co-constitution of endothelin and the
scientific activity of extracting it. It can thus be seen that this interviewer is included as part of the science of HPLC, through her activities as an outsider (Latour 1987)."4

**Reader**  
You wrote that the activity is recognised by all the participants. That leads me to ask a further question: Given that we are to take it that Mushakoji is participating in the extraction of endothelin in the interview talk, is the reading of this interview transcript any different? That is, are we -- I mean, the reader of this section -- inside this science of extracting endothelin? Are we establishing the work of extracting endothelin in Prof. Goto's laboratory, or are we merely establishing what the author of this section is saying (whether we agree with it or not)? And how far can we know the extent of our involvement in this matter?

**Writer**  
How far is our work of reading and writing the same as and different from that of Prof. Goto and Mushakoji? Are they not also creating a series of texts?

Making the Knowledge-Claim Significant

It seems that the analysis of intertextuality can show us how a knowledge-claim is made and reinforced, and what kinds of resources are brought in for the making of a knowledge-claim and its reinforcement. The intertextuality which works to link different texts -- what has been previously reported in a particular research domain, what is observed in a laboratory setting, what is read from an article and what is mutually understood and shared in talk -- is particularly interesting with regard to making knowledge-claims about endothelin. Finally, I would like to demonstrate how

4 With the notion of 'Metrology', Latour describes the world of technoscience in which the gigantic enterprise of science has succeeded in making "the outside a world in which facts and machines can survive":

> Scientists build their enlightened networks by giving the outside the same paper form as that of their instruments inside. . . . [T]he result is . . . they can travel very far without ever leaving home.  
> (Latour 1987: 251)

Mushakoji in the above extract is "enrolled" in this science world, while Prof. Goto is also enrolled in her candidate SSK world.
such linking plays a role in *making knowledge-claims significant*. The following two extracts show how such ties work by attaching a certain value, i.e., 'significance', to the original 1988 article, the Tsukuba Group's work, and the discursive topic of the interview:

2Q

441. Mushakoji Well as you said before-
442. Prof. Goto Yeah.
443. Mushakoji you recognised that this was something significant-
444. Prof. Goto Yeah.
445. Mushakoji and [1 then 1] you just committed yourself
446. Prof. Goto [1 Yeah. 1]
447. Mushakoji [2 to it for a number of 2] years. At what exact point was it that this
448. Prof. Goto [2 Right. Right. 2]
449. Mushakoji [3 started to look like it might be something significant- 3]
450. Prof. Goto [3 Th- well, no 3] that was already clear- at this point- at this point-
451. ((points to Fig. 2 in the original 1988 article))
452. Mushakoji Th- this persistence- persist- eh- [endothelin persists as-]
453. Prof. Goto [And also ] an extremely small
454. amount- at this small amount and this significant- eh- the object that lowers
455. the blood pressure-
456. Mushakoji Yes.
457. Prof. Goto or constricts the vascular- [had] never existed before. That's why they said
458. Mushakoji [Yes.]
459. Prof. Goto that a particularly astonishing object had been encountered.
460. Mushakoji Oh, okay.
461. Prof. Goto The structure of an object, its relatively modest activity, the kind of gene that
462. it is- if you just look at an object itself, it is not really all that interesting.
463. Mushakoji Mn.
464. Prof. Goto Because there are millions of objects which exhibit physiological activities.
465. Mushakoji Yes.
466. Prof. Goto On the other hand, however, eh- uh- That this one has a surprisingly unusual
467. nature which makes it- well, naturally enough, it really stays for a long time
468. or, that is, really grabs everyone's attention.
469. Mushakoji [Mm. Right.]
470. Prof. Goto [That's because it has this sort of] slightly strange structure and has such an
471. amazing [[[function.]]
472. Mushakoji [Mm hm.]]

(Prof. Goto, 14 March, 1994)

2R

346. Mushakoji Yes, based on what you've just said, well, uh- many of the significant features
347. of endothelin which were written about in the review (refers to the review
348. article written by Prof. Goto)) [1 had already, of course, pretty 1] much
349. Prof. Goto [1 Yeah. Yeah. Yeah. 1]
350. Mushakoji [2 appeared here 2] ((points to a copy of the original 1988 article))
351. Prof. Goto [2 Even then, it was already 2] clear!
352. Mushakoji and so already, the main [3 features 3] are what were written up
353. Prof. Goto [3 Right! 3]
355. Prof. Goto [4 That's right! I bet- yeah. 4]
356. Prof. Goto (laughter) You see, that's how [it is.]
357. Mushakoji [I see, so that's the way it went.]

(Prof. Goto, 14 March, 1994)
In the first extract (2Q), it is Prof. Goto who mainly seems to be constituting the significance of endothelin and their activity of reporting it. This constitution is accomplished by making a connection to a plethora of various texts: the connection is made between what was observed in the experiment, how that observation was perceived by the Tsukuba Group, what is represented in Fig. 2, how the original 1988 article was read by other people, what these people said (line 457, "they said"), what was surprising to them (line 466-468), and 'what endothelin is and how it is perceived'. Nature (i.e., endothelin) and society (i.e., the arena wherein the people, including Prof. Goto and Mushakoji, perceive endothelin) are linked together. In making this connection, a knowledge-claim (i.e., that endothelin exists) and a value (i.e., that endothelin is significant) are also linked. This link not only establishes the recognition of the significance of endothelin for Prof. Goto and his community, but also in so doing it establishes the significance of endothelin for both Prof. Goto and Mushakoji.

In the second extract (2R), Mushakoji's work can be seen as also establishing the significance of the original 1988 article but in a different manner. She does not constitute the significance of Prof. Goto's achievement as he does in the first extract (2Q): he is the only one in this talk who is entitled to draw upon the nature of endothelin as established in his laboratory and to give accounts of how this achievement is perceived as significant by his colleagues and other members of the relevant community (cf. Sharrock 1974). Instead, Mushakoji introduces other texts, such as the review article which Prof. Goto and his colleagues had published in *Folia Pharmacology Japan* (Goto, Sakurai and Kasuya 1992). This article reviews the (then) current understanding in endothelin research in its summing up of 117 papers. Mushakoji provides for the significance of the original 1988 article by tying what is presented in the original 1988 article to what is represented in this review article.

This link generates *the sameness* of what is represented in the original 1988 article and the review article (1992). The establishment of this sameness works to invoke the significance of the original 1988 article: the original 1988 article is significant because what is reported there is still true and still constitutes the main features of
endothelin as reported in the review article, published 4 years later. Moreover, this works not only to link these two articles, but also to make links between the genre of the original article and the genre of the review article. Given that the role of a review article is to synthesise all individual research articles into the knowledge of that field (Sinding 1996; Myers 1991), its content can be regarded as a representation of the main research achievements accumulated in endothelin research. Therefore, in pointing out how most of what is written in the review article has already been written in the original 1988 article, Mushakoji is suggesting that the original 1988 article has been persistently the main achievement in the whole corpus of writings comprising endothelin research. Thus, while what is reported in the original 1988 article is held to be the same as in the review article, it is what accounts for this sameness (persistence) which works to establish the value and significance of the original 1988 article.

In the above cases, the participants' establishment of the significance of endothelin seems to be interwoven with their establishment of their knowledge of endothelin. It seems that knowing endothelin and knowing its value goes hand in hand. Or rather, it is that the establishment of its value is part of knowing endothelin. It is through generating intertextuality that we all engage in the activity of providing and acquiring knowledge.

Stirring Intertextuality

*Reader*  
I'm still confused... You are writing about the intertextuality generated in your analytic materials. But the nature of this intertextuality is not now the same as that

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95 The evaluation of a knowledge-claim alters with changes in what that knowledge-claim refers to. For example, a priority dispute about a discovery involves not only the issue of who first made the knowledge-claims, but also the issue of which knowledge-claim is correct (Brannigan 1980: 77; Mulkay 1985: 191-196).

96 Likewise, it may well be that to make a knowledge-claim and to claim its value also goes hand in hand. In relating knowledge-claims and their significance in physics, Pinch contrasts scientists' experimental reports which make knowledge-claims with potentially low value and those which make knowledge-claims with potentially high value. The experimenter's dilemma is whether she should choose to make a report which is relatively trivial but which has a high chance of success, or instead, whether she should take the risk of making a report which is more significant and challenging but which would be more difficult to establish (Pinch 1985; see also Chapter 1, p. 17-18).
which was generated in the original contexts in which the scientists wrote, read, and were interviewed. It is also different over the course of your analysis and in each of your drafts (and presumably it will be different on each separate occasion of reading this chapter). Or, am I attending to the wrong question?

Writer No. You are right! Intertextuality emerges locally. For instance, the dialogue that took place in a particular situation, the sound reproduced from the tape recorded in that situation, and the text of its transcription are only made to be the same for the sake of our analysis. You know, even a doppelganger text is different from itself. Mulkay introduced this point by taking Borges's story about the Don Quixote written by Menard, who (re)wrote Don Quixote (written by Cervantes) word for word. Menard's Don Quixote turned out to be quite different from Cervantes' (Mulkay 1985: 142-144; Borges 1956a).

Reader So, if we are reading the original 1988 article and the interview transcript presented in this chapter with the intertextuality we have here, we will know endothelin in a totally different way from . . .

Writer Different from what? It is important not to search for the 'true' knowledge of endothelin, but to generate its intertextuality in an appropriate way. Let me repeat what I wrote in the last sentence of the previous section:

It is through generating intertextuality that we all engage in the activity of providing and acquiring knowledge.

(Mushakoji 1999: this chapter, p. 107)

The task of generating intertextuality in an appropriate way is only potentially pursued in an ongoing process of writing and reading. Therefore, I need support from you, the Reader. Let me also mention some other texts that will support my argument, and might inform your reading here. They include Ashmore (1989:14) and Mulkay (1985:154) and . . . (a pause) . . . well, no, actually what might be more useful are those exciting and stimulating talks in which I participated in my supervisor's office ((Mushakoji points her finger in the direction of Loughborough University, Loughborough, U. K.)), those
discussions with my colleagues in DARG sessions, my panel reviews, all those seminars ((making a mischievous gesture of enthusiastic and lively talk)), and . . .

**Reader** Oh, stop! What you are doing is too unfair on your readers! Will you ever finish (re-)stirring up intertextuality!

(a significant pause) . . . . followed by* (*The authoritative voices of . . .)

So, what then, you may well be asking, is the product of all this welter of activity [of SSK]? One answer, which is not as frivolous as it may sound is: a lot of paper and a lot of print. On second thought, such a reply is not staggeringly helpful. But if we put it another way and say the product is a series of "texts" -- whether written or spoken, formal or informal, or even verbal or nonverbal; a text being here an ensemble of interpretables -- if we put it like that, then we have a principled way of avoiding the sort of answer that is implied in asking for a product: namely, a neat list of actual findings about scientific knowledge which constitutes the coherent piece of credible sociology that is the sociology of scientific knowledge. If we talk in the postmodern fashion of texts rather than findings . . . this sensitises us to the permanently interpretable and reinterpretable nature of all such cultural products.

(Ashmore 1989: 14)

All of these readings of sameness/differences can be textually legitimated and strongly defended. I do not, therefore, wish to insist on the validity of any one. They are all viable readings. (Mulkay 1985: 154)

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97 This is an abbreviation of the Discourse and Rhetoric Group, which holds regular meetings and sessions in the Department of Social Sciences, Loughborough University.
CHAPTER THREE

The (Social) Basis of (Scientific) Discoveries

In the study of science (and knowledge practices generally) the student cannot avoid being inside and outside at the same time. Another way of saying this is that the reflexivity of inquiry into inquiry is not a problem for that inquiry but a constituent problem of it. (Ashmore 1989: 109)

Attributional Work Related to the Making of Knowledge-claims in Endothelin Research

Attributional Work in Writing about Cases of Scientific Discoveries in SSK

Attributional Work Related to Knowledge-claims which are in the Process of Being Made in the Candidate's Research

Introduction

The research object of this chapter is investigated along the same triple parallel lines as before. The first line is concerned with (1) how the work of attributing the initial reporting of endothelin in 1988 is organised in endothelin research. The second and third lines are concerned with (2) what is involved in SSK (and ethnomethodological) analyses of scientists' work as cases of 'scientific discovery', and (3) the way that the Candidate herself comes to deal with her research object as a 'discovery', in the course of conducting her research.

Reader I assume that your topic for this chapter relates to discovery studies in SSK. If so, are you proposing that your text may be an example of its topic? I mean, are you going to do the same thing — that is, produce a discovery?

98 This title is an allusion to Brannigan's The Social Basis of Scientific Discoveries (1981).
99 Scientific discovery is one of SSK's topics (Brannigan 1981; Brown 1994; Mulkay 1985; Myers 1987; Schaffer 1986; Woolgar 1978, 1980). Among these, Brannigan (1981) provides the most comprehensive
Writer: Well... would you mind not delving into such questions quite so early in this chapter? Anyway, similar questions have already been considered in some detail by certain SSKers (Mulkay 1985: Chapter 4; Ashmore 1989: Chapter 7).

Reader: Be careful! Such references may render your project unoriginal and more like a replication of those previous studies.

Writer: But still, I need to cite some studies as prior to mine, since "the element of difference enables [any knowledge-claim] to attribute originality", and "the element of sameness enables [it] to attribute validity" (Mulkay 1985: 145). Now, let's not presume that I have been attempting to establish this writing/reading as anything. I haven't even attempted to proceed along a triple parallel regarding that issue.

Reader: And quite right too. After all, on how many occasions do SSKers treat their achievements as discoveries? As far as I see it, SSKers make claims to have developed 'new' approaches, 'new' programmes, and so on, rather than claiming 'discoveries'.

Writer: I will defer comment until I have discussed what analysts of scientific discoveries say. But until I make a knowledge-claim, and set out to persuade people to attribute the status of a discovery to it, I don't think that your question is really appropriate.

Reader: Okay. Go on in your own way... I will keep my criticisms for later!

This last line of the triple parallel process (see p. 2-4, 55) entails considering how it is that knowledge-claims which are in the process of being made in the Candidate's research are further (intended to be) taken as an instantiation of that very thing to which they refer. In short, this chapter will be an examination of the accounts made in endothelin research, SSK and the Candidate's research in terms of the attributional work which takes place in and through each of these three domains.
3-1. Something Which Existed and Continues to Exist Prior to Endothelin

Throughout the Candidate's research, the achievement of the Tsukuba Group in 1988 has been called 'the discovery of endothelin'. In the interviews with members of the Tsukuba Group, there are numerous occasions in which scientists discuss how they began their work. In this context, one of the most striking remarks made with regard to the status of this work as a discovery is shown in extract 3A. Here, Prof. Kimura is accounting for the Tsukuba Group's foresight in taking up their research:

3A

317. Prof. Kimura That's how we have got along so far.
318. Mushakoji Yes.
319. Prof. Kimura So, in that regard he (the first author of the original 1988 article) is very- in that regard, he has a certain vitality, as they say, and definitely eh- is good
320. when it comes to establishing a sense of focus, and eh- he does not just- doesn't just consider what he's doing in retrospect, but works on it with considerable forethought. So, it was eh- his view eh- his positive outlook, I think-
321. Mushakoji Yes, eh- I am not yet quite sure about how he got the potentials, eh- how he could come to grips with them-
322. Prof. Kimura Yeah
323. Mushakoji eh- the potentials those kinds of potentials, how he managed to get a handle on things.
324. Prof. Kimura Oh- that was just Highsmith's thing. And then, eh- (Prof. Kimura, 1 July, 1994)

What does Prof. Kimura mean by "just Highsmith's thing" (line 330)? The name "Highsmith" had actually been mentioned in earlier remarks:

3B

77. Prof. Kimura So, when we started- [in '87-]
78. Mushakoji [With what is called NO Yes.] (NO refers to a theory regarding vasoactivities in endothelium cells)
79. Prof. Kimura and thus in such circumstances, eh- it was also- up to- well '87 the history of research was almost and exclusively in NO and EDCF (refers to 'endothelial cell-derived constricting factor') didn't have much of a history [at that point.]
80. Mushakoji [Yes.]
81. Prof. Kimura Not very much or rather- well, it had not been emphasised- was emphasised very little. Highsmith in '85- (refers to the article [Hickery, et. al. 1985] cited in Prof. Kimura's lecture handout at hand) and in the '87 article as well (refers to the article [O'Brien 1987]) we found it after that time while in the midst of working on it.
82. (....) (some lines omitted)
83. Prof. Kimura Thus, basically, the problem of Highsmith, namely, to pick this up, (points to a copy of Highsmith's paper)) well that idea was quite important.
84. Mushakoji [Mm.]
85. Prof. Kimura And, so- based upon this sort of idea and eh- of course for me it was just timely because eh- in co-operation with Prof. Goto eh- at that time we were working on the vessel. [And] although we did not produce the data from it, [Mm.]
86. Mushakoji [Mm.]
87. Prof. Kimura eh- we were working on it for about one-and-a-half years, so it was right in the
One plausible assumption that informs a reading of the above exchange is that Highsmith was one of the scientists whose publications relate to the discovery of endothelin. The discovery of endothelin which is regarded as having been achieved by the Tsukuba Group can be seen to be initiated basically by picking up "the problem of Highsmith" (lines 90-91) as "everyone was familiar with the literature" (lines 101-102). It was at this point that the Candidate recognised there to be some thing -- some entity that seems to have transubstantiated or to still be in that process -- *which both existed and continues to exist prior to endothelin*. This something cannot be 'endothelin' since otherwise endothelin could not be said to have been discovered in 1988. What is, then, this something that existed before the discovery and in relation to which endothelin is said to have been discovered?

In the talk immediately following extract 3A, Prof. Kimura had introduced the Candidate to a historical review article written by Highsmith. This article (henceforth, '1992 Highsmith paper') appeared in the book *Endothelin* (Rubanyi 1992). As we will see in the next section, Highsmith claims that in this publication, his group had been working on a hypothetical new substance since 1982, and that working with this substance resulted in the Tsukuba Group's having purified endothelin. In the interview recorded above, Prof. Kimura further notes that Highsmith was "really regretful" about the way that his own efforts turned out to be laying the groundwork for another's discovery.

After this interview with Prof. Kimura (3A and 3B), it suddenly occurred to the Candidate that something to which she had not given much attention had become relevant. The following transcript is a record of the encounter in question:

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Supplementary note: Dr. Mary Horton-Salway has suggested that I am formulating the above event as "at first I thought ... but then I realised" (Wooffitt 1992: 77-78). However,
In reviewing this transcript material, she has realised that the paper referred to by Prof. Goto (line 31) must have been Highsmith's paper (Hickery et. al. 1985). On the strength of such an inference, she then confirmed that this was indeed the case.

Reader Don't you think that such details are rather trivial and mundane to write in your PhD thesis?

Writer Maybe, but isn't it crucially important to show how the Candidate's research has proceeded chronologically? Explaining such 'trivia' also displays the process of knowledge construction in her PhD research. I am showing how she gets to know her research object. The data' such as that presented in 3C comes to have a recognisably different significance by virtue of engaging in this very process. And given that it is my story, I shall continue with it...

The Candidate sent a letter requesting confirmation (Mushakoji to Prof. Goto, 27 November, 1994). In his reply, Prof. Goto told her that the paper read at "the Journal Club" (3C, line 32) was indeed that of Highsmith's group (Hickery et. al. 1985). He

On second thoughts it doesn't look exactly like that, but perhaps it's some kind of 'stake inoculation'. What do you think? (Horton-Salway, personal communication, May 1999)

'Stake inoculation' has been described by Potter (1996). It is a discursive device "which works to dismiss any suggestion that the speaker might have a vested interest in constructing one version of events rather than another" (Horton-Salway 1999).
further informs her that the Tsukuba Group had known about the potential existence of endothelin prior to the initiation of their work: but that until endothelin was identified by them, they did not know the strength of its vasoconstriction and other features. In addition, he provides the following account with regard to his community's recognition of this discovery:

His account of why his group's achievement is currently attributed the status of a discovery -- because endothelin "was proved at [his group's] site in a single stroke" (lines 2-3) -- is particularly noteworthy here. It suggests that the conclusive factor in this case of discovery lies in the intensiveness of the scientists' work. That is, work that was pursued "in a single stroke" is more intelligible as a discovery than work which was not\textsuperscript{101}.

In the previous chapter, I have suggested that the 'significance' of endothelin is an essential value for recognising endothelin in the first place (p. 104-107). In considering the way the Tsukuba Group's work of reporting endothelin is recognised as a discovery, the value of 'originality' may be of additional importance for this recognition (e. g. Mulkay 1985). But given that there is something which both existed and continues to exist prior to endothelin, a perception of originality might not be the crucial factor. It may instead be a recognition of the novelty of findings (Brannigan 1981: 59-60), which is more important in the construction of discoveries. Alternatively, it may be the intensiveness of scientific work, as Prof. Goto suggests, which is most crucial to what counts as a discovery. It is thus particularly interesting to undertake a comparison between the achievement of the Tsukuba Group and that of Highsmith's group in terms of how the former, but not the latter, is said to constitute a discovery.

\footnote{This explanation of the discovery of endothelin can be compared with a similar account provided by another endothelin researcher (30, p. 160, see also footnote 143).}
Reader Don't you think this is rather strange? The sequence of events relating to the discovery of endothelin is generally understood to have proceeded from the first Highsmith article (Hickery et. al. 1985), to the Tsukuba Group's achievement (Yanagisawa et. al. 1988), and then Highsmith's paper (1992), and finally to the talks produced in the interview settings. This trajectory is more or less 'chronological' in nature. But in the reading pursued by the Candidate, as well as that for the readers and ourselves, the trajectory proceeds in a rather different, almost converse fashion. Before actually conducting the interviews, the Candidate arranged the agenda to be pursued in the interview with Prof. Goto, and prepared herself for that encounter by reading his group's article (Yanagisawa et. al. 1988). She was then informed about Highsmith's work, and it was only then that she actually read the Highsmith 1992 paper. After that she obtained the Highsmith group's article (Hickery et. al. 1985). We have analysed how the object is sequentially constituted in a series of publications, but in our writing/reading, we display the different sequence employed by the Candidate.

Writer Yes. We can show a difference in the order of research between what has taken place in the course of the Candidate's own research, and what has taken place in the course of the endothelin research. This difference should be emphasised rather than backgrounded.

3-2 Highsmith's Reversion

For an examination of the 1992 Highsmith paper, I will continue the analysis with one of the analytical points taken up in Chapter 2 (p. 77-83) — that is, the act of naming.

Juliet O Romeo, Romeo! — wherefore art thou Romeo?
Deny thy father and refuse thy name.
Or, if thou wilt not, be but sworn my love,
And I'll no longer be a Capulet.

Romeo (aside)
Shall I hear more, or shall I speak at this?

Juliet 'Tis but thy name that is my enemy.
Thou art thyself, though not a Montague.
What's Montague? It is nor hand nor foot
Nor arm nor face nor any other part
Belonging to a man. O, be some other name!
What's in a name? That which we call a rose
By any other word would smell as sweet.
So Romeo would, were he not Romeo called,
Retain that dear perfection which he owes
Without that title. Romeo, doff thy name;
And for thy name, which is no part of thee,
Take all myself.

Romeo I take thee at thy word.
Call me but love, and I'll be new baptized.
Henceforth I never will be Romeo.

Juliet What man art thou that, thus bescreened in night,
So stumblest on my counsel?

Romeo By a name I know not how to tell thee who I am.
My name, dear saint, is hateful to myself,
Because it is an enemy to thee.
Had I it written, I would tear the word.

(Shakespeare 1967: 85-86, emphasis added)

For the Tsukuba Group's achievement as a discovery, the act of naming seems to be particularly vital. It seems, in this case, that the object the scientific community sanctions as a discovery seems to originate from the act of naming. Furthermore, it seems that it is this act of naming which unifies the potentialities reported prior to it, and renders the object the same.

The 1992 Highsmith paper is entitled 'From Endotensin to Endothelin: The Discovery and Characterization of an Endothelial Cell-Derived Constricting Factor'. I would first like to point out two textual features that are relevant for the reading of this text. First of all, the "scripted nature" of scientific

102 In cognitive theory, scripts are assumed to represent regular, repeated features, with which we can more or less predict events and on the basis of which we can explain anomalies in events. People come to possess schematic scripts and sub-scripts:

mainly through living in the same culture and experiencing the same kinds of routine scenarios. This kind of knowledge is clearly fundamental to any sort of cultural competence, and we surely use it in making sense of what people are doing, where, and why. (Edwards 1997: 144)

Rather than studying scripts based on this assumption, Edwards shifts the research focus onto discourse where "such notions of a particular kind of place, and of particular kinds of action sequences, are produced in specific and discursively occasioned ways, perhaps even as variable or contested descriptions": the status of scripted activities can then be examined as not "perceptual, mental, nor real, but discursive" (ibid.: 144). This position allows us to see that:

participants describe the world [as if it is scripted]. Through naming and narrating them, people descriptively construct events as following, or as departing from, some normative or expected order. . . . Events are described as scripted, as instances of some general pattern, or as anomalies or exceptions. (ibid.: 144)
discoveries is evoked throughout the text. This begins with the first sentence of the abstract:

3E

Here, the narrative of the discovery of endothelin is claimed to be an instance of such activity. That is, what will be recounted is introduced as a routine and normalised discovery-related event such as takes place in "science at its best".

Secondly, there is a textually organised contrast, made especially relevant in this narrative, that can be observed in the last two sentences of the abstract:

3F
1. Within [the] framework [of historical perspective], the early characterization and properties of this unique peptide as well as the important confirmation of these findings by other investigators will be reviewed. Some original thoughts on the mechanisms of action of the constrictor and the later events culminating in the isolation and purification of endothelin will be discussed. (Highsmith 1992: 17)

In the first sentence, "the early characterization and properties of this unique peptide" (lines 1-2) and "the important confirmation of these findings" (line 2) are presented as a sort of contrastive pair. The first element of the pair is a gloss for the work of Highsmith's group, and the second for the work of "other investigators" (line 2), successively revealed to be that of the Tsukuba Group. This contrast is again worked up with the clauses "[s]ome original thoughts on the mechanisms of action of the constrictor" (line 3) and "the later events culminating in the isolation and purification of endothelin" (lines 3-4). There is a certain tension in the 1992 Highsmith paper that is produced in and through this contrast. This tension manifests itself as the simultaneous recognition and non-recognition of the status of 'discovery' for the achievements of the Tsukuba Group.

Constructing A Novel Something Whose Origin Lies Elsewhere

Instead of presuming that Highsmith's narrative is typical of discovery stories in science, it can be examined for how it is organised to display a scripted ('typical') character.
Highsmith's narrative begins with the story of how his research group initially came across a hypothetical substance.

3G
1. Approximately 10 years ago, our studies were focused on the role of the endothelial cell (EC) in the regulation of the fibrinolytic or clot-dissolving enzyme system. Kristine Hickey (née Agricola), a graduate student working under my guidance, had completed a portion of her Ph.D. thesis dealing with the effects of changes in pH and Po2 on plasminogen activator synthesis by cultured ECS. In the fall of 1982, as a part of our weekly literature review sessions, we discussed the pioneering finding by Furchgott and Zawadzki [1980] concerning the obligatory role of the endothelium in modulating the relaxation of arterial smooth muscle by acetylcholine.
2. We were particularly intrigued by the uncertainty surrounding the chemical nature of the diffusible second messenger (endothelium-derived relaxing factor [EDRF]), which was proposed to mediate the relaxation response. At that time, EDRF was thought to be a humoral agent, a lipoxygenase derivative, or a free radical. A few weeks later, Hickey proposed a series of experiments to define more rigorously the chemical nature of this endothelium-derived factor. (Highsmith 1992: 17-18)

The above extract is taken from the beginning of the first section of the 1992 Highsmith paper. One of the prominent characteristics is the specificity of the trajectory of events. These are recounted in chronological order: "approximately 10 years ago" (line 1), "in the fall of 1982" (line 5), "at that time" (line 10) and "a few weeks later" (line 11). This chronological specificity is referred to by Woolgar as a sequencing device, which renders events as ordered along a teleological trajectory -- "as having an order by understanding the sense of their associated accompanying time points in relation to other members of the same collection" (Woolgar 1981: 259). This characterises the narrative as one productive of factuality -- that is, of the reality regarding the origin of endothelin research. Furthermore, it works as the means by which the narrative starts unfolding its complexity.

It is noteworthy that, in this respect, the above account exhibits a similarity with the text of the original 1988 article, as well as of the extracts reproduced in the previous section (3B and 3C). They all mention a novel something whose origin lies

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103 Such detailed descriptions work to build up an account of fact, as well as being crucial for the very activities in which they are accomplished (Potter 1996: 112-118, 162-173).
104 Note how the specificity of time in the Candidate's research is referred to and argued for (p. 114, 116).
105 It exhibits another similarity with the narrative given in the discovery accounts of pulsars introduced by Woolgar (1976, 1978), as the events constituting the trajectory of discovery are similarly initiated with the Ph.D. research of a female doctoral student.
elsewhere -- 'a clue' which had already existed. In all these cases, the 'clue' lay in the published literature:

In the fall of 1982, as a part of our weekly literature review sessions, we discussed the pioneering finding by Furchgott and Zawadzki... We were particularly intrigued by the uncertainty surrounding... A few weeks later, Hickey proposed a series of experiments to define more rigorously...

(Highsmith 1992: 17-18)

Following the discovery in 1980 of endothelium-dependent vasodilation by Furchgott and Zawadzki [1980], vascular endothelium has been recognized as... Recent reports have described a protease-sensitive vasoconstrictor activity in supernatants of cultured EC... We have now isolated a potent vasoconstrictor peptide...

(Yanagisawa et al. 1988: 411)

Mushakoji
Prof. Goto
Well, Dr. Takada told me that starting in that way was your idea and-
Well let's see, it was I who introduced that paper to everyone at first, at- that which we have called the 'Journal Club', I introduced it to everyone there. I eh- handed it, to some woman, and said to her "why don't you introduce this?"

(Prof. Goto, 14 March 1994)

All these texts are about the pre-discovery events.

Now, in the previous section, I suggested the importance of the value of 'originality' for a discovery to be attributed as such. The Candidate's inquiry began when she found something which existed and continues to exist prior to endothelin.

Given that 'a novel something whose origin lies elsewhere' is one of the features of (pre-)discovery stories, it may be concluded that the consequent lack of the originality attendant upon the existence of such 'somethings' is no problem at all. Rather, the existence of this something prior to endothelin is a necessary feature of discovery stories. It can indeed be argued there is no origin in a pure sense (Ashmore 1989: Chapter 7), and that 'originality' is a value that is contingently constituted for a particular occasion (Mulkay 1985). There is, then, always an event/entity similar to that constituting the putative 'discovery', and it is we who construct it as different from the latter, when we claim the latter to be 'the original'.

The Act of Naming

Highsmith's narrative then shifts its topic to his group's attempt to publish their findings.

Let us read how the work of the group first came to be sanctioned:

1. In January 1984 we submitted our characterization of the novel EC-derived peptidergic factor for publication in Science. In that manuscript we coined the term
"endotensin" to designate the source and action of the peptide. In April, we were notified that the paper would not be published, and an opportunity for resubmission was not apparent. Despite an overall positive review, two major criticism were raised. First, both reviewers felt that there was a lack of data to support specificity of the cell type involved in the secretion and/or release of the factor. Similarly, the reviewers questioned whether the results might be merely an artifact of cell culture and thought that we needed to present more evidence that such a substance might be released in vivo. No objection was raised to our use of the term "endotensin". After much discussion with my colleagues and with the editor, we decided to resubmit a revised paper to Science because the necessary control experiments to demonstrate specificity using non-EC cultures had been completed and because several preliminary reports were emerging that had a very positive and supportive impact on our findings.

(Highsmith 1992: 20)

In the above extract, Highsmith claims that their findings were rejected by Science due both to the lack of specificity of the object and the weakness of the evidence in support of their claim (lines 3-10). He further claims that the specificity had been increased by further experiments and the evidential weakness had been overcome by the accumulation of other reports (lines 12-14).

These claims seek to establish Highsmith's group's finding as not "merely an artifact" (line 8) but a fact - i.e., the existence of the peptide. What is interesting for us to note here is his statement that the name of the group's research object had been "coined" (line 2) as "endotensin" on the occasion of publishing this finding. The remark works as a declaration of the quasi-success of the act of naming one's research object, and thus as a declaration of the legitimacy of the name which refers to the hypothetical peptide. The scientist who discovers an object is the one who starts to refers to it first with, of course, a name.

Highsmith's narrative continues with the tale of his group's efforts at collecting supporting data to "resubmit a revised paper to Science" (3H, lines 11-14). Then, the story takes a tragic turn when we are informed that as a result of the draconian editing policies of the journal, the group's efforts at publication were again unsuccessful. To wit, Highsmith states that the group was "notified that the reviewers had recommended that it be published but that the editors had decided to reject it once again 'because of an exceptionally large backlog of accepted manuscripts' " (Highsmith 1992: 21). Faced with this rejection, their next decision is to re-submit their findings to a different journal.
In October our results, along with copies of the prior review from *Science*, were sent to the *American Journal of Physiology*. After minor revisions, the manuscript was accepted in February 1985 and published as a rapid communication in May. The only criticism raised in the final review was of our use of the term *endotensin* to refer to the constricting factor. A reviewer and the editor felt that it was premature to ascribe a specific term to a "factor" that had not been purified, and we had not yet demonstrated that the factor resulted in an elevation in systemic vascular resistance. By the time the reviews were finished we had indeed partially purified the factor and had demonstrated its hypertensive effect in vivo. However, weary from the prolonged reviews yet thankful that the paper would finally be published, we reluctantly surrendered and settled for "endothelial cell-derived constricting factor" or "EDCF". (Highsmith 1992: 21)

Note that this time, the naming of the research object by the group (3H, lines 2-3) was criticised (lines 3-5) as "premature" (lines 4-7) because the factor had not yet been purified. Highsmith describes the group as having been made "weary" by an unnecessarily lengthy review process (lines 8-9), and that it was this that led to the sacrifice of the name "endotensin" (lines 9-10).

I see this story as tragic for Highsmith's group. Even though their results were accepted for publication, it was tragic in hindsight in that the conditions of publication precluded the naming of the research object. Of course, an objection may be raised against my emphasis on the significance of the act of naming. Just like Juliet, we can assert "that which we call a rose by any other word would smell as sweet" (Shakespeare 1967: 85-86, see also p. 116-117) and imply that what is really important is simply that it smells sweet (whatever name it has or even without any name). Likewise, the attribution of discovery may simply be warranted by the 'actual quality' of the scientific achievement itself. Sociologists could illuminate discoveries by investigating the criteria internal to experimental activities or the systems of values as explicated in scientists' accounts. To examine whether the act of naming succeeds or not is, however, to focus on attributional work; that is, our accreditation of when and by whom *it* -- i.e., a research object -- is discovered. In this sense, the act of naming plays a large role in the scientific success106. For the Tsukuba Group's achievement, naming *it* "endothelin"

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106 The significance of the act of naming has been recognised by Robert Merton. Merton states that:

Eponymy, the practice of affixing a scientist's name to his discovery, as with Boyle's law or Planck's constant ... [may] reinforce an emphasis on the great men of science and a neglect of
resulted in establishing the existence of endothelin and in rendering the scientific community's attribution of discovery to the Tsukuba Group's achievement. In short, the act of naming and the scientific community's attributional work go hand in hand. This will further be demonstrated in the next section with an examination of accounts provided by members of the Tsukuba Group (p. 154-162). In contrast, for Highsmith's work, I will elaborate the peculiarity of the naming of "endotensin".

A Rose is a Rose is a Rose is a Rose

Juliet's words,

What's in a name? That which we call a rose
By any other word would smell as sweet.
So Romeo would, were he not Romeo called,
Retain that dear perfection which he owes
Without that title. (Shakespeare 1967: 86)

are seen as a protestation that she loves Romeo and not his name. But this protestation would not emerge if someone called Romeo did not show up107. To replace the name 'rose' with another name is not to erase what can be done by using the name 'rose' in any events which are taking place in and through the use of this name. It is a vain exercise to attempt to separate it and its name. This attempt is, however, interesting because it works in a rather peculiar way. People generally appear to use names without any concern for whether they are replaceable or not. That is, their usage of names appears to be the most economical one for the purpose at hand. Where an attempt is made to separate the name from its object of reference, this must surely be a noteworthy and

the social and cultural contexts which have significantly aided or hindered their achievements.

(Merton 1973: 215)

In this argument, eponymy is the consequence of discovery and is one of the rewards for scientific achievement. On the other hand, my claim in the above case is that the discovery of endothelin is the consequence of succeeding in the act of naming. 107 But of course, the main point is not the name 'Romeo', but the name 'Montague'. This point has been suggested by the examiners of this thesis, Prof. Derek Edwards and Prof. Steve Woolgar (my acknowledgement).
significant process. In the case we are dealing with here, the explication of such an attempt brings such issues into sharp relief: to give a name, to become a godparent of this name, and to impute the status of a discoverer to this godparent.

The last section of the 1992 Highsmith paper, "Purification of EDCF and the discovery of endothelin" (Highsmith 1992: 28-29), appears to be about a different event. As the title of this section suggests, the word "discovery" is associated in particular with the work of the "purification of EDCF". "Discovery" is not used to refer to the work of Highsmith's group, but to that of the Tsukuba Group. The term 'discovery' in "the discovery of endothelin" has a different quality from the terms which Highsmith uses to describe the work of his own group. That is, it is used to refer to the task of purifying what had already been partially purified (as well as getting the result published\(^\text{108}\)). This aim had been pursued for a long time by the Highsmith group (ibid.: 26). But the task turned out to have been successfully conducted by the Tsukuba Group, whose claim is underwritten by the scientific community as the 'discovery of endothelin'. That watershed event is briefly described in the following account:

3J

1. In about mid-April 1988, a colleague informed me that there was an article in a recent issue of Nature (March 31, 1988) by a large group of investigators from Japan that I might be interested in. Later that evening I read the paper: "A novel potent vasoconstrictor peptide produced by vascular endothelial cells." Although the title was simple enough, the content of the article was overwhelming. Drs. Yanagisawa, Masaki, and colleagues had done it all — the EC-derived vasoconstrictor had been isolated and the cDNA cloned and sequenced. Endotensin had become endothelin!

(Highsmith 1992: 29, emphasis original)

This is a vivid and dramatic narrative of the surprise with which Highsmith came to learn about "the discovery of endothelin". Note that the emphasis on the word "might" (line 2) occurs in Highsmith's original text. This conveys a degree of irony concerning the level of his potential interest in this article, since it described exactly what his group had attempted. The next two sentences (lines 3-5) express his astonishment about the event.

\(^\text{108}\) The work of "getting the result published" is, in a different but significant sense, just as important, is it not? I refer here not only to the publication of the original 1988 article, but also to the 1992 Highsmith paper, as well as (hopefully) to some parts of this text of my thesis.
There are two points of particular interest here. The first concerns the phrase "done it all" (line 5). "All" can be interpreted to have a range of variable meanings. It can refer to the task of isolating and purifying the partially purified peptide. But it can also refer to the work of appropriating all of the credit for its discovery. That is, "all" can include the community's recognition of a certain scientific achievement, namely 'discovery' (and relatedly the whole glory). Furthermore, it can be inferred that those who are sanctioned by the community to have "done it all" (line 5), i.e., discoverers, are deemed to be responsible for all the work of giving birth to the idea of this research, designing and actualising the related experiments, conducting the painstaking laboratory work involved, and negotiating with the editors of the journals to which those findings had been submitted.

The second point concerns what it is to claim that a name, endotensin, "becomes" another name, endothelin (lines 6-7). Previously, Highsmith had claimed that the act of naming endotensin took place in 1984 (3H). Thus, if we accept this claim and the claimed chronological order (that is, the act of naming it endotensin in 1984, as well as the act of (re-)naming it endothelin in 1988), then such formulations as "endotensin had become endothelin!" (lines 6-7) and "from endotensin to endothelin" (the title of the 1992 Highsmith paper) become legitimate. It gives us the picture of endotensin having been re-named endothelin. What we are observing here, however, is that this claim could be made only after the success of the act of naming it endothelin. Claiming the name "endotensin" as the pre-name of "endothelin" relies entirely upon the prior existence of the name "endothelin". Highsmith claims that the act of naming endotensin took place first, and it was re-named endothelin: but the act of naming endotensin is actually the act of re-naming the object which was given the name "endothelin" in 1988. The research object which had been named "endotensin", could never have been published as such, until it was claimed in 1992, as an unsuccessful bid. The identity of endothelin's real origin, endotensin, is thus established backwards as a narrative construction: 'From endotensin to endothelin'.
And of course, the act of re-naming endotensin in 1992, is what we can now establish in our narrative re-construction in this text.

Note this rather elegant bit of work that takes place in the construction of the sequential order. All the issues regarding endothelin are actually those of endotensin if we accept the prioritisation in this chronological sequencing. Thus, what we learn about endothelin is actually what endotensin would have been if that act of naming had been successful. All the works relating to endothelin are the same as those relating to endotensin -- except for the change of name. All the significance attached to endothelin, as well as the significance accruing to the work of reporting endothelin would in fact be attached to endotensin and the work that could have reported it.

Okay. You are saying that Highsmith's claim -- that the act of naming endotensin took place in 1984 -- was established retrospectively in 1992, and that it is thus in fact an act of re-naming. So, are you not making a claim which attempts to construct yet another sequential order, from endothelin to endotensin?

The Vice-Versa War

To make the claim that a particular name refers to an entity that is, has been, or should be, otherwise named constitutes a potential attack upon the claim to originality which that other name -- in this case, "endothelin" -- works to accomplish. It also functions to undermine the attributional work that the scientific community has been engaged in.

However, this case can be compared with that in Horton-Salway's study of how the name 'M. E.' is constituted in medical articles and talk (Horton-Salway 1998: Chapter 3, 73-102). Commenting on my point above, she explicates her case as:

The point of the historical reviews was to establish ME as the same entity as other diseases/illnesses in the past. The rhetorical point is not to undermine the discovery claim of those previous writers so much as to use this material to undermine current claims that there is a new disease entity. (So the point is, for your writing here, naming/re-naming etc. is always a situated production and may work to accomplish different business in different contexts.) Like Mind and Body (Horton Salway 1998), naming/re-naming is a largely flexible resource.

(Horton-Salway 1999, personal communication, stress original)
up to that point with its use of that name. In his final paragraph, Highsmith continues the story in a way which mitigates the threatening aspect of such an attack:

3K
1. The feeling of frustration soon gave way to a sincere appreciation for the quality and quantity of work that had been completed by the Japanese group in such a short time. Indeed, in the few short years since our initial discovery and their seminal work, continuing remarkable progress in endothelin research has been achieved. Despite the competitive nature of research surrounding the discovery of any "new" molecule, the accomplishments of researchers in this area have been fuelled by the open sharing of both data and ideas on the part of scientists from around the world. Hopefully this spirit will continue in our efforts to unravel the physiological importance of endothelin. (Highsmith 1992: 29)

The mitigating work can be observed in the remark concerning Highsmith's "sincere appreciation" (line 1) and the complimentary evaluation of the achievement of the Tsukuba Group (lines 1-3). Further, the entire scientific community of endothelin research is "celebrated" (Mulkay 1985) in stating "continuing remarkable progress" (line 3), "the accomplishments . . . have been fuelled" (lines 5-6), "the open sharing . . . on the part of scientists from around the world" (lines 6-7), "this spirit" (line 7) and "our efforts" (line 7). Here, the mitigation and celebration work to establish Highsmith's own reputation within the scientific community about which he has complained. It can also be seen as a rhetorical acknowledgement of defeat without any churlish ill will. But more importantly, this defeat in the 'discovery-war' constitutes his subordination within a scientific community that has attributed the discovery in the way it has.110

What is represented in 3K is, nevertheless, a clear statement attributing credit for the discovery of endothelin. However, there is still a tension expressed in the contrast (see p. 117-118) between the reference to "our initial discovery" and "their seminal work" (line 3). This contrast implies that the credit is (or should be) distributed between the respective scientists' groups -- i.e., the group who failed to discover endotensin and the group who did discover endothelin. Moreover, the use of scare-quotes around the word "new" (line 5) indicates that this term is used with a kind of

110 It is interesting to compare this rhetorical work with the work of the members of the Tsukuba Group in constituting themselves as being courteous and well-mannered in reminding the Candidate that they cite the work of Highsmith's group (3D, p. 115).
irony directed at the "new"-ness of the discovery of endothelin. One of the discovery criteria nominated by Brannigan, "unprecedented"-ness (1981: 68), seems to be used with some irony; here directed at the mode of attribution of scientific discoveries by the scientific community to which Highsmith himself nevertheless belongs.

Now, we can further implicate this irony in the act of naming. The "newness" of endothelin can only be legitimated in and through the act of naming. Highsmith's group during the period 1982-1987 could not succeed in the act of naming, but the Tsukuba Group could. The success and failure of the act of naming seems directly to relate to the recognition of scientific achievements. The irony here is again directed at the scientific community whose members *prima facie* accredit its existence at the point where they start to refer to it with a name.

Then, what are we to make of Highsmith's claim that "endotensin" pre-existed endothelin? It is a reversion in the act of re-naming. It could only be done, as we have seen, on the basis that endothelin had first been successfully named. Thus, to account for how "endotensin" was discovered prior to "endothelin" is an extraordinary accomplishment for a member of the community which attributes the status of discovery to the Tsukuba Group.

In Highsmith's reversion, moreover, it seems that a dual and contrasting concern is attempted. Respect is accorded to the scientific consensus surrounding the events in question, and yet at the same time, a justification, and thus an implicit complaint, is offered for his achievement not being properly sanctioned. That is, although Highsmith's narrative never explicitly states that the editors of *Science* and the peer reviewers of the *American Journal of Physiology* have done him an injustice, this is nevertheless implicit in his account of their refusal to allow him to name "endotensin".

In order to craft a historical narrative from this concern, Highsmith joins in with the community's attributional work by using the name "endothelin", but he does so with

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111 This irony levelled at the discovery and applied to "any ... molecule" (line 5), can be heard as directed at any other scientific discoveries attributed by scientific communities in general. Note my interpretation that Highsmith's narrative evokes the scripted nature (Edwards 1995; 1997, Chapter 6) of scientific discovery (p. 117-118).
irony. The two concerns strongly contend with one another and yet they are closely tied with one another here. In our reading of the 1992 Highsmith paper, then, we hear him saying that endothelin is originally endotensin: but endotensin can only be so by being endothelin: but that this endothelin is originally endotensin, and vice-versa, and so on. Highsmith's reversion through the re-naming of it disposes us to see a 'vice versa war' between the two names.

_Writer_ And of course, here we're also engaging with this _vice-versa_ war in our own writing/reading.

_Reader_ Yes! And for Pete's sake, you also seem to be attending to a particular version of this event! You're constituting Highsmith's claim as unjust because it tries to reverse the issue. Indeed, you seem to think it a rather witty analysis to suggest that the existence of endotensin can be claimed only after endothelin has been first reported.

_Writer_ Well, I'm not . . .

_Reader_ But, do you really think it's clever to _re-reverse_ the chronological sequence that Highsmith tries to establish? I know that you're doing this re-reversion just because the Candidate first came across endothelin and that only later did she come to know about endotensin. You are thus implicitly working to justify such a reading of the event, by attending to "the specificity of the time" (p. 118-119, see also p. 116) — that is, of the publication dates of sources that you use as data.

_Writer_ But the chronological order of publication dates is the most convincing piece of rhetoric for the readers, including SSKers.

_Reader_ Really? If we are to invoke SSK, "in principle" (Ashmore 1989: Chapter 6), one of the tenets of "the Strong Programme" (Bloor 1976), that of symmetry, suggests that this might be problematic (cf. Ashmore and Richards 1996).

_Writer_ To justify a sequence of events according to the publication year is one of the credible criteria. And I stress that it's only "one of" the criteria -- although it might be the most relevant one for both scientific and sociological communities to construct their knowledge.
Reader  I think that you have started by taking for granted the criterion that you prefer rather than explicating all of the criteria for scientific discoveries!

Writer  But I am not denying the other criteria. And of course, the name "endotensin" has been used to refer to 'it'. I can imagine that is how it must have been labelled in Highsmith's laboratory since 1982! But are we now, like the scientists, also mitigating what we have done in our analysis?

Reader  You might be but I'm not. My point is this: if you say that the name endotensin existed locally in order to refer to a certain object and that the name endothelin also existed locally in order to refer to the same object, how can you be symmetrical in the use of these two names locally in this text? Is it from endothelin to endotensin, or vice-versa?

Writer  But is it a matter of deciding upon the proper sequential order? It may well be that each of the names has its local historicity in the scientists' embodied works (and talk) (Garfinkel, Lynch and Livingston 1981). If there occurs any moment of confrontation regarding the two different ways of naming 'it', this occurs in a situation where these two names are recognised to represent the same object. In such a confrontational case, some criteria for judgement may be invoked (such as the publication date, or which of the two names was properly sanctioned through the publication system). However, where the community routinely use one of the names, or rather the community, without considering other choices, uses the name to refer to 'it', then even a competitor wanting to challenge the community's attributional work must do so with the use of this name, thus making it quite difficult to challenge the discovery status.

Reader  I see your point. But what I'm asking for is your (a-)symmetrical position.

Writer  Fair enough, except that I have not endorsed anyone's story beyond my own narrative construction of the Candidate's research interest. The reason I examined the 1992 Highsmith paper was for the significance of his reversion, and its implications for the attributional work in question. Together with my proposal that Highsmith's
story is "scripted" (Edwards 1995; 1997 Chapter 6, see also footnote 102, p. 117-118), Highsmith's narrative suggests to us how difficult it is to directly contest the community's attributional work with respect to some scientific discovery. Nevertheless, one can pose a question about it. In this case, by ironising the attributional work of the scientific community in which one is necessarily implicated112.

Reader Okay, okay. But let me try to make you listen (or read) this once more. You keep committing yourself to the attribution of the Tsukuba Group's achievement as a scientific discovery, by using the name 'endothelin' instead of 'endotensin'.

Writer But surely, my writing focuses on the community's attributional work. It supports neither side.

Reader No! You -- in fact all of us in this text -- call it endothelin, and what we are doing is certainly attributional work!

Writer But like Highsmith we cannot avoid that!

Reader/Writer Now, what I suggest is that we should be more epistemologically radical (Ashmore 1996: 315-316), and unsettle the attributional work for this discovery!

3-3. Inside and Outside Scientific Discoveries

(In Fiction "The Helpful Analyst"

Brannigan114 speaks:

As an outsider, trying to understand science and scientists, I cannot adopt the same 'realist' view of the natural world as you do [. . . ] while at any time contemporary theories will be held as objective and valid, this validity has a provisional or conventional character. Like discovery itself, theoretical validity is socially constructed and is likely to be superseded by later social constructions. The only tenable sociological position is that both validity and discovery are what particular scientists at particular times take them to be.

112 This form of irony ceases to be the mere assertion that "what is true lies elsewhere", and becomes close to Woolgar's 'dynamic irony', which brings a reader to awareness (Woolgar 1983: 260). See footnote 72.

113 I took this dialogue from the drama written by Mulkay (1985: 201-234), where one of the characters is a fictional sociologist "Brannigan" who takes the part of a sociologist embroiled in the midst of a controversy surrounding the attributional work of a discoverer, "Spencer", at his Nobel Prize ceremony. This character is described by Mulkay as "a young sociologist who has written about the social construction of discovery. Canadian" (ibid.: 203).
Younger speaks:

I'm not sure that you can escape so easily from telling scientists that they're wrong. […] Maybe we need to rephrase your point more carefully. Let's make it: 'Dr Spencer has been widely recognized as the discoverer.' But there are several problems with this version. First, it's a rather weak and uninteresting claim. It says nothing about 'discovery', only about the recognition of discovery. […] In rejecting what you called 'naturalistic' accounts of discovery, you seem to me to be claiming superiority for your own non-naturalistic analysis. […] Given that our accounts tend to be naturalistic accounts which treat discovery as something 'out there' in the social world, are you not claiming to know better than us?

(Mulkay 1985: 229-233)

Sociologists' Attributional Work

In this section, I will examine how the attribution of scientific discoveries is organised in endothelin research, SSK and the Candidate's research.

Thus far, we have observed that the Candidate seems to have attributed the status of a discovery to the Tsukuba Group's work. In the various extracts taken from the interview talks with the participant scientists, she seems to be sharing the ordinary sense of the object in question with the participants. Subsequently, the analysis of Highsmith's reversion (Section 3-2) reveals that her own analysis results in a formulation (that is, a 're-reversion') of the issue: from endothelin to endotensin. These points suggest that she is, in her research, participating in constituting the facts relating to the discovery of endothelin. In other words, she attributes a certain event or achievement as a discovery in a "naturalistic" manner (Brannigan 1981). On the other hand, however, when she explains her research object to her supervisor and colleagues in the social sciences, or when this text is written/read, her inquiry is centred upon 'the constitution' of this discovery. Thus there seem to be at least two ways she constitutes her research object: (1) as orienting towards the discovery of endothelin and (2) as orienting towards the constitution of the discovery of endothelin.

These double concerns further indicate that the Candidate has been both an ordinary member of society and a candidate SSKer in the course of her research. The

115 This speech is cited from a dialogue of one of the fictional characters, "Younger". This character is described by Mulkay as "[a] younger man, who supports Spencer. British but now working in the USA" (ibid: 203).
research appears to be a doubled activity\textsuperscript{116} of both knowing the discovery and explicating its constitution. But considering the relativism upheld in SSK and the question of epistemological consistency, how legitimate is this doubled activity? Is it a matter of 'ontological gerrymandering'\textsuperscript{117}? Or, can she simply share her knowledge about discovery with those who have a similar "naturalistic" bent in their sociology? How does she constitute the discovery of endothelin?\textsuperscript{118}

In order to consider this question, I will examine the most comprehensive discovery study in SSK. In emphasising the social basis of scientific discoveries, Brannigan proposes that the task of sociologists is "to explain how certain achievements in science are constituted as discoveries" (1981: 11; stress original). That is, instead of explaining why a certain discovery occurred, sociologists are to analyse how members of society conceive and account for a certain scientific event or achievement as a discovery. Mulkay points out the implication of this analytical shift:

Once the analyst has withdrawn from the analysis of discovery as action, and replaced it with an examination of discovery as a participants' method for construing action, he seems to be obliged to treat all other classes of action in the same way. For there is no realm of social action where it is possible to identify 'what really happened' without treating at least some part of members' interpretations of what happened as analytically unproblematic, literal accounts. (Mulkay 1981: xi.)

This point raises some questions about Brannigan's study of scientific discoveries. When he takes up his cases of discovery, does he take an 'unproblematic position' in order to do so, and, if so, how does this effect his analysis of the constitution of discovery?

In his book (1981), Brannigan draws attention to cases of scientific discoveries and discusses how they have been attributed as such by members of society. These

\textsuperscript{116} This doubled activity is similarly observed regarding the relationship between knowing nature and analysing scientists' accounts of nature in SSKers' writings (Chapter 2, p. 65-72).

\textsuperscript{117} See footnote 45.

\textsuperscript{118} Of course, this question is also addressed to the entire text of this chapter, including this question.
discoveries are said to be the result of attributional work on the part of such members.

Four features of the intelligibility of the phenomenon of 'discovery' are identified as grounding the perception and constitution of a discovery:

The attribution of the status, discovery, is founded on the processes of social recognition by which the announcement of an achievement is seen to be a substantively relevant possibility, determined in the course of motivated scientific investigations or schemes of research, whose conclusion or outcome is convincingly true or valid, and whose announcement is, for all appearances, unprecedented.  

\(\text{ibid: 77}\)

These four criteria of intelligibility are 'recommended' as "a set of individually necessary, and collectively sufficient conditions of discovery" \(\text{ibid: 82}\) whose intelligibility is inherent in "our mastery of language and our common stock of knowledge". Given (at least some of) these conditions, a certain event or achievement can have the possibility, or the candidate status, of becoming a discovery \(\text{ibid: 66-67}\). This is, according to Brannigan, the social basis of discovery.

Just as we have seen that SSKers need to display their background knowledge about nature (Chapter 2: 65-72), Brannigan needs to show that he knows and recognises scientific discoveries in an ordinary sense, in order to treat them as a topic. Brannigan claims that the sociologist "ought to focus his attention on the meaningfulness which animates human behaviour" \(\text{ibid: 69}\):

\[\text{[T]he intelligibility of the world is an attribution of our conceptual makeup, and this makeup is built into our language, and becomes taken for granted and 'natural' when we are socialized into a culture. This applies not merely to the concept of causality [of scientific discovery], but to all our concepts, including the concept of discovery with which we are concerned here. According to Peter Winch, 'there is no way of getting outside the concepts in terms of which we think of the world . . . the world is for us what is presented through those concepts'. \(\text{ibid: 66, stress original}\)}\]
What is essentially important here is Brannigan's stress on the knowledge which the sociologist and his readers share of the concept of discovery. Drawing on Peter Winch (1958), Brannigan points out how what we (all) take as discovery is constrained through this concept. However, in discussing how "folk" members' engage in attributional work, Brannigan's next step is to make a clear distinction between the methods of folk members and sociologists:

"While the members' accounts may be teleological, our analysis of this behaviour is not also teleological, just as an analysis of religious behaviour is not itself religious. Simply put, the members' domain is one with its objective discoveries and their relevant features, while the analyst's domain is the criteria used by members of society to attribute and reflexively uncover those phenomena -- whether these be discoveries, frauds, artifacts or whatever. The use of the criteria is teleological, circular or reflexive; however, the position which accounts for the occurrence of discoveries via the use of such criteria is causal. (ibid: 83)"

Given that the members' domain is separated from that of the sociologists, is the sociologist described above actually stepping outside the purview of these members? It seems to be so in this account. Even though the sociologists' usage is "teleological, circular or reflexive", employing precisely the same term 'discovery', this is for the sake of an analysis in which the "causal" relationship between 'discovery' and its criteria of attribution is explicated. But on the other hand, surely Brannigan does not want to collect details of any case in which folk members label some event or achievement with the term 'discovery'? Brannigan's study is about meaningful discoveries, which Collins categorised as "successful-scientific-knowledge claim[s]" in his review of Brannigan's book (Collins 1985). This can be done by authenticating his cases of discovery precisely by showing how members treat them as such.

"Even our sociological study presupposes the folkways and relies on them for its accomplishments. In other words, our practices for uncovering the social basis for discovery are already apprised of these folkways as resources. Our usage is unavoidably part of the events we are trying to describe."
Brannigan defines this recognition as "a folk act", as opposed to "a sociological description" *(ibid: 194).*

Drawing on Melvin Pollner's study of mundane reasoning (1974), Brannigan confesses the difficulty of having "access to common sense reasoning as a phenomenon precisely because it is our own idiom" *(ibid: 124).* He admits that it is difficult to draw a clear line between idioms thus shared. Furthermore, he states that in the analysis, discoveries are "undergoing a retrospective and prospective interpretation" *(ibid: 142).* Yet Brannigan is insistent in making a separation between folk act and sociologist's task. In this respect, he criticises previous theorists of scientific discoveries, such as Thomas Kuhn, on the grounds that their explanations are no different from the "naturalistic" accounts of discoveries made by folk members. Because such explanations entail "the fact that the achievement is defined as a discovery from the start" *(ibid: 40)*, they fall into being merely descriptive or circular:

The failure to maintain this distinction will result in the analyst's explanation turning into a tautology, for, it might be argued, the conditions which we specify within the criteria of intelligibility are the defining elements of discovery.

Hence, the outcome is not effected by, but defined by, the initial condition. *Voilà* tautology! To avoid this we must realize that it is the use of such elements *by members of society* which constitute discoveries, while it is our designation of this usage as members' criteria which accounts for their behaviour for us.

*(ibid: 83)*

Apparently, this failure and its tautological consequences are anathema for Brannigan: and thus his own attributional theory is presented as both a corrective and an exception119. It is, however, curious how Brannigan's attributional theory and analysis

119 "As is your own 'right here, right now'!* (in this section, if not in this thesis?)" (Horton-Salway 19 April, 1999, commenting on a previous draft of this chapter).

* "With apologies to 'Fat Boy Slim' (and Jonathan Potter, who appropriated their song title to demonstrate a point about context in a DARG session!)" (Horton-Salway 17 May, 1999, further commenting on a previous draft of this chapter).
of discovery cases can manage to exclude folk acts of any kind and remain pure sociological description. The following "imperative" distinction provides the grounds for this management:

> While at any point in time contemporary theories will be held as objective and valid, this validity has a provisional or conventional character. It is socially constructed and is likely to be superseded by later social constructions. However, as I have said, since it is historically grounded, it has a provisional validity; it is an attributed or socially constructed validity. This is what the relativistic position yields when we adopt it as a methodological device. However, ontologically, we would not want to claim that all knowledge is a function of its social context, and that everyone, the author included, is a victim of historical circumstance, and consequently that all knowledge, whether it be Azande magic, Islamic revelation, or scientific inspiration, is equally valid, or invalid. Consequently, it is imperative to distinguish between the methodological relativism of the sociology of knowledge, and the ontological relativism typically attributed to it by its critics. \(\text{(ibid: 79)}\)

For Brannigan, sociologists ought to take the approach of *methodological relativism* in their study of scientific discoveries. This, however, involves no denial of their ontological commitment to the world in which they live. This distinction is similar to that between knowing things and knowing the ways of knowing them, which we have discussed by taking up Collins and Yearley's claim (Chapter 2, p. 68-69).

However, this demarcation does not allow Brannigan to banish tautology from his attributional theory. The scientific discoveries taken as cases do not refer to anything labelled as 'discovery'. They are identified by Brannigan as intelligible discoveries, and defined as such by him, through "our mastery of language and our common stock of knowledge" \(\text{(ibid: 66-67)}\). Because Brannigan's study is about meaningful discoveries, he needs to ratify his membership in order to know about the discoveries. As Ashmore points out, "[a]ccording to the general ethnomethodological account of universal practical reasoning, tautology is impossible to avoid". Moreover, Brannigan's criticism of previous theorists of scientific discoveries, when "accompanied by Brannigan's confident claim that his own attributional theory is exempt from the
contamination of reflexive constitution", is itself subject to criticism in terms of its management of the 'reflexivity of reflexivity' (Ashmore 1989: 95)\textsuperscript{120}. In other words, to study scientific discoveries, or even their social basis, sociologists need in the first place to constitute discoveries as such in their sociological texts.

What are the grounds of Brannigan's 'imperative' separation and espousal of methodological relativism? I assume that he tries hard to evade defining any achievement as a discovery (ibid: 40) by himself as an analyst. But why? Because he is after the social basis of scientific discoveries and not their definition. The 'sociological description' of scientific discoveries is to be distinguished from any folk theories (cf. Sacks 1973). The social basis of discovery -- i.e., the candidacy status of discovery -- is grounded in members' practices, which is to be described by the theorists. The boundary between members and theorist (analyst) is imperatively maintained for the sake of analysis.

A resource that Brannigan uses to maintain his immunity to the contamination of folk attribution is 'ethnomethodological indifference' (Garfinkel 1967; Garfinkel and Sacks 1970):

For the purpose of sociological analysis, it does not matter in the end what the 'real facts' are regarding novelty, etc. These matters are only of note as considerations of members of society. ... [The position of indifference] is essential methodologically if the observer is to have access to folk phenomena as folk phenomena.

\textit{(ibid: 197)}

\textsuperscript{120} Ashmore indicates that the imperative distinction is employed for the sake of managing the reflexivity of reflexivity.

'Management strategy' is one of the categories for dealing with the reflexivity of reflexivity which Ashmore extracted from various sociological enterprises (Ashmore 1989: 94, see also footnote 3). In particular, he focuses on ethnomethodological studies which are based on the notion that any descriptive practice constitutes the described object in and through that practice -- that is, "reflexivity as the constitutive circularity of accounts" (Ashmore 1989: 32) -- and explicates how they manage the self-referential implication of this in their own descriptive practices. Ashmore's finding is that they "[tend] to level all discourses to the status of the ordinary and thus renders them a disprivileging service, [but] this is only achieved by counter-position with, and by permission of, [ethnomethodology] itself as the fully-fledged form of professional descriptivism" (McHoul 1981: 116). And playing the role of the final descriptor can only work, according to McHoul, by avoiding the reflexivity of reflexivity which requires ethnomethodology to identify its "object" domain as but an effect of its discursive practice' (1981: 117) \textit{(ibid: 94)}. 

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That is the 'Puritan Mode' in ethnomethodology which commands the separation of topic and resource (Ashmore 1989: 94). Indeed, if I follow Brannigan's declaration, the Candidate can take up the case of "the discovery of endothelin" with uncontaminated hands and analyse the attributional work of those participant scientists' in a totally detached way. But it seems that this can never be the case.

I know. But don't just assert that and persuade your readers that it is an inescapable problem -- and therefore no problem (cf. Barnes 1983). I think that is one way to avoid the reflexivity of reflexivity. Instead, I think that you need to show how this text proceeds with a "celebratory practical reflexive inquiry" (Ashmore 1989: 110; Woolgar and Ashmore 1988; see also p. 6-11, footnote 132).

I believe that the sociological understanding of scientific discoveries is related to the activities of members of society. That is, a sociologist can take a folk members' discovery as her case, and both write about the members' attributional work and convince her readers that the case of discovery is authentic for the members. She can then make this explication a sociological achievement. But given that her study of scientific discovery is grounded in the conviction that her research object is locally attributed by members of society, such a study involves doing something other than members do. Thus, the sociologist seems to be required to adopt the status of an insider-and-an-outsider of the society whose members attribute discoveries. This paradox looks similar to one explicated by Latour (1981). Latour indicates that sociologists of science are in a peculiar position, as they are not permitted to be agnostic towards science, but are still required to be outside of the scientific community in order to be able to explain it. It is a paradox because sociologists of science are required to be both inside-and-outside science. Sociologists must move both toward and away from science if they are to take science seriously. But how can this double movement be accomplished, if, in being "immersed" in our language, we have no free choice in taking different sorts of epistemological stance (Woolgar 1992)?
In social studies of science, the paradox of being both-inside-and-outside is nevertheless somehow managed by sociologists. Let me briefly examine some solutions. One of them may be that of endorsing an analytic distinction between "local settings". Those who espouse "cultural relativity" may choose this solution (see the review of the 'Enculturation' approach in Chapter 1: 15-17; see also Pickering 1995: 201-208). Within the terms of this solution, what is constituted as a 'discovery' by a particular scientific community at a certain point in time, in a particular laboratory setting, or in a conversational chunk, will be an achievement of that one local occasion. What is constituted as a 'discovery' in a particular sociological discursive setting has a different locality. Therefore, it is not possible to correlate what is constructed as 'discovery' in sociology with some other construction.

Another solution is suggested by the notion of translation proposed in actor-network theory (e.g. Callon, Law and Rip 1986; Latour 1987; see also Chapter 1, p. 27-30). Translation takes place when different domains of interest or different worlds are provided for in such a way that they neither do damage to one another nor pass each other by without some mutually beneficial outcome resulting thereby. It is "[t]he methods by which an actor enrols others" (Callon, Law and Rip 1986: xvii). It works "to solidify actor-worlds" which result in the achievement of "the seemingly natural order, where each element relates with the other" (Callon 1986: 28). If this is applied to the domains of members of society and sociologists, the paradox is simply resolved. For the members' language is merely 'translated' into the sociologists' language, and vice versa.

Alternatively, we can choose to refuse to resolve this inside-and-outside situation in the first place. This refusal is possible by not engaging with the inside/outside dilemma. This stance is purportedly maintained in ethnomethodology. I

121 Pickering introduces a kind of relativism in which the incommensurability observed in science is said to take place between two distinct regimes. In each of the regimes, "its own more or less disjoint sets of models and resources" are drawn upon "in elaborating its distinctive realm of facts, phenomena, and understandings of the worlds. Within each, then, knowledge as produced was relative to its specific cultural antecedents" (Pickering 1995: 202).
will elaborate this by taking up one of the 'ethnomethodological studies of work'\textsuperscript{122} in science, which can be interestingly compared with SSK discovery studies.

**Discovering A Scientific Discovery**

In an article authored by Garfinkel, Lynch and Livingston (hereafter, GLL; 1981), instead of outlining any insider-or-outsider position for the attribution of scientific discovery, these ethnomethodologists attend to the local historicity whereby 'the optically discovered pulsar' is unfolded as emerging from scientists' embodied practice.

GLL study the case of scientists Cocke and Disney's night's work which was tape-recorded, and transcribed by GLL using the conventions of conversational analysis. This case provides for GLL's study of (1) what it is in the night's work that had the property of 'first time through', (2) the orderliness in the local historicity of the work, and (3) how their ethnomethodological study of work extracts 'the quiddity'\textsuperscript{123} of this work. Let me briefly illustrate these three points. First of all, it is tempting to treat the tape and log notebook as representing the scientists' in-situ work, *first time through*, of finding and exhibiting the astronomical analyzability of the pulsar. However, the astrophysically cogent mathematical collection of equivalent observations which are assembled and made astronomically accountable is, in the tape and log, "only obtainable, case-after-case, as an historicized series" (*ibid*: 135). This process is noticeably absent in the scientists' article which reports the discovery. For GLL, the night's work was:

\begin{quote}

done as a lived orderliness, in real time. \ldots [W]e need to identify what consists of as their local, interactionally produced, recognized, and understood embodied practices. \ldots The demonstrable pulsar was obtainable only via the collection's local historicity of the series of Runs; \ldots somehow it was 'evolved' from an evidently-vague IT\textsuperscript{124} which was an object-of-sorts with neither demonstrable

\end{quote}

\textsuperscript{122} See footnote 39.

\textsuperscript{123} 'Quiddity' is used as GLL's technical term. The quiddity of discovering scientists is defined as "interests in, their knowledge of, and their practices that compose the *in situ* apt and familiar efficacy of the day's work" (Garfinkel, Lynch and Livingston 1981: 133).

\textsuperscript{124} GLL calls this IT as a Sacksian IT'. They introduce the discussion of Harvey Sacks in his unpublished lecture.
sense nor reference, to a 'relatively finished object'. Some-how an evidently-vague IT became another object, 'the relatively finished work of the optically discovered pulsar'.

(ibid: 135)

Thus, the night's work that GLL attempt to show is "not yet 'naturalized' in a reportable just-so story" (ibid: 136).

Secondly, GLL presume that in the local historicity lies the finding and exhibiting of an IT's adequately astronomical details as the real pulsar (ibid: 137). This is done "in the practitioners' 'hands' " which provide for the strong orderliness:

[The] orderliness offers itself in elaborating details of attempts, repairs, and discards of locally motivated and locally occasioned modifications on the pulsar's existing material 'shape'.

(ibid: 137)

Thirdly, this study is aimed at delivering "material exhibits of work in sequentially developed and technical details" -- that is, GLL's programme is to pursue a study of the discovering work of scientists, in contrast with 'studies about their work' which "are commonplace" in social studies of science (ibid: 132-133). GLL find the topics, methods, findings, and problems of naturally theoretic social studies of scientists' work to be an irrelevance: instead, for the purpose of discovering the quiddity of scientific practices, their study is aimed at describing the discovering scientists' interests in, their knowledge of, and their practices that compose "the in situ apt and familiar efficacy of the day's work" (ibid: 133).

GLL's study is particularly interesting for us because I assume that they refuse to be themselves inside-and-outside of Cocke and Disney's night's work: they distinguish the setting where the scientists interactionally produce "their interests in, their knowledge of, and their practices that compose the in situ apt efficacy of their night's

Sacks described an 'IT' that occurs in conversation, i.e., it is produced, recognized, and understood before it has a definiteness of sense or reference. 'IT' is used and oriented in that and in the way that it has no sense or reference, and thus as a way a sense and reference is achieved for 'IT', and as a condition under which a sense, definitely, clearly, after all, etc., is achieved. (ibid: 157)
work" (ibid: 134) from the analytical setting wherein this work is exhibited and analysed as naturally accountable. For GLL's programme, it seems that sociologists' concern with being both inside-and-outside is perhaps irrelevant, if not misleading, because their focus is entirely on the local production in question which, given its historicity, is a de facto constitution.

In this respect, GLL's study appears to be different from SSK discovery studies such as Brannigan's. GLL's focus is not on how scientific discoveries are attributed as such, but on how the local historicity and the situated embodiedness of the scientists' work constitute the optically discovered pulsar: the discovery status is therefore not defined nor conferred elsewhere but lies in the scientists' in situ production by means of its unfolding vicissitudes. In this programme, they frame their analysis not in terms of the socially-attributed 'causes' of this discovery, but as a matter of describing the work of discovering the pulsar. Thus the analysts' involvement in the attribution of discovery, or the problem of being inside-and-outside, is not a relevant question.

With such an approach, GLL establish their distance from studies about discovery in order to engage in the study of it. Accordingly, the study proceeds with a transcription and log, in which the scientists' object emerges from a vague 'IT' to become 'the relatively finished work of optically discovered pulsar' (ibid: 135). For example, GLL display the scientists' log notebook where the following hand-scribbled record was written:

[... ] Jan. 15 [...]
# 18 SP* Blue filter. Looks like pulsar! N 700/ [...]
[... ]
#23 Repeat of #18 [...]
#24 Repeat of #18.

(ibid: 146)

Also in the conversation (about first noticing [Appendix 3] and then the further observation [Appendix 4 and 5: 149-153]), GLL display what is observed through articulating the pronominal reference "it": the conversational sequence shows the work
that took place before the word "discovery" was used. These displays inform us that GLL's research object is not why the discovery of the pulsar occurred nor how it is attributed as such: it is the work that constitutes discovering the pulsar which they try to present.

However, GLL's work is nevertheless a study of something called a 'scientific discovery'. We can immediately see that 'discovery' is the relevant topic in GLL's article, which begins as follows:

On the evening of the discovery of the optical pulsar at Steward Observatory, January 16, 1969, by John Cocke, Michael Disney, Don Taylor and Robert McCallister, a tape recording in which they reported their series of observations was left running and before it ran out recorded the evening's "conversations" from Observation 18 through 23. . .

(ibid: 131)

Obviously, the topic 'discovery' is the very first thing that we will grasp. While we can reconstruct the night's work as a discovery that can only be examined in the light of first time through, and as obtainable, case-by-case, as an historicized series, this introduction allows us, firstly, to recognise the work as a case of scientific discovery. In addition, GLL display, in their first appendix (ibid: 143-145), the scientists' original article published in Nature entitled 'Discovery of optical signals from pulsar NP 0532'. This display comes before GLL's explication of how the work progressively becomes that of discovering the pulsar. Thus, in GLL's article, their research object is worked up to inform the readers of Philosophy of the Social Sciences of this discovery.

In this respect, we can reconstruct the text of the GLL article as working along a dual trajectory. It can be read such that the discovery is made relevant in their introduction of the scientists' work, and that this article displays itself as sociologically

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125 I have also introduced different (chronological) trajectories into this text, and questioned how it relates to my own constitution of the research object of this chapter. See the discussion of how the narrative of the Candidate's own research proceeds chronologically (p. 114), the different trajectories between publication and the agenda of readings pursued in the research (p. 116), and the analysis of naming and re-naming (Section 3-2).

126 This study also selects the case of the pulsar discovery which is well-known to SSK readers through Woolgar's study (1976, 1978).
explicating the scientists' work which progressively becomes a discovery. GLL are (1) informing their readers that the scientists' work results in the discovery of the pulsar, and (2) doing sociology by explicating how the night's work constitutes a discovery, and inviting the readers to perceive the sociological relevance and significance of the article. GLL's dual achievement in constituting the discovery and sociologically describing the constitutive work of discovery has an interesting implication: it is pursued not only by GLL, but also by us who read their article. That is, given their emphasis on 'local historicity', we are at liberty to apply this concept to the local setting in which GLL's article is written/read. It permits us, self-referentially, to realise how the local production of GLL's research object and its sociological outcome are available for us in our reading/writing. Moreover, in questioning this, we are further questioning whether we are 'competent' members of the ethnomethodologically-informed community wherein their article is read/written.

At last, the question of inside-and-outside can be addressed again. First, GLL are aligning themselves with the scientists and ordinary members of society, by reporting the scientists' work as a discovery to readers of Philosophy of the Social Sciences. Second, GLL are constituting themselves as doing sociology, by explicating how the night's work is constituted in terms of how the "embodied work's particular occasions as of which the object's production -- the object -- consists, only and entirely" (ibid: 137; stress original). This is different from scientists and members of society who would refer to the night's work as 'the discovery of pulsar'. Likewise, it may be that in a reading of the GLL article, the text is (to be) organised such that the night's work is made intelligible in such a dual fashion. Just such a reading can be made available in its local historicity according to the "transitive order of written materials on a page of text" (Lynch, Livingston and Garfinkel 1983: 206).

127 But this setting of the reading/writing a sociological article is, I suppose, an irrelevant or impossible topic in their article: it is not a topic for the ethnomethodologists' work of doing sociology, nor one which can be intelligibly discussed within the setting, leaving aside some programmatic and methodological discussion and some self-referential occasions which are inherent and inevitable in any language. I will, however, try to examine it here.
Reader I'm not quite sure whether I read that in such a transitive order.

Writer You must perhaps protect yourself from being implicated so that you are now displaying a relevant reading of the GLL article in my writing.

Reader But, if that is the case, I can start afresh with the proper "transitive order" for my reading/writing.

Writer How?

Instead of re-starting the reading, I will introduce the reading/writing of another SSKer.

The Work of Doing Sociology in Science Studies

Collins wrote a review of GLL's study (Collins 1983), which was countered by Lynch (1992). The dispute can provide an opportunity to consider the difference in stance for studying scientific discoveries between SSK and ethnomethodology.

Although he highly values GLL's study, Collins claims that it cannot achieve a certain thing:

To know what it is about Cocke and Disney's night's work that makes them scientists is a very interesting question. To know what it is that makes them scientists who believe they have made a great discovery is also interesting. But to know what it is about their work that makes them scientists who are making a great discovery, one needs to look elsewhere. What it is that made it that they were making a great discovery is to be found outside their night's work.

(Collins 1983: 105)

Collins' argument further proceeds to locate the scientists' "competence" in the case.

While GLL observe the scientists' work as an instance of "competent scientists' work", for Collins, this work

is not competent in the sense of competently performed experiment (Collins, 1975). In the latter sense, competence is assigned or denied to experimental performance, often as a consequence of whether experimental results are in accord with or disagree with the ideas of the speaker. In this sense, competence
would be described as 'competent' by one who believed in pulsars and thought they had discovered a pulsar, and 'incompetent' if it turned out that their experiment had gone wrong in some way. [...] However, in the sense of competent used by GLL, [the scientists' work] was competent however it turned out. Thus, the contents of the tape recording and GLL's analysis of it -- indeed the whole of GLL's paper -- would be precisely the same even if what [these scientists] had 'discovered' through their evening of shop work was something that was not an optical pulsar. (ibid: 105, stress original)

For Collins, 'competence' is attributed as a function of members of the relevant community's prior judgement of the epistemological status of the knowledge-claim resulting from an experiment. So, whether or not the night's work is taken to be 'competent' (in this sense) depends on whether or not the judge 'believes in pulsars'. If yes: competent. If no: incompetent. But for GLL, what it is for scientists to be scientists, is intelligible in their "embodied practices whose efficacy has achieved an ordinariness and 'equipmental transparency' that allows no call for credentials" (OLL 1981: 140). Cocke and Disney's night's work is 'intelligibly competent' in this sense. The difference between the two sociologists' positions lies in how, when and where to establish scientists' work as competently 'doing science'.

This difference further highlights an interesting difference in ways to locate the competence of a sociology of scientists' in situ work: it is the issue of whether this sociology is to be judged as a competent performance from outside of the setting in which it takes place, or to take its intelligibility as settled without raising the question of 'who' is to judge (or negotiate) and 'where' should be the location for judgement.

GLL's own "interests in, and knowledge of, their practices [of scientists and members of society]" (ibid: 133) are elaborated in Lynch's reply to Collins (Lynch 1992). Based on Wittgenstein's stricture regarding the dualism between object and representation, Lynch re-confirms the points and the programmatic shift which GLL's work made in the study of scientific discoveries. Lynch claims that Collins' comment that "to know what it is about their work that makes them scientists who are making a
great discovery, one needs to look elsewhere" (Collins 1983: 105) originates from Collins' scepticism:

Recall from [the discussion] of skepticism that "the critical move is to isolate the formulation of the rule from the practice it formulates." If for "rule" we substitute "discovery" . . . the argument transfers nicely to the case we are now considering. It is consistent with the grammar of scientific discovery to say with hindsight that Cocke et al. made the first discovery of an optical pulsar on 16 January, 1969. This formulation presently counts as a "correct" description within astronomy; it is grammatically intelligible. . . . The skeptic retains [the explanatory duality of "discovered object" and the activities of representing it] but reverses the arrows: the object does not explain its representation as a discovery; rather the reverse occurs. The representation of an action as a discovery constitutes the discovered object . . . The counterfactual scenarios (and their related methodological horrors) do not come into play when we deny the skeptic's initial move of isolating the statement from the activity it formulates. Or, to be more precise, counterfactual scenarios may indeed come into play, but not as we would freely imagine them. . . . [N]o adequate use of a statement can be made in isolation from the lived order of activities it formulates. . . . Countertaxtual scenarios do not hover idly over the night's work, as though members were haunted by the skeptic's ultimate doubts about the possibility of airtight representation. Particular doubts and methodological worries may be interjected into the course of the work, but these do not license a skeptic's global interventions. (ibid: 251-252)

Lynch is critical of the dualism between object and representation as constructed in SSK: instead of engaging in such dualism and falling into scepticism, he emphasises that GLL align their knowledge with that of scientists and members of society, and thus intelligibly use the grammar of scientific discovery. Then, the ethnomethodologists' inquiry "would have to be a vicarious one, without the benefit of an observatory" (ibid: 256).

GLL make no claims about getting the discovery out of a tape recording. But to say this does not imply that the discovery is explained by events outside the astronomers' practices. (ibid: 256)
Lynch admits that "getting the discovery out of a tape recording" — that is, "getting the phenomenon out of the data", "would be the very sort of thing Cocke and Disney were up to in the first place" (ibid: 256). However, for ethnomethodologists, to explain this activity by going beyond scientists' practices is unacceptable. Now, what are the implications of this? The stance of 'indifference', espoused by ethnomethodologists\(^{128}\), argues that making value judgements and 'glossing' (Garfinkel and Sacks 1970) object, event or phenomenon, is an irrelevant task for sociology. It is emphasised that sociologists are to produce a sociological description of members' social practices.

However, I think that the proper task of ethnomethodologists is not the members' task itself. Making knowledge-claims about discourse and its intelligibility (including its legitimacy) is not the inside members' work, but that of sociology. On the other hand, if sociologists are entirely outside, they cannot be in a position to recognise that Cocke and Disney were making a discovery in the night's work, and inform us about it in their article. There remains the question of how we can bring the discourse of members of society into a sociological text; and this reflects back to my question of inside-and-outside: in order to have an 'interest in, know, and practice' the way scientists intelligibly use their grammar, we need to be insiders: but in order to describe it in sociology, we need to be "disengaged" (Lynch 1982, 1993) from the setting where the grammar is used without any need for descriptions.

Given that this sociological description also needs to be read/written as grammatically intelligible, what kind of setting are we to engage in? My reading of Collins' reading of GLL's article is that SSKers like Collins may not read the night's work as a "competently performed observation" without attending to the justification procedures. SSKers do not generally simply endorse "grammatically intelligible" sanctions. Instead, they show that 'it could be otherwise' for what is taken by members of society to be 'obvious', 'true' and 'factual'. But for ethnomethodologists like GLL, such works reveal a kind of scepticism towards intelligible and accountable social

\(^{128}\) See footnote 54. The 'indifference' policy is also adopted by Brannigan (see p. 138-139).
practices, which is valued negatively\textsuperscript{129}. This difference comes from the parties'
different conception of what 'doing sociology' consists in. What is written by GLL in
virtue of their mode of sociology is read by Collins in virtue of his form of sociology.

**Writer** So, I suppose that each of these programmes has its own local historicity
or attends to a certain justification procedure for validating its competent performance.

**Reader** But, I'm not sure how you can convince your readers that you are
competently performing your sociology in this text.

**Writer** Read footnote 145 and Chapter 5, sections 5-3 and 5-4!

In Brannigan's attributional theory and GLL's study, it seems that the social
basis and local historicity of the work of members of society and scientists is somehow
related to the social basis and local historicity of doing sociology, which is at least
worth investigating.

**Attributing and Analysing the Discovery of Endothelin**

Thus, I will now try to present another vision for doing the sociology of scientific
discoveries.

The analysis of the constitution of discovery seems to be enmeshed in
describing the case at hand: for any kind of activity, a recognition of the materials as
relevant for that activity is required in the first place. Formulations of sociologically
constructed objects are pursued by reflexively establishing and re-establishing this
object, whilst also attempting to hold a commonsense reading/writing of this object\textsuperscript{130}.

\textsuperscript{129} However, what counts as scepticism and whether being sceptical is critical in science studies, is a
matter that is contested. The SSK move of showing 'it could be otherwise' is obviously different from
scepticism in philosophy (e.g., Morton 1977: 13-17). Instead of valuing this in a negative way, I would
like to follow a recommendation of Barbara Herrnstein Smith in this matter.

If, as I believe, there can be no total and final eradication of disparity, variance, opposition, and
conflict, and also neither perfect knowledge nor pure charity, then the general optimum might well
be that set of conditions that permits and encourages, precisely, \textit{evaluation}, . . . : that is, the local
figuring/working out, as well as we, heterogeneously, can, of what seems to work better rather
than worse. (Smith 1988: 179)

\textsuperscript{130} In the previous chapter, the SSKers' simultaneous recognition of \textit{an object} and the recognition of \textit{its constructed-ness} are discussed in terms of their knowing and analysing nature (p. 65-72).
I find it crucial for us to observe, throughout the research, the *traces* of being inside-and-outside the society whose members attribute the Tsukuba Group's achievement as the discovery of endothelin. The Candidate tries to become an SSKer and also to join the 'others'; the members for whom the discoveries are meaningful. It has been an issue for her\(^{131}\) how the act of joining those members while at the same time exposing what is meaningful for them, is to be pursued. The discovery of endothelin constitutes her research object for doing SSK, and has to do with a different set of criteria employed by the participant scientists. Due to these traces, in the course of the Candidate's research, she has been observed to take the position(s) of an insider-and-outsider of the society where the discovery of endothelin is intelligible and also of the community where the constitution of this discovery can be analysed. In this respect, she is involved in both the attributational work and its analysis\(^ {132}\).

On the other hand, I find it also crucial to be involved in the continuous and intermingled recognition of 'discovery' both as a member of society and as a sociologist. Even though both scientists and sociologists may recognise issues in a disengaged way of the "familiar partisan and asymmetric uses" of general vocabularies for describing science (Lynch 1993: 79), a series of interactions are also ceaselessly (and perhaps seamlessly) taking place. The understanding of the 'discovery of endothelin' read/written in this thesis is of course not a direct derivation from scientists' "shop work" (Lynch 1995) in their laboratories. Nevertheless, it is not entirely isolated from them. The Candidate's research continues through her relationship with some

\(^{131}\) In the course of analyses, she necessarily included the task of deconstructing her own knowledge about the scientists' work and this achievement. Her research object is recognised on the basis of 'pragmatic intersubjectivity' which is one of the three senses of 'shared knowledge' Edwards categorises (1997: 114-115, see also p. 22-23): knowledge as pragmatic intersubjectivity can be observed in the series of dialogues where the participants, including both the scientists and herself, orient mutually to some assumptions regarding what they know.

If I may be allowed to make an ethnographic comment, the Candidate has experienced her reflexive analyses as a 'pilgrimage of pain' from the outset. But still...\(^ {132}\) She struggles to take a position which is close to what Ashmore calls "celebratory practical reflexivity" in his explication of a sociological form of counter-attack against relativism — that is, "*tu quoque*". He explicates the form of argument, where one's ground is exposed to have fallen into the fallacy against that which one is attacking, and one that strategically manages the potential attack. This explication is pursued with the simultaneous display of the form of *Ashmore's own* argument, which "suggests a going beyond" (Ashmore 1989: 87-111). It then results in both distillation and dissolution of the form of academic activity which we call arguments.
endothelin researchers and with her occasional participation in their conferences, meetings and informal reunions. Such interactions between the Candidate and endothelin researchers will render her writing/reading not totally disengaged from, but to some extent an extension of their laboratories. It is thus that "no discrimination will be possible between ['everyday'] discourse and scientific, mathematical, logical, or any other kind of 'formal' discourse" (Barnes and Law 1976: 226).

The boundaries of the applicability of the word 'discovery' have been "indeterminate, negotiable and subject to change" (Heritage 1984: 145): the research object of this thesis is constituted without finality and conclusiveness, given indexicality (Barnes and Law 1976). Thus, at any point, 'the discovery of endothelin' has been referred to as the same object, which still, if taken up in an analysis, ongoingly becomes differentiated. But the more important point is that the research object becomes known in the course of her research: it is being continually enriched, in and through its accumulating intertextuality (Chapter 2, footnote 82). People are not only changing the world by using the word, but also creating the object on the basis of the words in use.

133 'Indexicality' was introduced by Garfinkel (1967) and is one of the essential notions in ethnomethodology. He draws on Wittgensteinian argument regarding the word-world relationship, and explicates it as a practice. Any account of 'the discovery of endothelin' is to be seen in interaction "in which the specific sense and reference of a word is relative to the precise context" (Edwards 1997: 100). Thus, what is described as this discovery is "to be understood by reference to where and when etc. they occur" (Heritage 1984: 140). That is, it is most suitably understood as a practice which is consequential.

134 What is said always becomes different from itself, i.e. as "difference" (Derrida 1973, 1978). That is exactly what is taking place with reference to everything in this text. The Candidate's trajectory for making claims, includes the articulation of 'the discovery of endothelin' in the interactions with her participants, with her social science colleagues and in the writing of this thesis. Is what she is facing a problem inherent in writing itself? She tries to expose these issues in the hope of writing an SSK thesis, as this research domain seems to be appropriate in its tolerance of "celebratory practical reflexive inquiry" (Ashmore 1989: 110). But there is also another issue that matters. The text is expecting various sorts of readers. As well as the examiners of her thesis and SSK readers, this text is designed to be read by the participant interviewees, by sociologists other than SSKers, by her family and friends, and perhaps by some as yet totally unknown categories of reader. As noted previously (footnote 9), writing is always faced with its reading. Also, the one who is writing is always faced with the one who is reading... Then, what would be the local historicity here?

135 Regarding this point, my vision is closest to what MacMillan refers to as "spiralling" (MacMillan 1996; see also footnote 14).

136 To write that words and worlds are co-constitutive is not necessarily the same thing as engaging in a dualism between object and representation (Button and Sharrock 1993). What I am saying here is not that we can analytically examine the relevant epistemological issues by employing the distinction between words and worlds. Instead, I am saying that whenever the word "discovery" is used, there is an
As Mulkay claims, when words are so much embedded in our articulation of the world, this can lead to confusion. This might be the case in sociology in particular due to its closeness with ordinary language (Mulkay 1985: 128-129). Thus, our aim here may be to arrive at an explication of how a certain grammar is engaged in another related grammar (cf. Lynch 1993: 159-201). It is the task of sociologists to perceive and articulate their research object which is a practice they share in the first place with ordinary members (Barnes and Law 1976; but see also Garfinkel and Sacks 1970).

They then use 'it' in order to conduct an encounter with participants, to analyse the texts of members' accounts, to polish their own understanding with colleagues, and to present that understanding to the community of sociologists. And perhaps that is not all. This understanding may be perceived as an achievement in sociology.

The Candidate is observing the dynamic process in which "discoveries are not just static 'perspectives' on an event, but are retrospectively and prospectively organised and objectified social statuses" (Brannigan 1981: 129). Such processes are also self-referential in her research. Earlier I have introduced the intertextuality engendered in each setting for the recognition of any object (footnote 82) and that the object is always recognised as differentiated (i.e., it is the same as, and different from what is referred to with its name). These notions are relevant to the extent that 'the discovery of endothelin' is knowable in the course of her research. It is not that she has a variety of ways to know it, nor that the settings in which she knows it are to be separated from the setting in which I analyse it. It is rather that I know it in and as a process.

Sociologists, including Brannigan, GLL, and the Candidate, are likely to recognise and articulate such discovery in an ordinary sense and also take it as a topic for sociology. However, with the latter usage, they mainly aim at describing how the status of discovery is attributed by other people or how the scientists' in situ work of observation is the work of discovering the pulsar. So, what are the implications of this immediate potentiality of that word situatedly working, which can be observed with the intertextuality engendered on a particular occasion.  

137 Dr. Horton-Salway has suggested me that the Candidate "is not only observing, but also constituting it as such" (19 April 1999, personal communication).
for sociology? If some persons are sociologists, are they then not entitled to be ordinary members? I believe that they are. That is, they can contingently establish themselves as being incumbent ordinary members of society, while they can also specifically establish themselves as being incumbent sociologists (Sacks 1992: 40-56) whose task is to describe *other* members' work. In both of these situations, they are "doing being ordinary" (Sacks 1984; Edwards 1997: 71-72). Finally, is this question of mine ordinary in sociology? I believe that it is: but that is also up to you.

**Scientists' Sociology of 'The Discovery of Endothelin'**

It is not only sociologists but also scientists who (occasionally) discuss how scientific discoveries are attributed by the community. In the Candidate's research, it is not difficult to find the occasions on which endothelin researchers analyse how the discovery of endothelin is organised into being a discovery. What can be observed in these cases is that scientists do not naively claim an occurrence of a scientific discovery with a realist's stance. As Latour points out, scientists frequently enjoy their autosociologies (Latour 1981; Latour and Callon 1992; see also Lynch 1993: 315-319), and indeed endothelin researchers seem to have their own sociologies. Given these sociologies, it is not surprising that not only sociologists but scientists take the

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138 In terms of the 'Membership Categorization Device' (initially, MIR [membership inference-rich representative] device), Sacks illustrates a device or apparatus used in conversations. The categories are distinguished from such sociological concepts as 'group' and 'organisation'. Members of society choose among the available sets of categories in order to produce activities, see activities and organise their knowledge about them so that they can grasp and let others grasp some event. It is "a basic mechanism of social control" (Sacks 1992: 48).

139 What is the local situation here in this text? Am I not making a sociological explanation of some kind? What of the local conditions of writing (and reading — of yours) this text in which we inevitably need to use the word "discovery" for the Candidate's research object?

140 In explicating how sociologists are both insiders and outsiders, Latour makes the following comparison which indicates that scientists are also insiders and outsiders:

The sociologist explains what his informants are doing. But his informants do that as well. In fact, his informants do their own autosociology, and they do so in order to go about their work. When a project interests a given scientist, he must justify his interest. How does he do so? Contrary to our expectations, he does so with reference to social and personal factors.

(Latour 1981: 208)

In this way, scientists are observed to be constantly engaging in their *autosociology*. Latour suggests that such 'outside' concepts are used by scientists to account for fact construction.
constitutions of scientific discoveries as their object. Let me introduce the scientists' sociology of the discovery of endothelin.

The first extract shows Prof. Kimura's analysis of the attributed status of the discovery of the Tsukuba Group's work. As we have seen in the previous section, it is Prof. Kimura who mentioned the pre-existence of Highsmith's work prior to the discovery of endothelin. The talk begins by invoking the potential problematics of the discovery status posed by it.

In the above talk, Prof. Kimura questions whether the role of the Tsukuba Group may turn out to be that of a mere "developer" (lines 583-585) and whether the discovery will be "all right or not" (line 584). Although he establishes that they "achieved it" (lines 583-584), he expresses that the "feel" of "originating it" (line 594) is diluted in their case. He further questions whether the Tsukuba Group's achievement is something about which he should "feel content" (lines 603). These remarks indicate Prof.
Kimura's awareness of the contingent nature of this discovery. In these remarks, the meaning of originality in scientific achievement is interrogated (lines 596-599). This inquiry is followed by his own analysis of the community's attributional work. Here, the Tsukuba Group's work and Highsmith's group's work are implicitly compared (lines 608-610): the failure of Highsmith's group, and its consequent "low evaluation" (line 610), is glossed as a matter of 'knowing' without 'doing'.

In the above talk, even though the discovery status of the Tsukuba Group's achievement is called into question, the talk itself works to establish this status as pre-given. That is, Prof. Kimura's sociology does not itself work towards cancelling the discovery status of the Tsukuba Group's achievement. The community's attributional work is thus problematised, yet the problematisation itself serves to consolidate the de facto status of the discovery. What is problematic in the community's attributional work is so for Prof. Kimura because this is the community to which he belongs.

Scientists' sociology of scientific discoveries is not only directed at consolidating a discovery status. In the next extract, both the scientist's sociology and the work of projecting the potential status of his achievement are developed. The extract is from an interview in which Prof. Goto explains to Mushakoji how the community attributes his achievement as a discovery. Before the extracted talk, Mushakoji had introduced to Prof. Goto two 'stories' for understanding scientific discoveries. One is that scientific activities can be regarded as discoveries when they are recognised to have solved one of the core problems that exist in the relevant research area (that is, Kuhnian problem-solving [Kuhn 1962]). The other story is that, with hindsight, the notion of discovery is used to make sense of a particular scientist's

141 Likewise, as we have seen, Highsmith jointly problematises and confirms this same attributional work (this chapter, p. 126-131).
142 In my observation of the participant scientist's 'projection' towards what is becoming and what is in process in these extracts, there may be raised such a question as: am I not bringing in something from outside the texts which I analyse? The answer is no and yes. No, because it is in these texts that anyone can observe how the participant scientists characterise the outcome of their activities as not pre-given, established, nor settled. It is more that seeds are observed to be growing. Yes, because everyone brings their own reading(s) into these texts. See Chapter 4 for the two angles of projection and retrospection in my analysis.
143 We have previously seen another explanation of the discovery of endothelin provided by Prof. Goto: "[T]he reason that 'the discovery of endothelin' was achieved by Tsukuba University resides somewhere in the fact that it was at a stroke proved at our site" (3D).
contribution to a research area (that is, a kind of hero story). Just prior to the extract, Mushakoji had expressed her doubt that the case of the discovery of endothelin fits these stories. To this, Prof. Goto replies:

3M
459. Prof. Goto Our situation is, after all that. I think the same.
460. Mushakoji Really?
461. Prof. Goto We extracted it (endothelin) for the time being and it rapidly brought up blood pressure [that was] known- eh- found-
462. Mushakoji [Yeah.]
463. Mushakoji Right.
464. Prof. Goto Nevertheless, whether it actually does it, in the body, that sort of thing-
465. Mushakoji Oh.
466. Prof. Goto you see, this is not known.
467. Mushakoji (.). Not known- is that not known?
468. Prof. Goto It is not found. Yet. Whether it is a real cause of certain diseases-
469. Mushakoji Oh.
470. Prof. Goto If that [is known] then it will be "oh. After all, the research at that time was
471. Mushakoji [That- ]
472. Prof. Goto great-
473. Mushakoji Oh.
474. Prof. Goto It will be like this, you see? So, therefore, along the idea of "let's do our best at working on it", there are many people who stick to it and are working on it.
475. Mushakoji Hmm.
476. Prof. Goto In short, concerning human diseases, there are too many of them whose causes are unknown. So, it follows that we should say "now this may lead to an explanation of that disease."
477. Mushakoji Oh.
478. Prof. Goto This is especially true concerning the high blood pressure.
479. Mushakoji Well, that means that although the in vivo experiments went well, it has still not been [found, has it?]
480. Prof. Goto [It has not been found.] Those studies are at best conducted from the outside- eh- they are the same as taken from the in vivo-
481. Mushakoji Hmm.
482. Prof. Goto But in practice it was done merely by injecting our compound [1 and 1] and by observing the functions, wasn't it? [2 So, talking about 2] whether what is generated in vivo actually constricts vessels and whether it does something,
483. Mushakoji [2 Mm. Mm hm. 2]
484. Prof. Goto [3 has not become clear yet. 3] Yeah.
485. Mushakoji [3 Oh. I see. Up to that point- 3]
486. Prof. Goto [It is not yet-]
487. Mushakoji Yeah.
488. Prof. Goto So, at the point at which the result is found, probably, then, with hindsight-
489. Mushakoji Right.
490. Prof. Goto Eh- this can- give [1 a sense of 1] the discovery
491. Mushakoji [1 Actually, after all 1]
492. Prof. Goto [2 at that time and besides 2] it uh- results in indicating that it solved
493. Mushakoji [2 Mm. That's it 2]
494. Prof. Goto [3 the situation in that 3] we did not know [4 what to do 4] about the
495. Mushakoji [3 Yes. Right. 3]
496. Prof. Goto [4 Right. That's right. 4]
497. Mushakoji problems of many diseases [5 such as high blood pressure- 5]
498. Prof. Goto [5 Right. That's right. 5] By becoming
499. Mushakoji so, I say, in a true sense, this will be not simply epoch-making as such, but this- eternally- how shall I put it? It will indeed last as an incredibly significant piece of research, you see.
500. Prof. Goto Yeah. Oh.
501. Mushakoji If it is so, you see, really, it may well be that Masaki-sensei ((the head professor of the Tsukuba Group in 1988)) wins a Nobel prize.
Here, Prof. Goto's formulation is that the Tsukuba Group's achievement is still in the process of becoming a greater discovery: it is progressively acquiring a greater status in the community's ongoing attributional work. That is to say, as the research advances, its potential significance which so far has only been clear in experimental sites will gradually be demonstrated in human bodies, to the extent that its implications for human disease is clarified (lines 465-473).

Prof. Goto's account in the above extract ingeniously responds to Mushakoji's doubt regarding the discovery status of the Tsukuba Group's achievement. In a way that fulfils the two discovery stories she mentioned, the discovery of endothelin is established to be in the process of becoming a discovery which will have been a problem-solving discovery when it is looked back upon in retrospect (lines 471-473). It is also established that this discovery is in the process of becoming one achieved by the hero, Prof. Masaki, as he has already been tipped to win a Nobel prize (lines 511-514). In this way, what is gradually emerging is constituted as a discovery "in a true sense" (line 507).

In the extract, Prof. Goto is not only examining how the community's attributional work regarding the discovery of endothelin is organised. But also, in doing that as his sociology, he is working to make available a certain scientific status for the Tsukuba Group's achievement. Thus, the talk is oriented both to the analysis of the community's attributional work and to the projection of the discovery status in question. These two kinds of work are overlapping and intermingled (or rather, they are all one). Moreover, Prof. Goto is instructing Mushakoji how to understand and recognise the discovery of endothelin as a discovery. Just as we observed the involvement of the interviewer in the previous chapter (p. 97-107), it is not only the scientists, but also the interviewer Mushakoji, who consolidates and projects the discovery status.
A Prescription for Making a Discovery and Pulling It Down

In the previous section, Prof. Goto's sociology proposes that the community attributes discovery status to scientific achievements, not only when the significance of these achievements is well-established and consensual, but also when these achievements have a preliminary or limited discovery status. When the achievements reveal a greater significance, the community's attributional work is in parallel orienting towards attributing to them a greater status. Thus, the attribution of discovery status develops in tandem with the development of the research, which will only be visible retrospectively (lines 471, 473). According to this sociology, the more the research advances, the more definitive the community's attribution of its discovery status becomes. The community's attributional work is thus temporal and reflexive to the development of research.

Now, I would like to point out that the value of 'significance', which was suggested as one of the essential criteria of recognition (Chapter 2, p. 104-107), is again nominated as a crucial criterion for the community's attributional work. The following remark by Prof. Goto expands on this:

3N 461. Prof. Goto The structure of an object, its relatively modest activity, the kind of gene that it is- if you just look at an object itself, it is not really all that interesting.
462. Mushakoji Hm.
463. Prof. Goto Because there are millions of objects which exhibit physiological activities.
464. Mushakoji Yes.
465. Prof. Goto On the other hand, however, eh- uh- That this one has a surprisingly unusual nature which makes it- well, naturally enough, it really stays for a long time or, that is, really grabs everyone's attention.
466. (Prof. Goto, 14 March, 1994)
467.
468.

It is explained that the scientific work of merely finding an object is "not all that interesting", because there are plenty of trifling objects which can be identified if scientists wish to do so (lines 461-462, 464). What determines the scientific status of such work is the significance of the object, in terms of how "interesting" (line 462) it is and how much attention it will get (line 468). Its significance will thus be one of the determinants that establishes its scientific status. It helps to determine whether a certain
work is a discovery or a mere finding. It further determines the degree of greatness of a discovery.

The temporal and reflexive character of the community's attributional work, in parallel with the significance of the discovered object, is observed in the accounts of other endothelin researchers. For example, in the following extract, the status of discovery is held to be developing and subject to change, along with the community's attributional work:

30  
382. Prof. Masaki So, you see, I have been- I'd already had this kind of experience before and-
383. Mushakoji Mm.
384. Prof. Masaki And so I'm like, "this'll certainly generate eh- attract everyone's attention", I thought. "We will be forgotten about soon enough" in all of it.
385. Mushakoji Mm.
386. Prof. Masaki So, it's very important to pursue- do the initial tasks by oneself. I suppose that's the reason that (indistinguishable).
387. Mushakoji Uh huh.
388. Prof. Masaki To initiate the first tasks, to pursue- to pursue them like all at a stroke is rather important. I was well aware of that even then at the time. So, I myself went after it like all at a stroke.
389. Mushakoji Mm hm.
390. Prof. Masaki So, we were just "boom boom", here and there, in various places, we distributed the sample and (indistinguishable).
391. Mushakoji Mm.
392. Prof. Masaki So, it is- thus- the fact is that this research, that of endothelin, is an achievement that we ourselves obtained,
393. Mushakoji Mm.
394. Prof. Masaki that cannot seriously be denied by anyone.
395. Mushakoji Mm.
396. Prof. Masaki But, I'll tell you, even this point is being made open to question. I mean, now that the work on endothelin has spread out all over the world at a burst
397. [and ] so it's gone beyond our control, hasn't it?
398. Mushakoji [Mm.]
399. Mushakoji Yeah. That's [right.]
400. Prof. Masaki [It's ] gone way beyond our control, [hasn't it? And so,] at this stage, people like us get slowly forgotten step-by-step- so, we will become isolated. Very likely.
401. Mushakoji Well,
402. Prof. Masaki Unless we keep working on it all the time.
403. Mushakoji Right.

(Prof. Masaki, 7 November 1995)

What Prof. Masaki proposes in the above extract seems to cohere with Prof. Goto's account (3D). That is, to pursue the initial task at a stroke is important for making a discovery and becoming a discoverer (lines 390-392). If it is not fulfilled, one's initial work of finding a significant object will be taken over by others. In his estimation of

144 This explanation also interestingly contrasts the Tsukuba Group's achievement with the case of Highsmith's 'endotensin' (Section 3-2). We observed that even though the work of the Highsmith group might be that which initiated the whole area, it could not be credited with having pursued the whole task.
the current and future status of the Tsukuba Group's achievement, he establishes that the community's attributional work is still in process and thus changeable (lines 407-409, see also 3P below, lines 463-466). In order to maintain their discovery status, the members of the Tsukuba Group will need to keep working on their research object (line 411).

There is one additional finding in Prof. Masaki's sociology. It takes the act of naming as a relevant concern. In the continuation of the talk following the extract above (3O), Prof. Masaki introduces the example of his work on a substance other than endothelin.

3P
436. Mushakoji So, my impression is like how one works to- well, would you call it "attribute"? It is itself a very complicated and [subtle issue.] 437. Prof. Masaki [Yeah. ] It is what- whatever the case. Especially, for Americans, that is [quite the case.] 438. Mushakoji [Well, ] 439. Prof. Masaki They deliberately do not cite that- the article, [you see?] So, because of that 440. Mushakoji [Well, ] 441. Prof. Masaki kind of thing, you see, in the course of time, someone who names it something else steps into the limelight (laughs). So, it is um- well, I will tell you about um- this- another discovery of mine, it is about something called (emproten?). 442. Prof. Masaki That's something that I also discovered. 443. Mushakoji Mm. 444. Prof. Masaki There was also successively em- someone called (Ettenberger?) in Switzerland- 445. Mushakoji Mm. 446. Prof. Masaki it emerged that it was given another name by him. And so, well, it was given another name. 447. Mushakoji Well- 448. Prof. Masaki However, even though I still don't know how it came about in this way, the people in America, you see, they protested [on my behalf,] by saying stuff like 449. Mushakoji [Oh. (laughs) ] 450. Prof. Masaki "but that [[has already]] been named "(emproten?)" by Masaki". (laughs) 451. Mushakoji [[That's why-]] 452. Mushakoji Oh, right. (laughs) 453. Prof. Masaki In this way, the name of (emproten?) is- that very name itself- you see, still managed to remain- the name itself remains, after all. 454. Prof. Masaki Oh. 455. Mushakoji You know, this is the kind of thing that happens. So, you see, basically, that's the current trend. People just continue to ignore the original, see? You know? 456. Prof. Masaki And in the same way, endothelin is no exception, you see. We are getting less and less attention, I suppose. I suppose that i'll probably end up that way. (Prof. Masaki, 7 November 1995)

The episode Prof. Masaki discusses illustrates his observation of scientists' behaviours:

(1) deliberate neglect in citation practices (line 441); (2) the consequence of re-naming at a stroke. Prof. Masaki's account informs us that after some bitter failures in the past (line 382), he had started his kind of sociology of scientific discoveries in order to understand better how his own achievements were likely to be recognised as a discovery.
something that has already been named (lines 441, 443-444). Both of these are associated with his community's attributional work. Prof. Masaki attributes the first behaviour to the ways of American scientists. He then starts to explain the second behaviour in terms of his own experience (lines 444-461). Prof. Masaki's act of naming, which was at one time a success, was put at risk when another scientist tried to (re-)name this object, and thus to hijack the discovery (lines 448-449, 451-452). But the name Prof. Masaki gave was deemed to be legitimate by American scientists (lines 454-455, 457). This remark works to mitigate his previous criticism of the manners of American scientists. In this sociology, the success in naming something new, the community recognition displayed by citing this name, and the tie between the name and the scientists' achievement, are established to be crucial for the achievement of discovery status.

Sociologies of Scientific Discoveries
The sociologist Brannigan's sociology of scientific discoveries proposes the importance of the criteria of intelligibility with which discovery status is conferred by members of society. GLL's ethnomethodological study of the night's work shows us how the in situ work of scientists is constituted as 'first time through', and as having the properties of discovering the pulsar, in their embodied practices and their local historicity. The endothelin researchers' sociology informs us that the community's attribution of the discovery of endothelin is accomplished with the recognition that the Tsukuba Group pursued the initial tasks at a stroke. It also suggests that the recognition of the significance of endothelin is crucial for its discovery status. Furthermore, there is an indication that the act of naming plays a vital role in this attribution. This final point supports the analysis provided in Section 3-2, where another account of discovery -- of my own -- proposes the act of naming as crucial for the attribution of discovery.\(^{145}\)

\(^{145}\) But am I merely aligning these sociologies of scientific discoveries? Am I not also drawing a conclusive sociology from them? No. I will leave them to their own sociologies. To invoke any kind of "standard notions of positions -- perspectives, tools, rationales or approaches" is to justify one's own. This is the stance that Woolgar calls 'Positionism' (Woolgar 1992). Instead of using Positionism and claiming a 'post-attributional' or whatever new theory, I would like to

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Now, I will make two kinds of comparison between these sociologies of scientific discoveries.

(1) Brannigan's attributional theory and endothelin researchers' sociologies

With respect to the attribution of discovery, there is one prominent difference between Brannigan's attributional theory and the endothelin researchers' sociologies: it is the difference in their ways of relating discovery and its conditions. Brannigan searches for the conditions under which some event can have candidacy status for a discovery. This candidacy status of an event is, for Brannigan, the social basis of discovery (Brannigan 1981: 66-67; see also p. 132-138).

On the other hand, in the sociological framework which the members of the Tsukuba Group use in their accounts, the discovery status of their achievement is the ground on which they stand. They are interested in how the scientific community to which they belong, currently and ongoingly attributes (greater) discovery status to their achievement. In other words, the endothelin researchers search for the conditions under which the Tsukuba Group's achievement is a discovery and will become a greater discovery. It is the social basis of this discovery which they try to formulate on the grounds of a pre-given discovery status. In sum, for Brannigan, discovery is a potential state when the conditions he proposes are present (ibid: 82), and for the endothelin researchers, the discovery itself provides the occasion for them to work up its potential.

take a step into explicating the attributional work of discovery pursued by members of society, including the analyst, with the conviction that this analyst stands in an engaged relationship to the world (subjects, objects, scientists, things) (ibid: 334).

By including the analyst, particularly by including myself as an analyst, my position of non-positionism would be one of trying to escape from replacing the members' and previous sociologists' versions of scientific discoveries with a new version of my own -- that is, 'ontological gerrymandering' (Woolgar and Pawluch 1985). My attempt is to do sociology, and in doing so, to explicate sociologies including itself. Although it is paradoxical to take such a 'position' of non-Positionism, this can be formulated as the consequence of taking both inside-and-outside positions. Namely, I am trying to let all the positions proceed without dismissing any of them (as an outsider), while trying to define such a task as itself doing sociology (as an insider). See the explication of the inside-and-outside paradox in the next section (p. 165-168).

Instead of using 'Positionism', it is crucial for me to appreciate "a critical dynamic", a "dynamic of iterative reconceptualization" or "dynamic irony" (Woolgar 1983, 1991, 1992) in the Candidate's attributional work, and in doing the sociology of attributional work. Therefore, rather than drawing any conclusive sociology, I aim to relativise all these sociologies. A criticism of my 'non-Positionism' will be taken up as a topic in Chapter 5, 5-4.
The crux is that for the endothelin researchers, the scientific status of the discovery of endothelin has been ongoingly developing in process, and it is precisely their attributional work that fosters this development. Furthermore, the extracts displayed in this chapter suggest that this discovery is attributed as such by ongoing discursive work that consolidates and projects the existence, nature and significance of endothelin and its research as pursued by the participants. Even though discovery is a meaningful action embedded in language, and constituted in common sense practice and through the historical process (Brannigan 1981: 69), scientists work hard to maintain the 'black box' of discovery (Latour and Woolgar 1986; Latour 1987; Collins and Yearley 1992b). Namely, the discovery of endothelin has been managed and maintained in an ongoing constitution, worked up by members, including the endothelin researchers and the interviewer Mushakoji. Likewise, as we discussed in the previous chapter, this ongoing constitution has been inevitably taking place in reading and analysing the accounts of the discovery of endothelin. In other words, our attribution is in process, and we are all actively participating in it. None of the participants separate the discovery and how it is constituted (i.e. Brannigan's 'criteria of intelligibility').

(2) Scientists' Sociology and Sociologists' Sociology

We have observed that scientists' sociology is pursued through the in situ work of consolidating and projecting the scientific status of their own achievement. Given that they take the role of insiders and of outsiders in their accounts, sociologists doing sociology can also be considered as insiders-and-outsiders. While discussing members' attributional work and scientists' discovery work, they are participating in the members' activity of identifying discoveries, and informing their readers of this knowledge. They are like scientists, consolidating or projecting their own (ordinary) uptake regarding the case in hand, and thus, in turn, persuading their readers to be involved in such attributional work. It is therefore in their reading/writing sociology, that they constitute cases of discovery as ordinary, socially constructed, and so on

146 This, my latest sociology, is insinuating the ordinary sense of the sociologists' case.
sociologists can become both insiders-and-outsiders, and invite us to be so in their/our writings/readings.

An Insider/Outsider Paradox

Given that any one of us is a participant in the attributional work of discovery, sociologists are managing to do the analysis of this work in tandem with engaging in it. With respect to being inside-and-outside, this management is formulated differently in Brannigan's theory, GLL's study and in the Candidate's research. Brannigan seems to be both inside-and-outside without explicitly recognising it. This is made possible by commanding an 'imperative' (yet impossible) separation between the domains of the sociologist and the member. In GLL's case, they presumably would refuse to draw a distinction between being inside-and-outside for their analytic activity. Inspired by Wittgenstein and taking the 'ethnomethodological indifference' policy, their project entails a thorough concentration on the study of work and not about it (Garfinkel, Lynch and Livingston 1981).

In my own sociology, first of all, the Candidate seems not to have drawn a neat distinction between the methodological domain where she is to analyse the constitution of the discovery of endothelin, and her own ontological commitment to this discovery. Nor does she refuse the inside-and-outside.

Being inside and outside is a paradoxical position. But it is not something to be imperatively separated or avoided. Its ground can be nicely illustrated with the scheme of 'A Strange Loop, or Tangled Hierarchies' (Hofstadter 1979) which was briefly explained in Chapter 1 (p. 6-11). The Candidate's situation of being both inside-and-outside can be formulated as follows: she needs to be, and has been seen as, an insider member of society, but her activity of analysing other insiders' activity makes her look as if she is stepping out of this activity and doing sociology as an outsider: thus, she appears as an insider-and-outsider. This can be seen as 'A Strange Loop, or Tangled
Hierarchies' paradox. I believe that such a paradoxical situation is not idiosyncratic in sociology, but actually one of the activities people frequently engage in.

We have discussed Brannigan's injunction to maintain the purity of (sociologists') methodological relativism uncontaminated with (members') ontological commitments, and also noted GLL's emphasis on 'studies of the scientists' work'. But they nevertheless both inform us of the members' interest, knowledge and practice through their mastery of members' language, and also analyse the members' attributional work or describe the scientists' discovering work. In this sense, they are implicated in 'A Strange Loop, or Tangled Hierarchies'.

But what is, after all, this distinction of inside-and-outside? Of course, it is a formulation made by insiders -- such as myself, in the case of this text. But is it correct to write that it is made by "myself, in the case of this text"? No, not at all. It is precisely in this reading/writing that such a distinction can be made. Please remember that 'A Strange Loop, or Tangled Hierarchies' is a view taken from 'the Inviolate Level' (ibid: 689). From this level, a view of 'A Strange Loop, or Tangled Hierarchies' is always available (Chapter 1, p. 6-11). Here, this text is the Inviolate Level. It is neither 'the Candidate', 'I', nor 'you', the reader who makes this distinction. Only when such a distinction is made on this level, can 'you' and 'I' become insiders-and-outsiders of those members whose activities are analysed. The distinction is entirely situated.

It is important to emphasise that proposing this 'Inviolate Level' is not to presume any meta-level nor 'infinite regress' (Chapter 1, p. 5-13): my point is that any paradox is formulated as a paradox (or not) in a particular setting with its own contingencies. Note Ashmore's 'cornflakes packet example':

On your breakfast table is your packet of cornflakes and on your packet is a picture of the smiling Kellogg family at breakfast, and on their table is a picture of your packet which has a picture of the smiling Kellogg family, and so on, and so on (you know the one I mean). If you count how many packets there are the number will probably not be greater than the number accounted for in the last sentence, that is, four. Ah! you say, that is merely due to the limitations of the printing technology. And this, of course, is precisely my point.
We are now at a point in this reading/writing where we can observe 'A Strange Loop, or Tangled Hierarchies', if we wish to do so. But what we observe is just like what can be observed from a picture of this cornflakes packet.

Finally, I believe that by reflexively focusing on the attributional work of scientists and sociologists, I have engendered a multiphasic intertextuality regarding the discovery of endothelin. I suggest that this could be an outcome in this writing/reading. In the next section, we will see how the attributional work for this writing/reading is taking place.

Reader Hold on! The last task we gave ourselves now becomes relevant again. We have promised to be epistemologically radical and to show the unsettled nature of the attributed status of the discovery of endothelin. But those whose voices are present in this chapter are limited: to mention just the scientists, only some members of the Tsukuba Group, plus the voice of Highsmith's historical account. Here, my criticism against your asymmetrical position is not only with regard to Highsmith versus the Tsukuba Group, but also with regard to all the other endothelin researchers, scientists in related areas, and scientists in general, even those who scarcely publish and thus provide no access for us.

Writer And all the sociologists including those who scarcely publish, like me. And all the members of society including . . .

Reader Don't confuse my point! Our focus is supposed to be on how we cope with the textual symmetry here!

Writer But that point also applies to how other people cope with the Candidate's research. The members of the Tsukuba Group accepted her request for the interviews, but Highsmith did not answer the request, and so far, has remained silent.
Reader: Well, I remember that she sent a letter to him in the autumn of 1995, in order to inquire into the issues surrounding his work and how the discovery came (not) to be attributed to his group's work.

Writer: Yes. She did and received no response. Now, you are touching on a sore point. She lost an opportunity to expand on something interesting. But there is something worse than that. She felt she had intruded upon a scientist's research life as well as, perhaps, his feelings. That's how she felt. She thought that she ought not to be so insensitive as to make an inquiry into someone else's failure (e.g. Hornsby-Smith 1993: 52-67; Mason 1996: 29-31).

Reader: Oh. I can see that it is a touchy issue for her and perhaps, for Highsmith. But listen! The lack of a letter in response is still something to do with the intertextuality of the text of the Candidate's thesis.

Writer: How come?

Reader: The intertextuality of her thesis can be taken as including Highsmith's silence, as well as his voice as audible in his papers. His failure to respond is certainly not a failure of dialogue (Bakhtin 1981) between him and the Candidate. Such silence can even be celebrated, like Ashmore's witty acknowledgement to David Bloor, "for so intriguingly refusing to be interviewed" (Ashmore 1989: xxi). The Candidate is unavoidably including it as a factor in her research. This, then, means that her research has a sort of openness; a dialogue with someone who still remains silent.

Writer: Oh, well. That might work for now to cope with this research! I hope that openness will also apply to our reading/writing here.

3-4. A Discovery — Inside the Sociology of Scientific Knowledge?

Now, I will introduce a Hofstadteresque 'Strange Loop, or Tangled Hierarchies' or "confusion of levels"147 (Ashmore 1989: 245) once again. Let us consider the potential

147 See Chapter 1, p. 7-10.
findings in this chapter with respect to how they can (not) be recognised as 'a discovery for SSK'. Clearly, they benefit from Brannigan (1981) and GLL (1981). I am hoping that they more or less contribute to the further understanding of scientific discoveries.

Reader    Wait a minute! You had better not emphasise the benefit of those previous studies. If you do, the thesis will risk its potential of becoming a discovery.

Writer    I'm not sure. At the beginning of this chapter (p. 109-110), you have suggested that if we succeed in claiming that it is based on previous achievements, then that may help to constitute the significance of her research.

Reader    Yeah, now I have just realised that it is not clear here what kind of conditions are required for SSKers to attribute their research as a discovery for SSK. We have claimed that discovery is established as 'a novel something whose origin lies elsewhere' (Section 3-2). If this knowledge-claim is accepted as a condition, it is sensible to claim a debt to previous studies.

Writer    But then, Brannigan's condition of 'unprecedentedness' will be threatened. Of course, I have just emphasised 'the act of naming' which we have actually performed a couple of times in this text, and this may result in rescuing this text from the lack of 'unprecedentedness'. But perhaps those criteria cannot merely be collected together for the use of... who will be the one to own the criteria of intelligibility for SSK discoveries?


Writer    But he is also an inside SSKer! It is our claim that SSKers, like other scholars in many disciplines, do write/read their research object, including their own activities of studying it, within the study itself. It is, after all, Brannigan who claims:

[T]he sociological identification of [the natural sense of discovery] is itself an achievement which the present work recommends to sociology as its own discovery, with the conviction that this work is part of the very order which it describes.  

( ibid: 171)
You have also shown that in their sociology, scientists and sociologists do not only analyse the attributional work of scientific discoveries: in doing so, they participate in the attributional work itself.

Reader If that is correct, then SSKers' achievement of the social basis of scientific discoveries applies, self-referentially, to itself. Okay. The implication of that, for an SSK candidate thesis, is that the text of this thesis can itself be potentially and ongoingly attributed as a discovery for SSK. Now, you must try your best to make it a discovery!

Writer I'm not sure what you are implying by that.

Reader Come on! Learn more from what you have displayed and constructed as knowledge-claims! Just like the participant endothelin researchers, you must force the Candidate to pursue the research "at a stroke". You also need to be persistently consolidating and projecting the sociological status of this research! Besides, I don't think our act of naming (of our concepts such as this "act of naming") is yet powerful enough, and so you must stress it more strategically, like Prof. Masaki!

Writer Hold on, hold on! Unless the examiners of this thesis recognise this writing as an achievement in the first place, it won't work like that. That is, unless what we write/read intelligibly works as a contribution to SSK discovery studies, we won't be able to assume that it even has a candidate status for self-application.

Reader True. But as we claimed, the situation is not settled. Thus, there is nothing wrong in assuming that it may be in an ongoing process of becoming a discovery! Let's work towards such a goal!
Part II

PROCESS

The problem is in conceiving of reflexivity as a problem in the first place; as if it was at all profitable to seek an escape from realist ontology once we have committed to conventions of representation which buttress just that particular ontology. The strategy is to sustain and explore the paradoxes which arise when we attempt to escape the inescapable, not to attempt their resolution.

(Woolgar 1989: xviii-xix)

I am against closure in a text. Mechanical categories: "opposition," "change of codes"... sequential formalization and depersonalization: all relations are logical (in the broad sense of the word). But I hear voices in everything and dialogic relations among them... The problem of "precision" and "depth."

(Bakhtin 1979 quoted in Morson and Emerson 1990)

In this part, I will take up the process of knowledge construction both as \textit{its own research object} and as \textit{the textual state of this thesis itself}. That is, writing/reading about the process of knowledge construction will be pursued as an attempt to recognise itself as being a process of knowledge construction. I will try to display the process of knowledge construction both as what this thesis is about and as how the writing/reading of it is performed. The writing/reading of this part is, I will propose, one form of 'wrighting' (Ashmore 1985, 1989).


CHAPTER FOUR

THE ANGLES OF PRACTICE

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Pre-Introduction

The focus in Chapter 4 is on 'the process of knowledge construction' rather than 'the constructed knowledge'. I will try to see how far we can study it not as a construction -- i.e., as that which has already been constructed and is now temporally stable -- but as something currently in construction.

I will continue the examination with the triple parallel, this time as indicated in the above box. For this examination, I will employ two angles -- that is, the angle of retrospection and the angle of projection. Relevant participants' accounts will be analysed from both of these angles. For example, from the angle of retrospection, the endothelin researchers' accounts will be analysed with respect to how the nature of endothelin, the state of endothelin research and the scientists' actions are established in retrospect: from the angle of projection, the accounts will be analysed with respect to how the endothelin researchers discursively project the issues regarding endothelin and its research, so that the issues are made currently relevant.

\[148\] This title is an allusion to Pickering's *The Mangle of Practice* (1995).
An Introductory Narrative of 'The Development of Endothelin Research'

In Part I, I have explained that endothelin was reported by a Japanese group of medical scientists referred to as the 'Tsukuba Group'. I have also stated that endothelin was established as a potent vasoconstrictive peptide (Yanagisawa et al. 1988), and that this has been attributed as a scientific discovery by the relevant research community. Now, let me further continue my narrative of how this discovery of endothelin has prompted the development of a research domain called 'endothelin research'.

Since 1988, endothelin research has rapidly taken shape as an emergent research field in biomedical science. It is possible to illustrate this development with reference to the growth of endothelin literature during 1988 - 1997 (Fig. 4-1). In endothelin research, the reason for this growth is 'attributed to', for example, the nature of endothelin itself, namely, the potency of its strong vasoconstriction, and the consequent research interests which have been "so intense" (Vane 1989). Furthermore, this potency is claimed to have led to research outcomes which present "considerable experimental and some clinical evidence for involvement of endothelin in cardiovascular and renal disease. [ . . . ] , where it represents a novel target for therapeutic intervention" (Webb and Haynes 1993). Endothelin thus seems to have wide implications for medical science and clinical practices.

When it is explicated in terms of its 'nature', however, endothelin seems not to be established as a stable part of the body of knowledge in biomedical science. For instance, although investigations in endothelin research since 1988 can be said to have developed mainly into its pathophysiological aspects, its vascular effect, and the

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149 I shall first formulate my own construction of this narrative. For a problematisation of constructing one's research object in order to analyse how 'it' has been constructed in previous research, see Horton-Salway's discussion of the different kinds of explanations for 'M.E.' as the 'mystery illness' (1998, Chapter 2). See also Chapter 2 for how sociologists introduce insider-member's knowledge (p. 65-72).

150 The problems of engaging in the attributional work pursued both by members of society and analysts are discussed in Chapter 3, 3-3 (p. 131-168).
The volume was counted by using the MEDLINE database and retrieving all the articles indexed under the heading 'endothelin'. The reason for the marked decrease in the number of articles in 1996 could not be explained.

Fig. 4-1 The growth of endothelin literature$^{151}$
development of its receptors and antagonists (Gulati 1995), since 1994 the state of endothelin research seems to have changed course. In other words, 'another face' of endothelin has appeared. A new genetic engineering method, called the 'gene targeting technique', or more familiarly, the making of 'knock-out mice', has been introduced into endothelin research. With this method, deficiency in one of the endothelin receptors has been identified to have a relationship with Hirshsprung's disease which "is characterized by an absence of enteric ganglia in the distal colon and a failure of innervation in the gastrointestinal tract" and which is assumed to be a multigenic disorder (Puffenberger et al. 1994). Deficiency in the genes of endothelin and its receptors has also been found to cause morphological abnormalities and malfunctions in embryo development for mice, such as craniofacial abnormalities (Kurihara et al. 1994), and megacolon and coat colour spotting (Hosoda et al. 1994; Baynash et al. 1994).

4-1. Knowledge as Being in a 'Process'

**The Janus-Face of Science (and Endothelin)**

It may be not surprising if a social scientist starts a discussion with reference to Fig. 4-2 (p. 176), but it was rather startling to have seen a scientist using it in order to describe his research object in his concluding speech at the Fourth International Conference of Endothelin held in London, in 1994. (This double face of 'Leeper's ambiguous lady' [Boring 1930] can be nicely compared with an SSK version of a double face, i.e., the 'Janus' of "ready made science" and "science in the making" [Latour 1987: 4; see p. 177, Fig. 4-3]). The scientist said that when endothelin was first reported in 1988, it was a new and potent vasoconstricting peptide, but now it is possible to see another face arising from endothelin -- that is, as a gene related to unexpected diseases.

Thus, endothelin seems not to have been sustained as the same stable natural object. Accordingly, the scientific activities in endothelin research can be assumed to have been in transition. In terms of this thesis, the knowledge of endothelin is therefore
Fig. 4-2 Endothelin with 'Leeper's ambiguous lady'\textsuperscript{152} 

\textsuperscript{152} The figure is taken from Edwin G. Boring (1930).
Fig. 4-3 A two-faced Janus\textsuperscript{153}

\textsuperscript{153} The figure is taken from Latour (1987: 4).
regarded to be in construction. Consequently, I take it that the Candidate's formulation of the process of knowledge construction in endothelin research, i.e., this text, is also yet to be developed. Endothelin, the scientific activities, and the Candidate's research are therefore all currently developing in a mutually constitutive process.

Three Components of The Process

Following this rationale, let me introduce endothelin research as a process constituted in the accounts of the interviewed endothelin researchers. There seem to be three essential components of the process in their accounts: that is, linearity, velocity and orientation.

The first component, as found in these accounts, stresses that the development of endothelin research follows a linear time-line. What is established in such accounts is, first of all, that the research develops linearly in a chronological order, from past to present, and to the future. Secondly, researchers refer to the time it takes (velocity) to achieve a certain state or quality of knowledge with such remarks as "I said 'in one month, or two months at the longest, we can probably conduct a measurement' " (Prof. Kimura, 1 July, 1994). Such issues as how soon or how fast their work will be accomplished or the research goal will be attained, and how long it will take for a certain issue to be grasped, appear to be a common concern for the researchers. Finally, orientation is exhibited when researchers account for their own activities as directed toward some goal or end-result. Although the goals and end results are various and variable, this discursive feature nevertheless constitutes their activities as having an orientated character.

Let us take a look at how these three components are observed in the following extract taken from an interview conducted at the Fourth International Conference on Endothelin:

4A 157. Dr. Yanagisawa I suppose it may be that classical physiology and pharmacology will be tied up with that kind of genetics. Over the next couple of years anyway.
158. Mushakoji [Not only ] as regards endothelin but also (indistinguishable) will this prove
159. [At this poi-]
160. Dr. Yanagisawa enduring, I think. I think that this will be a major outgrowth for about the next
ten years to come.
Dr. Yanagisawa suggests some interdisciplinary integration "will be a major outgrowth" (line 161) for endothelin and related researches. With the future tense "will", this outgrowth is set in a linear temporal process, from the current state to a future state. This process also has a time span ("over the next couple of years" [line 158] and "the next ten years to come" [lines 161-162]). The goal to which endothelin research is orienting is to tie "classical physiology and pharmacology" (line 157) with genetics; the goal which Dr. Yanagisawa had just introduced at the conference where the interview was conducted.

Here, we can observe linearity, velocity and orientation in his account of his research. In referring to what kind of result will take place and how soon it will be accomplished, endothelin research is constituted as a process. The three components inform us that this process is a chronological one.

**You Will Proceed, If You Are Significant**

Now, I will examine some features of the process of knowledge construction in endothelin research which are crucial for understanding the continuation of this process. A central concern is the value of *significance* that seems to be giving life to the research. In accounting for endothelin research, a particular concern has been how much *significance* should be attributed to endothelin and endothelin research. The process of knowledge construction, a progression from the unknown to the known, seems *only* to proceed if the significance of the research object is, at the very least, recognised. In other words, the life of endothelin research continues only so long as endothelin is recognised as significant. In Chapter 2, I pointed out that the work of establishing the significance of endothelin goes hand in hand with the work of constructing the knowledge of endothelin (p. 104-107). Then, in Chapter 3, I argued that the value of significance was an essential trigger for the attribution of the Tsukuba Group's 1988 report as a scientific discovery (p. 159-162). Let me again present Prof.
Goto's remark made in 1994, which indicates that the significance of endothelin was a major factor in their report being attributed discovery status:

4B  The structure of an object, its relatively modest activity, the kind of gene that it is- if you just look at an object itself, it is not really all that interesting. Because there are millions of objects which exhibit physiological activities. [...] On the other hand, however, [...] this one has a surprisingly unusual nature which makes it [...] really grabs everyone's attention.

(Prof. Goto, 14 March, 1994, see Chapter 3, Extract 3N [p. 159] for a more detailed transcription)

This remark can be considered in relation to Prof. Goto's current engagement with this research. That is, it can be observed as attending to the successive development of endothelin research. As this interview was initiated by the Candidate's request for explanations of the development of endothelin research from the "Core Set" (Collins 1981) of endothelin researchers, the above remark takes as its concern the relevance of Prof. Goto's current scientific activity as an endothelin researcher. Namely, by establishing the significance of endothelin, in this interview, Prof. Goto's current scientific activity is thereby legitimated. The justification of the continuous pursuit for endothelin seems to depend on whether endothelin can be referred to as a significant research object in science.

Although endothelin research seems to be regarded as having a legitimate life, there is still some discrepancy in different accounts that construct it as either an advancing field or one that is being trivialised. In all but one case, the interviewed endothelin researchers claimed that the research domain is a developing field, albeit in a variety of directions. In the single exception, endothelin research was said to be declining:

4C  But- in addition, there is another issue concerning endothelin- whether it is really significant or not. I myself very much have doubts about it. [You see, now it's like] we ourselves are gradually coming to change

475. Prof. Masaki 476. 477. 478. Mushakoji 479. Prof. Masaki 480. Mushakoji 481. Prof. Masaki 482. Yeah. Because if we only keep going on and on with endothelin, we will be- under endothelin- for the name of it-

(Prof. Masaki, 7 November, 1995)
In the above account, Prof. Masaki relates his doubts about endothelin's significance (lines 475-6) and his own (non-)devotion to endothelin research (lines 477, 479, 481-2). Prof. Masaki's comment is interestingly ironical when we compare it with his account of the discovery of endothelin: it was the naming of endothelin, which Prof. Masaki claims has made the achievement of his group (i.e., Tsukuba Group) successfully attributable as a discovery (Chapter 3, p. 161-162). Now, the above comment indicates his reluctance to stick to endothelin just for the sake of its name (line 482). The account is somewhat surprising, as the majority of the talk and texts on the state of endothelin research which the Candidate has collected indicate that this is an advancing research domain.

The common view regarding this research domain is that the significance of endothelin, which is biochemically identified as having a strong potency (Yanagisawa et al. 1988), needs to be investigated pathophysiologically and genetically, and by assessing the availability of drugs which can be used to counteract this potency (Clozel et al. 1993; Webb and Haynes 1993; Vane 1994; Goto and Warner 1995; Masaki 1995; see also Extract 3M, p. 157-158). That is, simply from the action of a substance identified in vitro in laboratories, it is not possible to trace a clear direct link with the cause of human diseases. In order to clarify this link, it is commonly stated that various complex factors need to be sorted out. The above remark of Prof. Masaki questions the straightforwardness (if not the existence) of such a link.

Here, Prof. Masaki downplays the significance of endothelin by contrasting it with other more significant substances and factors (lines 686-8). This remark suggests that even
though he was the head of the Tsukuba Group, recognised as one of the major representatives of endothelin research, and actively publishing research papers and reviews in this area, he might not really be committed to endothelin research. Moreover, this suggestion is reinforced by the claim that the suspicion is held not only by himself, but by "everyone" who, he supposes, "thinks the same way" (line 690-691). Thus, the recognition of the significance of the research effort is undermined by downgrading its object as ambiguous or trivial.

This case suggests that the value of significance plays an important role in establishing that knowledge construction in endothelin research is in process. The role of this value in the recognition of a continuation of the research can be compared with how the attributional work of discoveries is organised in terms of this value (Chapter 3). The assessments of endothelin's scientific status rest upon this value (p. 159-162). The recognition of the significance of a certain research object thus seems to be crucial both for scientists' identification of this object as a discovery, and for scientists' continued pursuit of it in research to be meaningful.

But of course, there is also a difference between the way that a scientific achievement is attributed as a discovery, and the way the research initiated by the discovery continues. In the case of historical scientific discoveries, once the assessment is made, the object and its research may not necessarily require the currency of being significant ¹⁵⁴. In contrast, in order to continue the pursuit of the object, scientists need to keep emphasising its significance. Whether the object is treated as significant or not, is directly connected to the current activity of scientists in the field. The connection will define the legitimacy and status of their ongoing engagement with a particular field of research. Thus, the pursuit of endothelin is accountable on the basis of its current and potential significance, which naturally makes researchers' activity significant as well.

¹⁵⁴ For historical discoveries, it does not even need to have the currency of being true and real.
What Do You Mean By Saying "I Want to Know More"?

Finally, I will introduce the researchers' accounts, which suggest in what sense endothelin is further known.

Despite the abundance of scientific controversies found in SSK case studies, the Candidate has not found any equivalent controversy in endothelin research. As we have seen in Chapter 3, the attributed status of the Tsukuba Group's achievement as a discovery has been subject to mild dispute (p. 112-129, 154-158). But the existence of endothelin seems to have not been disputed at all. It appears that the concern of endothelin researchers is solely on the further pursuit of the nature of endothelin.

Let me introduce a researchers' talk which suggests some ways that the nature of endothelin is further pursued:

4E
543. Dr. Yanagisawa  But to see it genuinely, uh- physiologically, you see? The issue surrounding
544. what kind of role endothelin plays after all- it might still take time for this kind
545. of issue to be built up into a neat and clear schemata, like in the case of the
546. lenin-angiotensin system, you see? I'm afraid that may be the case.
547. Mushakoji  Oh. Really?

(Dr. Yanagisawa, 26 April, 1995)

In the above extract, it is first suggested that the research has an orientation to what will be done "genuinely, uh- physiologically" (line 543). It is presupposed that the research has not yet achieved this degree of genuineness. Then, what will be known is referred to as "what kind of role endothelin plays after all" (line 544). The outcome is established as "a neat and clear schemata" (lines 545-6). Then, Dr. Yanagisawa draws an analogy from another related case (lines 545-546), and applies a similar time span to the case of endothelin research.

Participant endothelin researchers have provided a variety of accounts regarding the ongoing state of knowledge construction in their field. Frequently, it is portrayed as persistently developing new phases for solving (new) problems, and thus following a kind of agenda. For example:

155 For instance, those of N-rays (Ashmore 1993b), gravitational radiation (Collins 1975, 1985), and vitamin C and cancer (Richards 1991).
So far, the sort of work in classical physiology and pharmacology have not been- within those eh- areas-

Yes.

with these- how can I put this? They have not been tied in very much with uh- that 'level' (in English) where work is done on something by modifying the DNA of animals, you see?

[Oh, right. Yeah. I see.]

Specifically- specifically, this has involved certain sorts of classic research areas such as that involving the physiology of the cardiovascular and related systems, which have not been- which were not like that- uh- well, you see, these have not tied themselves in with genetics at all-

Mm.

but this may now be the case. At this point, anyway. For the time being.

(Dr. Yanagisawa, 26 April, 1995)

In the above account, Dr. Yanagisawa is talking about the state at which "classical physiology and pharmacology" (line 165) are to be linked with the techniques developed in genetics. The linking of these fields has yet to be achieved which justifies the pursuit of a particular line of scientific work. In this way, endothelin and its research are described to be in a state such that problems and tasks are continuously emerging for the researchers to solve. Solving problems and completing tasks are methods for encountering the next set of problems and tasks. Consequently, scientific activities are continuously engaged in a process that is driving towards novel phases of problem-solving.

For endothelin researchers, their work constitutes a progression from what is unknown to what is known, and what is to be known. It seems that time is crucial here, as it is the time-progression which transforms the unknown into the known. The researchers frequently referred to newly introduced techniques which will shed light on the unexpected dimensions of endothelin, and contribute to the way that their knowledge of the field will be built in the future.

This importance of time in the progression of research can be compared with the way that scientists justify or account for error and ambiguity using a 'truth will out device (TWOD)' (Gilbert and Mulkay 1984; see also Chapter 1, p. 21-22). Gilbert and Mulkay claim that the TWOD is used by scientists for reconciling discrepancies between the "empiricist repertoire" and the "contingent repertoire" in the course of
justifying their own views, expressed in terms of the "empiricist repertoire". The TWOD dissolves the threat of current doubts and uncertainties by affirming the potential for truth to manifest itself, given time. In the endothelin researchers' accounts, the reference to time seems similarly to frame the way that endothelin will be further and better understood within each researchers' own agenda.

In this section, we have seen that the significance of endothelin is intrinsically related to the way that the research proceeds as a process. Furthermore, endothelin research seems to be a process related to the current activities of the participant endothelin researchers. Indeed, 'the process' seems to be raised as a topic in the course of legitimating their own scientific actions. Consequently, the process of building knowledge about endothelin through various types of research is made significant through the researchers' own actions. Conversely, these researchers' actions are legitimated by establishing the significance (and, in one case, the possible insignificance) of endothelin. Thus, the recognition of endothelin as significant makes the researchers' actions justifiable. It is for the sake of making these actions justifiable, that endothelin is recognised by researchers as being worthy of further study. Endothelin and the researchers' actions are in this way reciprocally constituted as account-able.

ASKING MORE ABOUT THE PROCESS

**Reader** But will that be all that we can see as a 'process'?

**Writer** What do you mean?

**Reader** It seems to me that you are merely showing a kind of product and not the process itself! Analysing how 'the process of knowledge construction in endothelin research' is itself constructed in the participants' accounts, you are making 'the process' into another SSK research object, like pulsars, quarks, oxidative phosphorylation, gravitational radiation, solar neutrinos, N-rays, scientific truth, facts, etc. You are
merely claiming the process as a social construction. But it is the process, which you should pursue, not the construction. It keeps going, and you should keep going.

Writer But it is the process which I have shown -- or at least what that process is for the participants. The process of knowledge construction displayed in this section is one the participants have provided for us.

Reader But listen! If you are keen to give them their voice, I'll remind you that for them, endothelin research is ongoingly and potentially proceeding. But your account takes a snapshot of this proceeding and reduces it to a small construction!

Thus far, what we have observed as 'process' is an illustration of construction. It is not satisfactory for me to deal with my case merely by explicating how it is constructed as such. Here, the research object is a process and I thus contend that in order to analyse 'the process of knowledge construction' as an ongoing process, I need to go further. The construction in this section illustrates a kind of sequence divisible into before and after, or past, present and future time components; a history, that is, which can be glossed as proceeding from what is unknown to what is known, and then to what will be known in future. I believe that there are more issues involved in recognising the process as process. I assume that the 'process' is, among other things, a state of ongoingsness\textsuperscript{156} and potentiality\textsuperscript{157}. With this ongoingsness and potentiality, the

\begin{footnotesize}
\textsuperscript{156} I define 'ongoingsness' as what orients us to see the process as a movement, which represents particular stages of a life in continuation. The ongoingsness of the process resonates with the Deleuzian idea of becoming, the essence of which is a movement, a pulling in both directions, before and after, past and future, at once (Deleuze 1990).

\textsuperscript{157} I assume that in recognising the 'process', many issues are potentially raised to be related and followed. One of the examples of different methods for doing accountable understanding, provided by Garfinkel and Sacks, put this another way. They call it 'Rose's gloss'. It is a glossing practice, reported by their colleague Edward Rose, which:

On a visit to a city he has never seen before, Rose is met at the airport by his host. They are driving home when Rose [looks] out the window -- which is to say that Rose, after doing [looking ahead] then does [watching something go by] by turning his head to accord with the passage of the auto. Rose's problem is to get his partner to provide him with what he has been looking at. Doing the notable particulars [looking ahead] and [watching something go by] and their serial arrangement are the crux of the matter, and make up Rose's artfulness. Continuing to do [looking out the window] Rose remarks, "It certainly has changed." His host may say something like, "It was ten years before they rebuilt the block after the fire." Rose, by having said, "It certainly has changed," finds in the reply, and with the use of the reply, what he, Rose, was talking about in the

\end{footnotesize}
object of pursuit is recognised as temporally conditioned, or even as yet-to-be-known. It further engenders a variety of actions and events, each of which is in process. To imply the incompleteness of such an object, I will use the term 'a becoming'.

But how far will we be able to analyse a dynamic state that is ongoing and potential? One way to further integrate the ongoingness and potentiality of the process may be to focus on the reciprocity between the nature of endothelin and the scientists' actions.

4-2. Strategic Actions, Nature and The State of Research

Intertwining of Human and Non-Human

In order to understand the dynamics of the process of knowledge construction, I will now consider endothelin research in terms of the reciprocity between the nature of endothelin and the scientists' actions. By applying Pickering's term to this case, endothelin and the scientists' actions can be said to be "intertwined" in a temporally
emergent practice (Pickering 1995; see also Chapter 1, p. 30-32). Let us first take a look at one of the endothelin researchers' accounts of the development of endothelin research:

In the above extract, Dr. Yanagisawa is explaining how the research has been developed by introducing genetic engineering techniques, and how the nature of endothelin has been revealed through these processes. In lines 334-335, with the remarks "it should" and "[t]hat's what I think", he suggests that his own knowledge of the research outcomes and appropriate follow-ups were what drove the development of endothelin research. But then, with the phrase "unexpectedly" (line 337) and "we were like all 'So, now, what's going to happen?' " (lines 341-342), he claims that he also did not anticipate the first place. Picking that up he formulates further the concerted, sensible matter that the two parties are making happen as the recognizable, actual, plainly heard specifics in a course of conversation: "You don't say. What did it cost?" (Garfinkel and Sacks 1970: 366)

In the case of endothelin researchers' actions, I have never encountered such artfulness as seen in Rose's gloss. However, in the interviews, conferences and more informal occasions, their actions seem to have made the potentiality of the nature of endothelin available. In most cases, what they claimed in the first place is seen to be found in the reply or reaction (when it was a lecture or conference presentation, and the audience listened to it without making objections). At least, what counts as their knowledge-claim is seen to be identified or made clearer in the sequence of discussion about 'it'.

I understand that what is referred to as endothelin has potential ties with a variety of other texts in any particular setting. What is claimed about endothelin is recognised, agreed with, or accepted with these texts. This uptake is another way to formulate what I discussed under the notion of intertextuality (footnote 82).

As I stated in footnote 36, on one point my understanding may be different from Pickering's. That is, my understanding is that the "performative idioms" and the "representational idioms" (Pickering 1995) cannot be separated. This will be discussed in Section 4-3.
outcome. In line 344, the remark "as expected" indicates that his investigation into the identification of a gene that causes Hirschsprung's disease was actively pursued with his prior knowledge. This is then again modified by the phrase "in a way that we weren't at all expecting" (line 349) which is an expression of his overview of the research development. In this way, Dr. Yanagisawa formulates his own actions with respect to endothelin as being based on his prior knowledge of it; a claim which is rhetorically neutralised by his subsequent claims that he had no prior awareness of how that research would turn out.

In this case, I should emphasise that the two vectors contrasted here are human and non-human. It may be, at first glance, that the contrast is easily drawn between scientific actions and endothelin, as SSK literature points out the contrast between human and material agencies (Pickering 1995), between the "empiricist repertoire" and the "contingent repertoire" (Gilbert and Mulkay 1984) and between Nature and Society (Latour 1993). In this case, however, it seems that things are a bit more complex: scientists' actions obviously appear as one vector for the research to proceed further and for endothelin to be further known. However, not only endothelin itself but also the state (i.e., growth, declination, etc.) of endothelin research is constructed as being beyond the control of human action. Thus, the contrast made in this case is between human and non-human -- that is, endothelin researchers' actions versus endothelin and its research.

Now, let me further illustrate how researchers' actions are constructed as actively fostering the research process. In their accounts, the endothelin researchers frequently formulate how they and their colleagues are driving the process of endothelin research. Here, I will refer to the researchers' actions as 'strategic actions'. The following two extracts suggest how those 'strategic actions' are accountable in making a particular domain of knowledge available.

4H
240. Prof. Goto
241. So, it may be that this one and this one (the endothelin research in physiology which Prof. Goto says his group is pursuing, and which is pursued by using the 'knockout' model) are-
243. Mushakoji  Mm.
244. Prof. Goto  totally independent issues.
245. Mushakoji  Hm.
246. Prof. Goto  But there is a possibility that they are tied [somewhere.]
248. Prof. Goto  So, [the interspace must] be investigated from now on. Yes.
249. Mushakoji  [It's exciting.]

(Prof. Goto, 2 October, 1997)

41
405. Dr. Yanagisawa  Well, but anyway, there is no doubt that endothelin is at a very important stage
406. [among them.] In order for this stage to come about, [[now, we are, ]] [[From this point on-]]
407. Mushakoji  [Yes. I see. ]
408. Dr. Yanagisawa  laboriously- in the preparation stage, working to make all of the 'knock-outs'
409. ((refers to the engineering of gene deficient or 'knock-out' mice)) [[laughs]]
410. Mushakoji  [Yes. ]
411. Dr. Yanagisawa  working on it by using mice, [[as you know.]]
412. Mushakoji  [[Mm. ]]

(Dr. Yanagisawa, 26 April, 1995)

The first extract displayed above is an account of the current state of endothelin research. What Prof. Goto is projecting is "a possibility" (line 246) toward which his group is working. Although this possibility has not yet produced any outcome, his statement that "the interspace must be investigated from now on" (line 248) establishes the ongoing actions with respect to endothelin of scientists working in his laboratory.

Likewise, Dr. Yanagisawa in the second extract remarks that his current work is pursued for the preparation of the next stage of endothelin research, and thus for revealing further knowledge of endothelin (lines 406, 408–409). In these two extracts, by establishing their actions as strategically pursuing the research and thereby revealing the nature of endothelin, these scientists formulate the accumulation of knowledge regarding the nature of endothelin as ongoingly and potentially acquired.

In parallel with scientists' strategic actions, endothelin is worked up to be emerging independently from their prior expectations. This view can be aligned with the process of 'becoming', such that endothelin and endothelin research are both established as 'becomings'. Let us examine, first of all, how endothelin itself is accounted for as 'a becoming':

43
397. Dr. H. Kurihara  Well, one reason that our recent research was published in Nature is that it was
398. like "something totally unexpected has occurred." (laughs) [1 That 1] may be
In this extract, in order to explain his success in publishing his group's finding in *Nature*, Dr. Kurihara is making a distinction between scientific achievements which are expected (and thus less surprising) and those which are not (and thus are more surprising) (cf. Pinch 1985). Here, this is worked up in accounting for a success, which is related to the discovery of an unexpected aspect of nature that had previously been beyond human prediction. It is suggested that even though Dr. Kurihara's group first acted on "it" (line 413), it is endothelin itself which turns out to be ("something comes out uh- only after that" [line 414]) and which urges scientists to respond to its nature. In this account, the scientists' lack of prior expectation is contrasted with achievements which are based on "the concept of endothelin" already established, and whose results are recognised as a case of "'Yeah. Of course, that's the way it is.'" (lines 409-410) -- i.e., "confirmation" (line 412).

In the interview from which the above extract is taken, Dr. Yukiko and Hiroki Kurihara mainly discussed their 1994 research that was published in *Nature* (Kurihara et al. 1994). Their group had reported the outcome of the first successful use of gene targeting techniques for making mice deficient in endothelin-1. The endothelin-1 gene
then turned out to be a cause of abnormal manifestations in foetal development and haemodynamics (Kurihara et al. 1994). This, as is briefly suggested in the introduction to this chapter (p. 173-175), amounts to a shift in endothelin research. As the authors explain, the result was unexpected and the group found they had to convince Nature's peer reviewers in an intense argument and negotiation. This shift is also seen as having re-ignited endothelin research as a whole, since when this group has been persistently and ongoingly investigating the nature of endothelin. In this instance, even though the discovery is the consequence of using the newly employed gene targeting techniques and thus exhibits scientists' strategic actions, scientists are still constituted as passive onlookers at the emergent nature of endothelin.

Endothelin research is also frequently observed to emerge out of itself.

Dr. H. Kurihara: Well, it's surely the case that the research into endothelin itself is- well, in this kind of field, there existed a- uh- some kind of progress which took place in a very neat way, though.

Mushakoji: Yes, [right.]

Dr. H. Kurihara: [In such a way] since at first there was a discovery and eh- various kinds of uh- medicine was produced and then the mice were made, it's really like- you know, in reality, it is such- en- looking at it from the position where some kind of research is organised.

Mushakoji: Mm.

Dr. H. Kurihara: Although I am not exactly sure how to put it, but- well, yeah, it was somehow, as if we were groping in the dark, like "Oh, there's something. Oh, now I see" and- it's that sort of thing- it was like this-

Mushakoji: Mm.

Dr. H. Kurihara: It's really- it does not go so very neatly-

Mushakoji: Oh.

Dr. H. Kurihara: as 'Spatt' ((a mimetic expression indicating a sharp clear-cut movement))

Mushakoji: [as ] it might appear to be from the outside.

Mushakoji: [Yes.]

(Dr. Yukiko and Hiroki Kurihara, 8 November, 1995)

Dr. Kurihara is talking about the "progress" (line 272) of endothelin research. He contends that when the development of the research is formulated, it tends to become a story (lines 275-278) which looks "very neat" (line 273). This is attributed as the common story which outsiders may tell. He claims that what took place "in reality" (line 277) for those who are actually organising the research is entirely different. The real story is, first of all, established as far more difficult to formulate (line 280) due to a
chaotic reality which reduces the actions of the scientists to "groping in the dark" (line 281) and responding to what happens in nature.

It seems that when the task is to explain researchers' actions, the nature of endothelin is constructed as their research object, and how the research develops is made subject to their own actions. On the other hand, when the nature of endothelin and its research development are construed as 'becomings' beyond the control of researchers' expectations, their actions are constructed as passive and reactive to contingencies. Observing these two kinds of account, I would argue that it is the intermingling of actions, nature and the state of research which serves to display the process of knowledge construction as 'in process'. Each of these kinds of account – the human and the non-human -- seems able to function as a vector, in which the other plays the role of its target: one emerges from the researchers' actions, and the other emerges from endothelin and the research as becomings. The basic understanding is that the non-human is held to be out-there and beyond the reach of human actions, while scientific actions work to reveal nature. Each of the two vectors must work together in a co-implicative way for the process of knowledge construction to proceed.

ASKING FURTHER ABOUT THE PROCESS

Reader So, is this all of what we can see as a 'process'?

Writer What do you mean?

Reader You have offered a kind of explanation about the process of knowledge construction which is fostered by two vectors. But I still do not see how the process is actually ongoing and potential, as you claim. You have written that you will show the process, not the construction.

Writer Right. I am still wondering how far we can analyse the issue. I want to be careful not to relapse into stating some reality 'out there' or to predict what will be the logical effect of some cause, as I have already chosen relativism and social

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159 I have not been able to explicate how endothelin and its research interact with one another in the endothelin researchers' accounts.
constructionism to analyse the issue. And of course, don't forget that I am also taking a reflexive stance.

Reader Hey! That may work!

Writer What? . . . No, I don't want to be bogged down by discussing those reflexi...

Reader Oh. No, no. Not that. Listen! The endothelin researchers are not merely talking about how their research is developing. They are also working up their research as ongoing and potential.

Writer You mean, in their talk?

Reader Yes, precisely. You see, in talking about endothelin and endothelin research, they are exactly projecting what the nature of endothelin has been, is now, and will become, in all sorts of settings in science, including those interview settings. And their talk is simultaneously constructing the nature of endothelin and the state of endothelin research, in retrospect.

Writer But don't you simply mean they are 'talking about' what endothelin is and how it turned out to be?

Reader They are 'working up' those things. Can't you ever stop separating representation and performance?! The participant scientists are saying both that it is they themselves who pursue the discovery of nature, and also that they have been passively waiting for nature to reveal itself. It seems that it is a kind of reciprocal relationship between what they work to accomplish and how 'it' turns out for them which makes things ongoing and potential. It is precisely because of the co-implication of these two that the process can proceed! Why can't we learn from them? Can't you view things from both the projective and retrospective angles?

Writer The reciprocal relationship between how objects are constructed in retrospect and how their accounts work to project them?

Reader Exactly! Retrospection and projection may be the two key angles for us to bring the process to life.
The Angles of Retrospection and Projection

From now on, I will examine the process of knowledge construction from the angles of 
retrospection and projection. In Chapter 3, I have briefly introduced the notion of
'projection' into my analysis (footnote 142). There, I have drawn the analogy of
observing a seed growing. Now, let me redefine 'projection' further. It is a
performative action of constructing in situ. It works so that anyone can see that things
are ongoingly and potentially becoming. Projection is performed by the participants
and works to make available an object to which they can sequentially refer. However,
in order to make the object available, this action must be viewed from the angle of
retrospection. Or rather, through retrospective description, we make it 'the object'. This
is the view from the angle of retrospection. Any discursive work can be seen as
performative from the angle of projection, and descriptive from the angle of
retrospection.

Let me explain these ideas further with an illustration (Fig. 4-4). Previously, we
observed that the process of knowledge construction is recognised as ongoing and
potential with the two-way vectors. That is, both 'strategic actions' and 'becomings' are
recognised to be vectors in the process of knowledge construction. Now, this
observation is being made at both the angles of retrospection and projection. At the
angle of retrospection, the endothelin researchers' 'strategic actions', as well as
endothelin and endothelin research as 'becomings', are observed to be involved in the
construction of action, nature and state. But action, nature and state are not only
working to represent objects as 'out-there'. The participants are observed to be
performing a projection for what is in construction. In other words, action, nature and
state are all 'becomings' at the angle of projection.

Our analysis too works with and from both angles. Such an analysis will reveal
both how the process of knowledge construction is itself constructed and how the
process of knowledge construction is in situ worked up as ongoing and potential.
The (Constructed) Process of Knowledge Construction

In a Process of Knowledge Construction

At the Angle of Retrospection (descriptions)

At the Angle of Projection (performances)

Fig. 4-4 The process of knowledge construction
Let us now examine how we can observe the reciprocity of the two vectors at the two angles with the following three extracts:

44.
96. Prof. Goto  These issues have been, you see, gradually grasped.
97. Mushakoji  Mm.
98. Prof. Goto  So, as expected, just like how we have been aiming at from the outset-
99. Mushakoji  Yes.
100. Prof. Goto  or more than how we were aiming at, after all, we are like "so, it is working
101. in an incredible way".
102. Mushakoji  Mm.
103. Prof. Goto  We see it this way.
104. Mushakoji  Yes.
105. Prof. Goto  Probably, what I told you previously is that "we have found a very significant
106. object,
107. Mushakoji  Mm.
108. Prof. Goto  but we still do not know how it is working in the human body."
109. Mushakoji  Yes. You told me, like "it will come out from now on."
110. Prof. Goto  "From now on." Yeah, so that is now gradually- step by step-
111. Mushakoji  Yeah.
112. Prof. Goto  it has been made clearer now.
113. Mushakoji  Mm hm. What you told me at that time- already em- a few years ago, [uh-]
114. Prof. Goto  [Mm.] came out to be almost actually [[exact]]
115. Mushakoji  [[Most of them]] have been becoming how we
116. Prof. Goto  thought.  

(Prof. Goto, 2 October, 1997)

The first extract suggests how Prof. Goto accounts for the research development, and how he works to engender this development as ongoing and potential in accounting for it. Thus, endothelin is established both as the outcome of the scientists' strategic actions ("just like how we have been aiming at" [line 58]) and as 'a becoming' which is beyond those actions ("more than how we were aiming at" [line 60]). The intertwining of these 'strategic actions' and 'becomings' is established as proceeding in a trajectory. This is the construction of endothelin at the angle of retrospection. But we can also see, in the first place, that what is constructed as the research development in this extract is a co-performance of both the interviewee Prof. Goto and the interviewer Mushakoji, such that the construction is temporally built up. Namely, they are seen to be working together to make the research development recognisable as ongoingly and potentially proceeding. In the above extract, most of the time it is Prof. Goto who is taking the role of formulating the object (i.e., the research development), while Mushakoji's role is the
crucial one of agreeing with Prof. Goto's projection. The object is thus ongoingly in construction. This view is what can be seen from the angle of projection.

The next extract provides an interesting observation. I have already shown Prof. Masaki remarking that endothelin research may be declining (p. 180-182). At the angle of projection, this remark can be seen as performing a trivialisation of the significance of endothelin. It is interesting, however, to look at the extract further:

In the above talk, Prof. Masaki first claims that he knows the path that endothelin is following -- namely, becoming insignificant. But then he qualifies this specification (lines 94-95) by establishing his involvement as passive in relation to endothelin as a becoming. This kind of qualification recursively takes place in this interview. At the angle of projection, we can see that a lot of work is accomplished by this qualification, for example, mitigation or avoidance of criticism in case his claim turns out to be wrong. However, endothelin is in situ constructed in this qualification as 'a becoming' which has potential significance. Thus, it is interesting that even though Prof. Masaki's claim is seen as trivialising endothelin and undermining the endothelin research, it is still his own performance that accepts its status of 'a becoming'.

The final extract shows the scientist's formulation of endothelin research as a construction at the angle of retrospection:

Thus, it challenges us to analyse it further from both the angles of retrospection and projection.
Dr. H. Kurihara: It is really rather- How shall I put it? Yes, it's like there is an- um- issue which is not really very clear, and then it finally becomes clearer, and then when we look back in retrospect, we can then finally [start] seeing things as it were.

Mushakoji: [Hmm.]

Dr. H. Kurihara: [Very much]] like "Oh, now I see, it had been this kind of a story all along."

Mushakoji: [Oh.]

Dr. H. Kurihara: [I So, this is rather-] [2 Yeah. 2]

Mushakoji: even though you have one-

Dr. H. Kurihara: Mm hm.

Mushakoji: eh- this very fact and- if it is so, uh- how- oh, yes, still, when you are working [3 Of course, 3] you are assuming that it's like "in this way, this time, things [2 broadly speaking, 2] [Yeah. 2]

Dr. H. Kurihara: [3 Yeah. 3]

Mushakoji: may proceed in this direction" [(4 indistinguishable) 4]

Dr. H. Kurihara: [4 Yeah. That is 4] also of course true.

((But still, )) For example, I also- when I engaged in the first discovery of endothelin, um- I was not so much, at the beginning- was not all conscious that this has been discovered- no, developed in this way-

Mushakoji: Hm.

Dr. H. Kurihara: it may be that- eh- those with whom I was working- well, someone like Dr. Yanagisawa or Prof. Kimura-

Mushakoji: Yes.

Dr. H. Kurihara: well, they were not making such clear predictions [that] it would take on the [Yes.]

Mushakoji: form of this kind of development, and, well, after it was discovered, it was like "What will be attainable? This can be attainable" and-

Dr. H. Kurihara: Oh.

Mushakoji: in just this way it has developed in a variety of different ways- the things are much likely this way [and ] it was also like this in the case of the 'knock-

Mushakoji: [Yes.]

Dr. H. Kurihara: out- when the knock-out was eventually made 161

Mushakoji: Mm.

Dr. H. Kurihara: it was like "Oh. So, this is that sort of a thing. Oh, I see" and only from that point, did we rush [in order to study it-] it's went something like that.

Mushakoji: [Oh. ]

(8 November, 1995)

The account is particularly interesting, as the participant scientist is talking about the difference between what can be described in retrospect (lines 220-223, 230-232, 234) and what is actually taking place in the research. At the angle of retrospection, he is talking about a difference between how the research development looks and how it actually is. (cf. Gilbert and Mulkay 1984). What he formulates as the contingent appearance of endothelin research and what he establishes as taking place in reality are both constructions in retrospect. At the angle of projection, however, Dr. Kurihara is

161 This phrase in Japanese can be interpreted either as "the mice were successfully made" or that "the work was completed".
working to make both these constructions relevant; and also to work them up as such in this setting.

Note the repetitive use of "how shall I put it?" (lines 222, 230, 226-227). This indicates the difficulty of formulating the gap between what appears as the common story of the research development and what takes place in reality. While the former is a clear and tellable story, the latter can only be formulated once it has come into being (lines 231-232). Thus, he is quite reflexive in performing his claim -- that is, expressing his inability clearly to formulate the gap between them at the angle of projection. His talk is a performance of formulating endothelin research and his scientific work as an achievement which can only neatly be described in retrospect.

At the angle of retrospection, the researchers' claims appear to legitimate the achievements and scientific status of their own actions while doing the work of separating what is natural and 'out-there' from their own actions. At the angle of projection, the researchers successfully work up formulations of their own strategic actions, the nature of endothelin and the state of research as ongoing and potential. They disclaim them as their own formulations by claiming their status as 'in situ becomings' -- i.e., as the currency of reality. The process of knowledge construction is thus maintained as ongoing and potential, precisely because of the seamlessness of such discursive work.

ASKING MORE . . .

Reader  So, is this all of what we can see as a 'process'?
Writer  Oh, not again! What more do you want?
Reader  Throughout Section 4-2, you have examined the process of knowledge construction in endothelin research by showing how distinctions between the endothelin researchers' strategic actions, and 'becomings' of endothelin and endothelin research, intertwine as the two vectors along which the process of knowledge construction proceeds. You have claimed that the process is ongoingly and potentially proceeding when the intertwining is viewed from the two angles of retrospection and projection.
You seem to believe that this is the most convincing way to understand the process, but there is at least one intrinsic issue left out in this formulation.

Writer So, you are saying that I haven't addressed all of the process. What is left out?

4-3. Analysis, Constructions, and Contributions to SSK Knowledge

Lost Creativity

Reader How can the process of knowledge construction encapsulate creativity?

Writer Creativity? I thought you would finally want to bring your reflexi...

Reader Well, you know, there are a variety of terms which invoke the impression that something is left out in the process of knowledge construction you have written up to now. For instance, 'originality' (see also Chapter 3), 'astonishment', 'inspiration', 'serendipity' (Roberts 1989), 'sense-making', 'the idea' which leads to a "paradigm reformulation" (Kuhn 1962). All these terms emphasise a creative aspect engendering the inherent human urge to bring something new, unique and radical into the process.

Writer Right. So, I should emphasise that in our formulation, the creativity in the process of knowledge construction is to be located not in human mental process, but in the intertwining of human and material agencies, following Pickering (1995: 20).

Reader No, not that. I am not suggesting that the materials are creative. It's rather... whether creativity is the consequence of our descriptions or not. That is, we analyse it as a construction in retrospect. Is this the limit of what we can analyse? Can creativity be analysed at the angle of projection? At the angle of projection, scientists' strategic actions might be creative, or they might fail to be so at all, but...

162 A dialogue on how creativity in music is located is written by a jazz pianist, Brad Mehldau. Creativity in music can of course be said to be different from that in science. But how the impellent force or "animus" (Mehldau's term) is held in process for those who espouse living in a protean world is revealing and amusing in this quotation:

Take a back seat for a minute. Listen. Whether it's showtunes or Schoenberg, the fundamental animus for composer and improviser has never been about finding something 'new', really. What's new becomes worn-out immediately, and if there's any 'objective' truth to be found in art
Writer Are you . . . suggesting a kind of cleavage between projection and retrospection?

Reader What do you mean?

Writer Are you saying . . . for scientists, they project things in the expectation of obtaining something which is, in retrospect, a certain outcome. It seems that their own formulation of scientific activities are that nature has historically, and in retrospect, taken various and variable figures, textures, relationships, etc. (cf. Woolgar 1988c: 68-69). But before that is inscribed, before the consequences are made relevant, it just awaits . . . Likewise, our analysis follows what is described in retrospect, even though we have attempted to take the angle of projection as well. Are you saying that there is thus some kind of cleavage between the object in our analytical pursuit and what is in situ projected?

Reader Yeah. Say, if the scientists' work is formulated as a strategic action, and if this action is attributed as a 'creative' scientific achievement, then it may well be that this kind of creativity can be described in an orderly way. Such retrospective constructions are meat and drink to us. But at the very site where the strategic action is taking place, or at the very site where a becoming of what-may-retrospectively-be-seen-as-constructed is projected, neither the action nor the becoming are yet established as anything. We just remove what is consequently described as 'creative'.

Writer But then, you seem to be saying that the 'reality' constituted by cognitive operations comes first and then the consequential descriptive activities arise out of

---

This account can be related with the notion of intertextuality as defined by Kristeva: "[t]he transposition of one (or several) sign system(s) into another" (Kristeva 1984: 59-60; see footnote 82).

More importantly perhaps, the concern for creativity can be related with the Bakhtinian idea of unfinalizability, which is defined as "immanent in and essential to quotidian existence" (Morson and Emerson 1990: 38). Morson and Emerson explain that for Bakhtin, "the only way for creativity to be real is for it to be immanent in constant, ongoing process" (ibid.: 40, emphasis original).

These remarks suggest that what is creative in a setting is made up of things which are already established: and although it may be described, this description is retrospective. The creativity in the original setting is never captured, but can be potential, given one's performance is in an ongoing process. In this sense, I am attempting to make the Candidate's research object creative, and to write/read this chapter creatively. Such creativity is further emphasised in the final chapter.
them. That will sound, for social constructionists, a disturbing turn to cognitivism (Edwards and Potter 1992; Edwards 1997).

Reader No, no. I'm only talking about how the descriptive activities can be analysed from the angle of projection. We can surely analyse participants' rhetorical and discursive work in those activities. But for scientists, the aim of their activities is not to employ this work, but to project nature through this work. Can we analyse how creativity is progressively brought out in their projective work of informing nature?

Writer Okay. Then, we can shift our focus onto how creativity can be orderly described in a particular setting.

Reader Hang on. That task can only be pursued by your own description of 'creativity' as seen in retrospect. I am instead talking about the setting where the projective work of describing 'it' is still in process. Is it not also the case that 'what-we-can-recognise-and-analyse' is also evolving in it?

Writer But you cannot analyse how scientists' work is creative without descriptions. Let's not separate our description from our analytical object! We will be accused of not concentrating on scientists' embodied practice of discovering nature (Button and Sharrock 1993; Lynch 1996). Creativity is the consequence of our description! Don't be so foolish as to lead our discussion into an analytic trap.

Reader On the contrary, I think that you are too cautious! I am questioning how far we can take such analysis. We have tried to analyse the endothelin researchers' actions not only at the angle of retrospection, but also at the angle of projection. As such, we aim for more than a retrospective analysis of 'what is constructed'. We ought to shed light on how scientists inform us about nature. If we keep our analysis only at an angle of projection, which is itself grounded in retrospective description, how can we say that we are dealing with a process?

Writer But we know all too well that any analysis can only be grounded in, and emerge from, descriptions.

Reader Yet, these descriptions do not really show the state of what is becoming known in a process, do they? Indeed, you are currently struggling to analyse some
order out of a state where something is becoming. But how can you analyse the object of her observation, analysis and description, given that her object is still being constructed in situ? The position of being able to describe something is far from the position from which something is recognised in process.$^{163}$

The Janus-Faced Analysis

**Writer** Oh. Well... any analyses are conducted by taking the angle of retrospection, in the sense that they are entirely grounded in descriptions. I think I pointed out in Chapter 3, that for a sociologist to understand how people attribute scientific discoveries, she first needs to be an inside member of these people. But in order to analyse it, paradoxically, she needs to set herself outside of the people's domain. Likewise, however hard she tries to keep her object in process, her description terminates and fixes the processes which are ongoingly and potentially developing. Can't you ever accept that? These two angles are effectively employed in analysing the process of knowledge construction in endothelin research. But the state of our analysis itself has to be kept solely at the angle of retrospection.

**Reader** If the analysis is only performed as the outcome of such descriptions, what we see as a process is only a construction made out of other constructs.

**Writer** Yeah. I think that in Chapter 2, I also pointed out that SSKers have to pre-emptively describe nature in order to analyse how it is constructed in science (p. 64-70). Now, the very fact that a piece of research has begun means that the object has already been implicitly pre-interpreted (Pollner 1987: 124). If you want to analyse, you need to use descriptions produced in retrospect. To examine how it is described or how these descriptions work to constitute it, you need to pre-empt this 'it'. No other choice.

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$^{163}$ In fact, I am reluctant to write/read how the description can be a process by describing it as such once and for all. I think what is crucial here is not how to describe descriptions, but my own engagement with descriptive activities (Woolgar and Ashmore 1988). I will keep on saying nothing of "the more severe problems of describing that which provides for the possibility of 'description'" (Pollner 1987: 20); except to say that the description in the reading/writing which is now taking place here is in the process of constituting the research object, as well as the research outcome of this thesis.
Reader: Coward! If you remain at that angle, we are only analysing the construction retrospectively accounted by them and retrospectively observed by us in their accounts. By neglecting the angle of projection, you are discarding the process.

Writer: No, I'm not a coward. You seem to be telling me that I am not writing the ongoing process of knowledge construction at all. But I am claiming something obvious. I think you have fallen into a sceptical muddle.

Reader: Take the constraint of analysis away from our analysis!

Writer: No! It's an inherent feature of analysis, and must remain if our work is to count as analysis!

Reader/Writer: Otherwise, you are merely in an abyss!

Analyses, Constructions and Contribution in a Process

Although science is performative (Pickering 1995), and although the distinction between human agents and material agents is relative (Latour 1993; Ashmore 1993; Edwards, Ashmore and Potter 1995), what are seen as scientists' actions, nature or the state of research are established in human retrospective accounts. It is the scientists' (human) accounts which are suggestive of the centrality of endothelin's (non-human) actions. Even though human discourse is tightly constrained by the resistance that it encounters in its performative practice (Pickering 1995), human discourse articulates this practice and makes sense of it. That is, the convergence of humans and non-humans is displayed in human descriptions retrospectively. Those who use descriptions have a device which represents in retrospect (and thus advantageously). But this is an obvious disadvantage for the study of 'process'.

Let us examine Fig. 4-4 again. I have tried to analyse not only what is constructed in the endothelin researchers' accounts, but also what is performed by them when they account for their actions, endothelin, and endothelin research. My contention is that knowledge construction as a process is to be studied both at the angle of retrospection and the angle of projection. The process of knowledge construction, with its assumed two vectors, that is, 'strategic actions' and 'becomings', can be analysed as
social constructions at the angle of retrospection. At the same time, the angle of projection is concerned with how such a process is itself in construction. However, we have now found that the state of the analytical object is of quite a different quality when viewed from this angle. Let us remember the analysis in Section 4-2. I have tried to investigate how these participants perform, and what is projected in their accounts that indicate an ongoing process of knowledge construction in their research. But no matter how I analyse it, the analysis itself will maintain a retrospective feature. It is inevitably based on the participant endothelin researchers' accounts. Although these accounts are oriented towards the future as much as the past of science, the accounts themselves are taken as describing a certain object, state or action. That is, being able to take them as accounts is the result of making sense of transcriptions that are justified as reproductions from the recordings of interviews. In addition, the analysis starts by invoking an ethnographic context in order to make the setting of the interviews intelligible for the readers. Sometimes, it also endorses a further retrospection: I can do it by identifying Mushakoji in the extracts and myself as the same person: I can provide information about the research context and also provide an account of what I meant in the transcribed sentences. 

Although the status of analysis at the angle of projection seems to be somewhat obscure, it is crucial to recognise that it works to make the object describable for analysts. The participants' projections (via writing, talk, gesticulation, etc.) not only allow analysts to analyse the rhetorical and discursive work employed within it, but also to refer to and talk about the participants' object: they can start participating in discourse where this object is constituted as 'shared knowledge' (in the sense of 'pragmatic intersubjectivity'; see Edwards 1977: 114-141; see also Chapter 1, p. 22-23). In other words, it is through participants' projection that analysts can know the object in the first place. The counterpart of this knowing at the angle of retrospection (the other face of Janus; see Fig. 4-3, p. 177) is that the object is said to represent some substance,

164 I have introduced the multiple subjectivity for this thesis in Chapter 1 (p. 52-55). The problematics raised in this approach will further be discussed in the next chapter, Section 5-2.
phenomenon, state, event, etc. This retrospective description can then be analysed as a construction.

The link between knowing and analysing (Chapter 2) and the analyst's paradoxical position of being both inside-and-outside (Chapter 3) can now be seen in terms of these two angles: from the angle of projection, participants performatively project their object, and the analyst can know this object by being an insider: from the angle of retrospection, the participants are viewed as constructing their object, and the analyst describes it as an outsider.

From the angle of projection, the analysis and the analysed object appear evident in participating in the field, reading the materials, keeping dialogues with scientists, etc.: analysts are informed by texts in science. Likewise, in this text, reading the extracts informs us of the issues, in the course of making sense of how the talk is organised. This is pursued in and through a dialogical relationship with these extracts, accomplished by projection. But this dimension seems only to be available in the course of reading/writing when what is read/written is described; and this is only observable at the angle of retrospection.

Reader So, is that a constraint in the mechanism of our analyses and reading/writing? Are SSKers' objects all retrospective constructions?

Writer Of course, their analysis is not that naive. None of them are saying that they are analysing "ready-made" social constructions. Instead, their analytical objects are constructions "in the making" (Latour 1987).

Reader But surely are they not still pinning down some kinds of constructions as such -- as stably and safely the selfsame objects? For any SSK research, "however up-to-date one's topic might be, all research is necessarily past-oriented" (Ashmore 1989: 6). No matter how they espouse relativism and social constructionism, aren't their objects thus analysed in retrospect, by pre-empting constructions?

Writer I'm not sure that you have taken the right interpretation of the word 'construction'. The word is to be used as some kind of action, function or performance,
which is temporally working. In fact, what is analysed by SSKers must be a process. The process will render the construction unable to be stable, safe and unchanging.

Now, let us briefly review how researchers in SSK and related programmes for studying science are situated with regard to the issue of angles. Fig. 4-5 suggests how the angles of retrospection and projection may be applied in relation to SSK analyses. First, in Arena 1, I have set out what is discussed in Section 4-2. Then, if we can accept it and draw an analogy, the process of knowledge construction in SSK can be formulated in a similar way (Arena 2). That is, SSKers can self-referentially assume that the process of knowledge construction in SSK is proceeding with similar vectors -- i.e., SSKers' strategic actions, their research objects, and its research development. In this arena, the process may also be seen in SSKers' accounts, both at the angle of retrospection and projection. For example, in the introductory parts of their research papers and books, the progress of SSK research as a whole would be provided in retrospect.\(^{165}\)

Arena 2 seems to be nurtured by analyses from Arena 3. The analytical outcomes of the authors are situated in this progress. The presence of Arena 3 is discussed at the beginning of this section (p. 200-205). That is, the analyses are seen to be grounded in the descriptions at the angle of retrospection (N). This angle can be applied to any SSK research. As any scientists' action is observed with scientific writings and transcriptions, SSKers' analytical object stays retrospective. At the angle of projection (M), SSK studies the rhetorical and discursive work in scientists' talk. But how scientific writings and scientists' accounts inform SSKers about nature and scientists' activities are not made relevant (except here: see Chapter 2, p. 65-71 and Chapter 3, p. 132-154).

\(^{165}\) Woolgar and Ashmore's attempt (1988) is a nice reflexive deconstruction of such a history.
The Process of Knowledge Construction as It Proceeds in Science

2 vectors:
1. Scientists' Strategic Actions
2. Nature and the State of Research as Becomings

The Process of Knowledge Construction as It Proceeds in SSK

3. SSKers' Strategic Actions
4. SSK Research Objects and the State of Research as Becomings

SSK ACCOUNTS

SSK ANALYSES

SSK ASSESSMENTS

Fig. 4-5: The angles of practice in science and SSK
Arena 4 comes under consideration when we self-referentially think of how SSK analyses (writing/reading) are themselves taken up. Here, SSK analyses will, at the angle of retrospection (X), be assessed in terms of whether and how it contributes to SSK knowledge. Then, at the angle of projection (Y), we can see not only how SSK authors work to have their analyses assessed as significant, but also to inform us about their research object. In order to see how each of the analyses contributes to SSK knowledge, however, we must wait until someone in Arena 4 starts producing their readings/writings. It is unlikely that Arena 4 is actually a domain independent from Arenas 2 and 3 (but see Collins' position in Ashmore 1989: 110). In accounting for their actions, research objects and research development, SSKers do include their own assessment of SSK. Moreover, some reflexivists in SSK (e.g., Ashmore 1989; Woolgar 1988b) shift their focus onto the analysis and accounts made in the SSK domain (Arena 2 and 3), and try to analyse and account for them within the SSK domain (Arena 2 and 3).

These arenas may be regarded differently in related programmes in science studies. In 'ethnomethodological studies of work' in the sciences (e.g. Lynch 1982, 1985, 1993; Garfinkel, Lynch and Livingston 1981; Lynch, Livingston and Garfinkel 1983, see also footnote 39) and in some STS research (e.g., Pickering 1992), the emphasis is on the study of the actions, practices and performances of scientists. They accordingly stress their departure from studying the construction of scientific knowledge. Ethnomethodologists seem to concentrate thoroughly on describing what is in Arena 1, and to argue that the other arenas are irrelevant as sociological objects. On the other hand, actor-network theorists and STS researchers in cultural studies, nowadays seem to be more or less relativising the relationship between human actions and nature (or materiality) ([1] and [2] in Arena 1), as well as the relationship between science and SSK (Arenas 1 and 2). They also accept, if not celebrate, that their own analyses employ the angle of projection. Moreover, some studies (e.g., Haraway 1989; Traweek 1992; see also Rouse 1996) declare that their projective work (Y) is directed at readers beyond science studies (Arena 4).
To me, what the Candidate is studying is any of the above phases and all of them. But how shall she address herself to any and all of them, given that what she is writing/reading is both a projective performance and a retrospective representation? At the angle of projection (Y), she is trying to focus on how to make sense of the process of knowledge construction with all the participants. Then, of course, she will wait for this research to be analysed at the angle of retrospection (X).

Lost Nonsense

Reader Now you are back to my first question of cleavage.

Writer What?

Reader You are talking of making sense of the Candidate's analytical objects and making them accountable.

Writer Yes.

Reader Then, I would ask you about your writing at the angle of projection. What if your writing is not made sense of in retrospect? What if it is unintelligible and unaccountable, being entirely nonsense?!?

Writer What do you mean by 'nonsense'?

Reader (I know that you want me to negotiate with you by describing it. You want to display that we are mutually making sense of 'nonsense'.) For you, maybe 'nonsense' is only made intelligible in a setting where we have mutually made sense of it as such.

Writer I suppose that it is only in such circumstances that we can see it made available as such in the first place. If they say, "nonsense!", that is intelligible only when we mutually let that comment pass.

Reader So, you are projecting that your readers are mutually constructing the issue with you?

Writer ...

Reader So, is that all?
The Interviewer's State of Knowledge in a Process

Now, I will again focus on the status of the Candidate's research. Let us take a look at the following two extracts which suggest how the interviewer Mushakoji's state of knowledge is seen to be in a process:

4O

150. Mushakoji I repeatedly uh-eh- think like this. After all, the point is in that figure- ("(the Fig. 2c in Yanagisawa et al. 1988)"
151. Prof. Goto So, now you know [that figure- its meaning,] don't you?
152. Mushakoji [that is precisely-]
153. Mushakoji Yes. When I was first told it by you-
154. Prof. Goto Mn.
155. Mushakoji why it is significant was-
156. Prof. Goto (laughs)
157. Mushakoji [somehow uh- ] difficult to grasp only by [[reading uh- the article, ]] 
159. Prof. Goto but when I looked at the later development-
160. Prof. Goto That's it.
161. Mushakoji it (("the later development")) has come to take place relating to that (("what is presented in Fig. 2c")), hasn’t it?
162. Prof. Goto That's it, [you see? ]
163. Mushakoji [Uh- including] the fact that the studies have come to centre on ET-1-
164. Prof. Goto Uh huh.
165. Mushakoji and including most of the hypotheses held by you currently-
166. Prof. Goto That's it, [you see?]
167. Prof. Goto Uh huh.
168. Mushakoji (Prof. Goto, IS June, 1994)

4P

637. Mushakoji And I've [been trying to write a PhD thesis.] 
638. Dr. Yanagisawa [Oh. Really? ] Will you be able to write it up in two years?
639. Dr. Yanagisawa two years?
640. Mushakoji Well no, actually I won't.
641. Dr. Yanagisawa (laughs)
642. Mushakoji In sociology, it doesn't quite work out that way.
643. Dr. Yanagisawa Mn.
644. Mushakoji So-uh- I hope- (exchanges remarks with passers-by upon opening door) after I go back-
645. Dr. Yanagisawa Uh huh.
646. Mushakoji Well, it should be something like that. I will take a couple more years. I should end up attending the ET-5 ("the then next upcoming International conference for endothelin") and that will be about the last point at which I will be able to finish up- about that point- will be just a match- the point that I will feel obliged to write it up- well- (laughs) (exchanges remarks with passers-by upon opening door) so, as I pretty much have to summarise what you've just spoken with me about- so I hope that again eh-
647. Dr. Yanagisawa Mn.

166 See Fig. 2-3 in Chapter 2 (p. 90).
The first extract (4O) shows that both Prof. Goto and Mushakoji are agreeing that Mushakoji has been gradually acquiring the way to make sense of one of the figures presented in the original 1988 article, in which the Tsukuba Group reported endothelin (Yanagisawa et al. 1988). Here, both participants are establishing that Mushakoji is gradually developing knowledge about endothelin.

The second extract (4P) presents a different issue. Dr. Yanagisawa and Mushakoji are talking about the progress of Mushakoji's research on endothelin research which features Dr. Yanagisawa as one of the main members. In this extract, Mushakoji's research is also constructed as proceeding in process\(^\text{167}\), along the lines of endothelin research.

These two extracts display that the process of knowledge construction in Mushakoji's research is based on the ongoing development of her own knowledge of endothelin (4O), and is itself ongoingly developing (4P). Mushakoji's knowledge of her science case and her own research as a process are constructed by the participant endothelin researchers and herself, at the angle of retrospection. At the angle of projection, Mushakoji seems to be working to construe her knowledge as ongoingly and potentially developing and her research as ongoingly and potentially proceeding, and thus to make her research legitimate and relevant to endothelin research. This performance resonates with the participant endothelin researchers' performances in projecting their research to the interviewer Mushakoji.

Now, we can go further: the above analysis of extracts 4O and 4P has been pursued at the angle of retrospection: at the angle of projection, I have worked to project the Candidate's research as ongoingly and potentially proceeding in a process with reference to the endothelin research discussed in this thesis; and I am working to

\(^{167}\text{Although, in this extract 4P, Mushakoji states that she would finish her thesis by the time of ET-5 (lines 647-651) which was held in 1997, this did not come about. As you can see, she submitted this thesis in June, 1999, had her viva in January, 2000, and is now in the process of resubmitting the thesis (May, 2000).** CHANGE WHEN A DIFFERENT STAGE COMES **}
project that the self-referential reflexivity, regarding the process of knowledge construction in her research with respect to endothelin research, is to be recognised as legitimate and relevant. This is achieved by claiming an identity between Mushakoji, the Candidate and myself.

I Will 'Keep Going'

Up to this point, I have been trying to analyse the process of knowledge construction in endothelin research, not as a stable 'object' nor a stable 'social construction', but as a becoming in an ongoing process. In trying to do so, our process of knowing the relevant issues is also ongoingly developing. In addition, now we can see that the Candidate's research is not only a description of the process, but also a performance of projecting her knowledge-claims in the reading/writing of this thesis:

The writing of this thesis is ongoingly proceeding. It is pursuing the process of knowledge construction in endothelin research, which is ongoingly proceeding. In addition, the reading/writing is pursued on the basis of the SSK knowledge which is ongoingly proceeding. Given that this thesis is also a kind of knowledge construction in process, there will emerge a further inquiry: in this ongoing setting, how is it able to be identified in retrospect? This inquiry will make another pivotal action explicit: namely, it is your reading/writing of her PhD thesis, which will be another essential occasion for the retrospection/projection of her thesis.

Now, we have arrived at a point where we need to consider what has already begun in our analysis -- namely, that the ongoing process of knowledge construction in endothelin research is reciprocally constituted together with the process of our own knowledge construction.

Writer My analysis also remains a becoming in process, which relates to all the arenas in Figure 4-5 (p. 109).
Reader Really? Who is studying yours retrospectively? As a candidate thesis, it is still merely performing a kind of projection (X) now. It needs to await an assessment in retrospect (X), but only if it makes sense.

Reader But what do you mean by that "now"? Where are we "now"? Perhaps the SSK assessment (Arena 4) or in the Candidate's analysis (Arena 3).

Writer I believe that we should attend to all these arenas.

Reader In addition, I don't think that you have convinced me that your writing performs projections throughout all the arenas. In fact, your writing appears to be lingering around Arena 3.

Writer Well, I have introduced others' voices — for example, those of endothelin researchers' — which potentially resonate with the reading/writing of this thesis. There is potential intertextuality with SSK texts, too, as this text is grounded in SSK knowledge and attempts to project its contribution to that field. Moreover, my writing creates an ongoing discursive space with these others, as it tries to be open to them. Like Mushakoji in the extracts (e.g., 2M-2R, p. 99-105; 4O and 4P, p. 212-213), I can project my knowledge-claims onto these arenas and make them readable/writable by others.

Reader Hold on. You haven't stopped writing about the issues from your own perspective. Whether you are truly opening the dialogic space for these others, is yet to be seen. Even though you claim that your writing is open to others, if your projection has been thrown in the air in vain, then . . .

Writer Stop right there! There is no way to predict what is in the air! Throw out the idea of cleavage between retrospection and projection right now! After all, what will be described in retrospect (X), as an assessment in Arena 4, is everything, including your effort at projective performance (Y)!

Reader Really? But the description in retrospect as an assessment (X) is surely not the end point. Don't ever forget that we are dealing with the process, and not a construction which can be finalised at some point.

Writer So? You are still saying this is not all.
Reader  You need to deal with how it can work that way not only for the sake of your own writing, but also for those in the other three domains. Unfortunately, you still seem to be merely saying that it is a triple parallel process of knowledge construction, and you need to perform this triple further in the next chapter.
CHAPTER FIVE

GIVE ME A PROCESS AND I WILL CONSTRUCT THE KNOWLEDGE

The only difference is that they have a laboratory. We, on the other hand, have a text, this present text.

(Latour and Woolgar 1979, 1986: 257-258)

Nothing conclusive has yet taken place in the world, the ultimate word of the world and about the world has not yet been spoken, the world is open and free, everything is still in the future and will always be in the future.

(Bakhtin 1984: 166)

The question of what one who is doing formulating is doing — which is a member’s question — is not solved by members by consulting what the formulation proposes, but by engaging in practices that make up the essentially contexted character of the action of formulating. Even the briefest consideration of doing formulating in consideration returns us — naïve speaker or accomplished social scientist — to the phenomenon in conversation of doing [the fact that our conversational activities are accountably rational].

(Garfinkel and Sacks 1970: 355)

[The Author is] obsessed by the idea of doing and not saying, or doing the saying by means of the doing, or acknowledging what one does while one does it.

(Ashmore 1989: 166)

<table>
<thead>
<tr>
<th>Wrighting of</th>
<th>Endothelin Research</th>
</tr>
</thead>
<tbody>
<tr>
<td>The Triple Parallel Process of Knowledge Construction</td>
<td>SSK, and The Candidate's Research</td>
</tr>
</tbody>
</table>

5-1. Analysis Analysed Back

Introduction to My Presentation

168 This title is an allusion to Latour's *Give me a laboratory and I will raise the world* (1983) and which is, of course, as Latour states, "parodying Archimedes".

169 Now you are attending to my presentation, which is based on the oral presentation given at the session, 'Reflexivity II: Infra, Meta, Supra, or Not Such a Good Idea After All?' (Mushakoji 1996). This session was one of the sessions at the Joint Conference of the European Association for the Study of Science and Technology (EASST) and the Society for Social Studies of Science (4S) held in Bielefeld, Germany, October 1996. Because I am addressing it to you now, some alterations will be made.
Hello. I will now present a paper entitled *Analysis Analysed Back*.

In pursuing my PhD research, I have been observing that the knowledge construction proceeds in a triple parallel process which is illustrated on this OHP sheet (Fig. 5-1). The triple is constituted through knowledge construction in science, sociology of scientific knowledge (i.e. SSK), and in the Candidate's PhD research itself.

In this triple process, each of the three lines seems not to proceed so independently as it intermingles with other lines to raise interesting issues in relation with others. But today, I will try my best to introduce each line in turn as three stories: first, by examining how a scientific discovery becomes integrated into a body of knowledge, and then the corresponding cases in SSK and in the Candidate's PhD research.

There is one more crucial issue in this presentation. I will try to make it a *wrighting*. The form of wrighting was originally created by Malcolm Ashmore (1985, 1989), as "a neologism connoting 'writing', 'righting' (correcting), and 'wright-ing' (making and working)". By using this term I intend to produce a text which engages in and attends to the textuality of its own construction. I hope that you can agree on such wrighting and enjoy participating in creating this text.

Now, let me start the first story.

*Story 1: The Process Wherein the Discovery of Endothelin Becomes Integrated into a Body of Knowledge in Medical Science*

The first story is based on a published article, entitled *Transition from scientific discovery to an established body of knowledge in medical science: the case of endothelin research* (Mushakoji 1995). It is about the process whereby the discovery of endothelin becomes integrated into a body of biomedical knowledge. This is based on the Candidate's PhD research during 1992 - 1995. Her research background was the
A Triple Parallel Process of Knowledge Construction

<table>
<thead>
<tr>
<th>Nature (endothelin)</th>
<th>-pursued in-</th>
<th>Science (endothelin research)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Scientific Knowledge</td>
<td>-pursued in-</td>
<td>SSK</td>
</tr>
<tr>
<td>The Process of Knowledge</td>
<td>-pursued in-</td>
<td>The Candidate's PhD Research</td>
</tr>
<tr>
<td>Construction</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Fig. 5-1 OHP 1: The triple parallel process of knowledge construction studied in the Candidate's research
field of Library and Information Science, and she had started to shift her interest to SSK. During this period, she had conducted interviews with five scientists who had been members of the group that is recognised to have discovered endothelin. I will call this the 'Tsukuba Group'. With the support of the interviewed scientists, the Candidate had illustrated the process of knowledge construction in endothelin research by focusing on the texts of formal scientific publications. First, she quantitatively measured the growth of the literature in endothelin research (see Chapter 4, p. 174). She then listed and collected the 'core papers' of this research domain and that of other genres of texts such as review, encyclopaedia and dictionary articles. The bibliography of the texts of these two groups are in your handout (Tables 5-1 and 5-2).

The scientists identified the first article in Table 5-1 as the one which reported the discovery of endothelin. From now on, I will call it 'the original 1988 article'. In this article, endothelin is reported as the most potent vasoconstrictor in endothelial cells, which implies its significance in medical science. The publication of this article can accordingly be regarded to have initiated the emergence of 'endothelin research'.

All the listed texts were analysed in terms of how they can be read to constitute the process of knowledge construction in endothelin research. This analysis was informed by Discourse Analysis and SSK, where object and text are viewed as reflexively constituted. The analysis thus builds on the assumption that the process of knowledge construction in endothelin research is reflexively constituted in the scientists' interview talk and the listed literature. In particular, the Candidate's analysis of the construction of endothelin was informed by Steve Woolgar's study of the textual organisation of a 'scientific discovery' (Woolgar 1980). The focus was specifically on these four aspects (displayed on the OHP sheet -- Fig. 5-2): that is, how the text is organised, so that we can recognise the existence, originality and significance of endothelin, as well as its integration into medical knowledge.

Then, a comparative analysis was made of how this same endothelin comes to be represented as such in these two groups of different texts. The objective was to show
<table>
<thead>
<tr>
<th>generation</th>
<th>text</th>
<th>year</th>
<th>bibliographic data</th>
</tr>
</thead>
</table>
Table 5-2. List of a variety of texts about endothelin, other than 'core papers'
(Reproduced from: Mushakoji 1995: 264)

<table>
<thead>
<tr>
<th>text type</th>
<th>year</th>
<th>bibliographic data</th>
</tr>
</thead>
</table>
1) **The existence of endothelin:**
   how the existence of the object called 'endothelin' is reported;

2) **The 'originality' of endothelin:**
   whether the 'originality (newness, novelty, unprecedentedness)' of (the research into) endothelin is transferred; if it is transferred, how that transference takes place;

3) **The 'significance' of endothelin:**
   whether the 'significance' of the reported entity endothelin is transferred; if it is transferred, how that transference takes place; and

4) **How endothelin is integrated into medical knowledge:**
   how the object called endothelin or what is presented in the original 1988 article is situated in a body of knowledge

Fig. 5-2 OHP 2: Four aspects for analysing the textual organisation of endothelin literature
how endothelin is transformed from a discovered object to an integrated fragment of knowledge in and through the texts. This transition can be seen as a *textual transformation* which can be understood as the process of knowledge construction in endothelin research. For this comparison, Latour and Woolgar's notion of 'modality' (1979, 1986; see also Chapter 1, p. 25), and Star's notion of 'simplification' in scientific research (1983) were employed. The Candidate then analysed what kind of textual transformation can be observed in the two groups, and how it can be interpreted as a process of knowledge construction.

Let me briefly report the results. Comparing the textual organisation of the texts of each group, three types of textual transformation were found: *diachronic transformation*, *synthetic transformation*, and *reconstruction of knowledge*. I will first illustrate *diachronic transformation* with reference to Table 5-3 in your handout. This is observed in the transformation in the texts of the 'core papers'. It suggests a process of knowledge construction within the continuing research of endothelin. An 'object', which did not exist previously, was identified and given a name in the original 1988 article. This text seems to have established the existence of endothelin by means of the reputation of a prestigious academic journal, the authors and their affiliation, and by the consistent and *exhaustive* description of the attendant research procedures involved. In using the term 'exhaustive', I mean that a procedure appropriate to the reporting of a discovery is described, and the text is organised in such a way as to constitute the existence of the discovered object.

In the research reports which constitute a continuation of the original 1988 article, the existence of endothelin (Table 5-3, p. 225, left-hand column) is a pre-given assumption. In these texts, it is also assumed that this existence is established by means of the reputation of both a prestigious academic journal and the involved authors with their professional affiliation. In addition, the reiterability of endothelin is demonstrated.

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170 Star's 'simplification' takes place in the process of doing science in which problems which involve a multiplicity of contingencies come to be transformed into problems which are simple enough to work on (Star 1983: 206-207). See also p. 18.
<table>
<thead>
<tr>
<th>The existence of endothelin</th>
<th>The 'originality' of (research into) endothelin</th>
</tr>
</thead>
<tbody>
<tr>
<td>OA1</td>
<td>Report of object which did not exist beforehand; the object is isolated and its nature identified; the object is named 'endothelin' (excluding title reference); involves the continuation of previous studies into endothelium-cells; the existence of endothelin is established through means of the reputation of a prestigious academic journal, the authors and their affiliation, and the consistent and 'exhaustive' description of attendant research procedures.</td>
</tr>
<tr>
<td>OA2</td>
<td>Pre-givenness of object; the existence of endothelin is established through means of the reputation of a prestigious academic journal, the authors and their affiliation; the object of reference is reiterated via citation relating it to past research thus establishing its replicability; the type of object is specified.</td>
</tr>
<tr>
<td>LE1</td>
<td>same as above; also, there occurs a subdivision of the object (endothelin comes to be referred to in the plural)</td>
</tr>
<tr>
<td>LE2</td>
<td>same as above</td>
</tr>
<tr>
<td>OA3</td>
<td>same as above</td>
</tr>
<tr>
<td>LE3</td>
<td>same as above</td>
</tr>
<tr>
<td>OA4</td>
<td>same as above</td>
</tr>
<tr>
<td>OA5</td>
<td>same as above; also, the object either is further specified, or alternatively referred to in terms of another perspective (viz., as genes 'EDN3' and 'EDNRB' whereas previously referred to as a member of endothelins, 'ET-1', and the type of endothelin receptors, 'ETB').</td>
</tr>
<tr>
<td>OA6</td>
<td>same as above</td>
</tr>
<tr>
<td>OA7</td>
<td>same as above</td>
</tr>
</tbody>
</table>

Table 5-3 The diachronic transformation of textual organisation
(Reproduced from: Mushakoji 1995: 265-6)
| OA1 | The making relevant of research for studies of "endothelium-cells", "peptides", "cardio-vascular control system", etc.; indicates implications for further research and clinical practice (the possibilities of new hypotheses, contributions to the pathogenesis of hypertension and ... pathological vascular spasm", etc.) | Report that an unknown object is isolated and purified, its structure is determined, it is referred to as 'endothelin' and regarded to be a most potent vasoconstrictor the study of which holds implications for the further development of research in related areas; indicates new hypotheses derived by the nature of the object towards previous knowledge which is related thereto ("a novel cardiovascular control system", etc.) |
| OA2 | Declaration of "an important role of endothelin in [the] regulation of the mammalian cardio-vascular system", its having "the potent vasoconstrictor and pressor actions" and its producing "a wide spectrum of biological effects." | The making into commonly owned or known fact of that which is presented in original 1988 article (the existence of endothelin and its nature comes to be regarded as something other than merely a claim but rather as what is known in the research area as a whole); synthesises individual reports; establishes a research area in relation to which reports are seen as making a contribution |
| LE1 | Declaration of the potency and physiological role of endothelin | same as above; presupposes that knowledge is accumulated with the continuation of research endeavours |
| LE2 | same as above; also, indication that endothelins "are crucial in the regulation of vascular smooth muscle tone" | same as above |
| OA3 | Introduction of endothelins in the context of various cardiac diseases; establishes a set of relationships between various diseases and the object and its nature as something in need of further research | Setting-up of research in the context of an explication of diseases; extends possibility for contribution from research area to the domain of clinical practice |
| LE3 | Statement that "Since its discovery, endothelin-I has attracted considerable scientific interest"; presupposes that an investigation into the physiological and pathological effects of endothelin is required (involves a logical 'reversal' such that the search for the nature of the object as contributing to the development of medicine and clinical practice is replaced with a logic in which the developement of medicine is seen as contributing to the further understanding of the nature of the object) | Setting-up of research in a context wherein the development of medicine contributes further to explicating the nature of endothelin; presupposes a relationship between endothelin and disease |
| OA4 | Report of novel significance for endothelin with the declaration that endothelin-I has an essential role in normal ontogeny and haemodynamic regulation | presupposition that there exists a gradual development of research in this area; indicates points of minor consensus and those which "remain unclear"; assumes a consistent and systematic body of knowledge requiring the possible filling-in of gaps |
| OA5 | Declaration of an important role for a single type of endothelin receptor; indicates new hypotheses and possibilities for further research; indicates clinical application with reference to "the identification of at-risk individuals in the Mennonite population, which has a high incidence of megacolon." | Background engagement with the problems involved with Hirschsprung's disease; through reference to accompanying paper (OA6), relates findings from the perspective of genetics to previous body of knowledge; synthesises individual reports; speculates about the gaps yet to be filled in |
| OA6 | Declaration that endothelin-B receptor is an essential component in the normal development of two neural crest-derived cell lineages, viz. enteric ganglion neurons and epidermal melanocytes; with reference to the accompanying paper (OA5), indicates that this receptor also plays an important role in humans | Engagement with studies of the development of neural crest as its background; relates what is currently known about endothelin to those studies; creates synthesis of endothelin studies with another subject through reference to accompanying paper (OA5); integrates what is defined in individual studies and proposes what can be hypothesised as a result of this integration |
| OA7 | Declaration that endothelin-3 "plays an essential role in the normal development of two neural crest-derived cell lineages, epidermal and choroidal melanocytes and enteric ganglion neurons" and that "endothelins emerge as important regulators of mammalian neural crest." | Engagement with previous endothelin studies as its background; indicates what is unexplored in current studies; through reference to accompanying papers (OA6, 7), introduces a new phase of the nature of endothelin towards the previous research area |
through citations of the previous report(s), thus ratifying its replicability. What is particularly interesting here, is that while the identity of endothelin is itself maintained as it is, its further specification and subcategorization is nevertheless achieved by referring to it in the plural form (LE1) and by altering the referent from 'endothelin' to the 'gene' of endothelin (OA5).

In giving attention to the 'originality' of endothelin (Table 5-3, p. 225, right-hand column), the original 1988 article declares that it undertakes the presentation of a "novel" object and a "novel" idea. In contrast, in subsequent studies, even though the initial report of endothelin's existence is attributed to the original 1988 article by explicitly stating this fact or implicitly by citing this article, the focus of these studies shifts to that of their own originality -- that is, the originality of the subsequent work itself. This latter movement is especially remarkable in the papers that comprise the basic research of the third generation (OA4-6).

With regard to the 'significance' of endothelin (Table 5-3, p. 226, left-hand column), its significance is asserted both with respect to the research interests endothelin raises and in that it is being applied in clinical practice. What is interesting here is that the implications of the basic research for its application, especially in clinical practice, are generally emphasised; but conversely, in a letter that reports on the development of a related medicine (and that thus appears to be the most 'clinical' of the texts [LE3]), it is emphasised that the development of this medicine can make a potential contribution to the basic research.

Comparing each text for 'how endothelin is integrated into medical knowledge' (Table 5-3, p. 226, the right-hand column), it can be observed that what was reported in the original 1988 article has established an area of research, that this area has subsequently developed with the accumulation of related studies, and that the area has come to be extended into studies which have clinical applications. Throughout this process, the results of the studies are articulated as filling in the unknown parts of a body of knowledge (LE1-2 and OA4-6). Furthermore, it can be observed that the prior body of work is shifted into another context, thereby generating a new mode for its
recognition (OA3, LE3, OA5-7). In each text, individual past reports are always integrated into the new exposition, and those reports come to be seen not as distinct remarks in single texts, but as constitutive of 'a body of knowledge' which is generated by drawing upon "the disproportionate amount of linkages, resources and allies [which thereby come to be regarded as] locally available" (Latour 1987).

In this way, diachronic transformation can be observed to constitute a process of knowledge construction. Let me briefly sum up this process with the four generations of texts in endothelin research, as indicated in Table 5-1. The original 1988 article reporting on the existence of a newly discovered entity, provided the basis upon which all successive studies in endothelin research could subsequently take place. It can thus be allocated into the category representing the first generation of endothelin research. The next original article (OA2) demonstrates that there are three different forms of endothelin which together constitute 'a family', while the two letters (LE1, LE2) identify two different types of endothelin receptors. These texts were assigned to the second generation of work, since they contribute to the explication of endothelin's basic nature. The third generation includes reports that implicate the current state of endothelin research. This generation appears to have more complicated lines of research development than its predecessors. One line of development begins with the elucidation of the nature of endothelin and moves on to the exploitation of its associated antagonists as well as to its applications for clinical practice. That is, this line of work describes a process of development from basic research to application. For example, the original article published in 1993 (OA3) is concerned with the possibilities for the application of what is known about endothelin for the treatment of pulmonary hypertension while the letter published in that same year (LE3) reports on the role that a developed medicine might be able to play in further defining the function of endothelin. The last four articles (OA4-5) however, exhibit a different line of development in the basic research itself. This line of development has occurred with the introduction of genetic engineering methods into previous research, resulting in a new understanding of endothelin.
Now, with Table 5-4, let me explain how the transformation between the texts of the second, varied, group of texts can be interpreted as engaged in the mode of knowledge construction I am calling *synthetic transformation*. Endothelin, as presented in the original 1988 article (the upper right-hand column), appears as a pre-given substance, in the review article, commentary, textbook and dictionaries. In a review article or textbook, many reports are synthesised, and the repetitiveness in what is reported there is itself displayed. This repetitiveness guarantees the research as an independently established area. Furthermore, endothelin is recognised by virtue of the backgrounding of the many texts which are either cited explicitly or else referred to implicitly here. As examined elsewhere (i.e., this thesis, Chapter 2: p. 87-93), the result of this recognition is such that the text of endothelin comes to be seen in (and as) a mesh of innumerable related texts, which can be referred to with the term *intertextuality*.

While this intertextuality is more explicit in the review article and the textbook which feature multiple citations to other works, it is present, though in an inverted form, even in the case of one of the dictionaries (D12) in which citations do not appear. Endothelin as presented in this dictionary can (only) be read with the implicit interweaving of the whole body of texts in this research area.

In the review article, commentary and textbook, the 'originality' of endothelin (the upper right-hand column) is attributed to the original 1988 article; however, in the dictionaries, the originality of the research seems not to be a relevant concern. With regard to the 'significance' of endothelin (the lower left-hand column), the concern is with both its significance for basic research and for clinical practice. It can also be said that due to the function of these genres in synthesising research (Mushakoji 1984, 1990), there occurs an 'automatic' ratification of endothelin research as an area that simply *is* significant.
| RA1 | Reference to endothelin as a pre-given: endothelin conceptualized as an entity; the existence of endothelin established through means of the reputation of a prestigious academic journal, the authors and their affiliation and through means of the genre of scientific literature in which they write (constituted by 117 research reports); displays the intertextuality of 'endothelin' (all cited texts are referred to as generative of what is recognized as 'endothelin') | Declaration that the isolation and purification of endothelin was first reported in original 1988 article: indicates that the nature of the object is "extremely unique" |
| CO1 | Reference to endothelin as a pre-given: the existence of endothelin is established through means of the reputation of a prestigious academic journal, the authors and their affiliation and through means of the genre of scientific literature in which they write, subdivision of the object (endothelin comes to be referred to in the plural and its types are specified) | Declaration that the isolation of endothelin was first reported in original 1988 article; also refers to new developments in successive research ("the first studies with orally active endothelin antagonists", etc.) |
| TB1 | Reference to endothelin as a pre-given (reiteration of endothelin as an object of learning); the existence of endothelin is established through means of the reputation associated with the authors and their affiliation and through the genre of scientific literature in which they write; establishes grounds that endothelin is a reiterated object and that there exists an established area devoted to its research; extension and reinforce mentioned elsewhere [Chapters 17 and 43 of this textbook]. | Representation of endothelin(s) as factor sharing similar actions; statement that endothelin was first reported in the original 1988 article no longer occurs in Chapter 8 Section D wherein the nature and functions of endothelin are discussed, although bibliographic data is mentioned elsewhere [Chapters 17 and 43 of this textbook] |
| D1 | Reference to endothelin as a pre-given (intensified reification endothelin as object of learning; presupposes that endothelin can be cited in a reference volume, whose existence is already given) | Establishment of endothelin as a medical term; no citation of original 1988 article text; referring to facts about endothelin in general (rather than specific) terms |
| D2 | Reference to endothelin as a pre-given (same as above); accomplishes the status of related research findings as constitutive of a single oeuvre ("inversion": the intertextuality accomplished by means of citation ceases, indicating that endothelin is the descriptive object of an entire oeuvre) | Establishment of endothelin as a medical term; no reference made either to original 1988 article or research undertaking which that text reports |

**Table 5-4 The synthetic transformation of textual organisation**

**The existence of endothelin**

| RA1 | Presupposed the significance of endothelin studies through the genre of scientific literature with the announcement of a "the new trend in circulatory pharmacology"; significance of endothelin research established through the characterisation of that work as pursued with the publication and accumulation of related literature; refers to the significance of developing research and clinical practice through citation ("it seems that the function of research ... has an extremely important implication"), or through the listing of related diseases and a corresponding indication of how related medical developments are to be pursued |
| CO1 | Declaration that the development of medicine related to the functions of endothelin and research into the role of endothelin in health and disease indicates "exciting clinical potential" for endothelin antagonists |
| TB1 | An examination of endothelin's relation to disease and various phenomena related to such disease; states that endothelin is the most potent vasoconstricting factor (Chapter 8 and 43 of this textbook), and that it plays an important role in the pathogenesis or maintenance of hypertension |
| D1 | Description of endothelin as "the most potent known vasoconstrictor" lists a set of endothelin functions related to clinical practice and associated diseases |
| D2 | Description of endothelin as possessing a "potent vasoconstrictor" declares that endothelin may play a role in controlling blood pressure and may also function as a neurotransmitter |

**The 'originality' of research into endothelin**

| RA1 | Presupposition that text represents a summary of the current trends in research area as a whole; report of individual studies presented as a single research theme despite the fact that individual reports are construed to exist under a consistent system with reference to a unitary body of 'structured' knowledge which is to be saturated through the filling of gaps; assigns to the area an orientation in co-operative tasks (defined in terms of problems, hypotheses and the potential for further research; research results are not regarded as belonging to specific individuals but rather as the outcome of work in the research area as a whole); extension of research community knowledge to include those working in pharmacology; generalises individual reports in terms of the introduction of facts; summarizes several conditionally distinct reports through adjectival reference ("many", "pretty much", etc.); entails the distribution of knowledge (presupposes that the body of knowledge is being constituted in complimentary fashion ("the details is not described in this paper but see other review article[30]) |
| CO1 | Discussion of the integration of endothelin studies into the body of medical knowledge and clinical practice (the clinical potential of endothelin antagonists is examined); distinguishes areas of established knowledge from that which is in need of further investigation, especially as this relates to clinically significant developments |
| TB1 | Establishment of research in the body of systematic knowledge organised under the subject "hypertension" (referring to endothelin in the context of hypertension and focusing on its relation to disease); presents endothelin as an object of knowledge in an oeuvre as established through a wide-range of citations; indicates areas for further research and areas involving findings which are inconsistent when endothelin, as a subject, is related with other known phenomena; indicates controversial issues which arise with the synthesis of individual reports |
| D1 | Presentation of endothelin as a medical term locating it alphabetically within a terminological index (with some citation taking place); descriptive work establishes the constitutive nature of endothelin within a coherent body of knowledge to which it belongs |
| D2 | Presentation of endothelin as a medical term locating it alphabetically within a terminological index: intertextual nature of the referent is transformed in the text (establishes endothelin as an object of reference of an oeuvre as constituted by a multiplicity of alternative texts comprising the research area as a whole); endothelin regarded as belonging to no specific individual given that citations no longer occur, endothelin regarded as that about which everyone (within the research community) knows |
This same point can be made with regard to 'how endothelin is integrated into medical knowledge' (the lower right-hand column). Just because endothelin becomes a theme for review articles and commentaries, it can be seen as having a significance in the broader field, and just because it is adopted as an item in textbooks and dictionaries, endothelin research is displayed as situated within a body of systematised knowledge. In the review article and commentary, individual studies are summarised and thus transformed into 'condensed knowledge,' in the common ownership of the field. In the textbook and dictionaries, what counts as a summary of the corpus of texts (McHoul 1982: 122-128; Edwards and Potter 1992: 37-40; Mushakoji 1990) itself becomes further generalised, and the descriptions themselves come to be simplified (Star 1983) with an implicit intertextuality. In this way, these texts arrive at the stage where endothelin is presented as integrated into 'a body of knowledge'.

Finally, a further dimension of the process of knowledge construction was found. The texts of the last four articles in the group of 'core papers' (Tables 5-1 and 5-3) can be interpreted as suggesting the reconstruction of knowledge. With the help of accounts from interviewed scientists, it was found that these texts constitute a shift in endothelin research: by 1994, the research had been restructured by introducing genetic engineering into the previous modes of research. This shift brings with it a recognition of endothelin as "a two-faced Janus" (Latour 1987: 4; see also Chapter 4): the most potent vasoconstrictor (as biochemically and physiologically identified in 1988); and as the 'gene'171 causing Hirschsprung's disease, which is a disease of the intestine.

I will conclude this first story with what this OHP sheet illustrates (Fig. 5-3). In analysing the texts of endothelin research, we can observe the diachronic and synthetic transformations, and the reconstruction of knowledge. This process of knowledge

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171 The gene structure of endothelin itself was identified in 1988. The shift in 1994 was brought about with the recognition that the lack of the endothelin gene in mice and humans results in abnormalities (e.g., Baynash et al. 1994, Kurihara et al. 1994); an understanding that did not follow from endothelin as the most potent vasoconstrictor. Just as Fujimura has shown with her study of how the concept of the 'gene' has changed and developed in basic medical research (1996), a new set of tools in genetics has technically (re-)invented the nature of endothelin.
The original 1988 article —— the continuing research ———> diachronic transformation

production of review articles, encyclopaedias, dictionaries, etc.

synthetic transformation

Fig. 5-3 OHP 3: The process of knowledge construction in endothelin research
construction is of course retrospectively achieved. That is, the text of the original 1988 article is recognised as reporting an original and significant discovery, with the intertextuality of those texts that are categorised as emerging from this discovery. In other words, the 'discovery' is not the cause of the production of the rest of the texts, but the consequence of the intertextuality of all the texts.

Moreover, these types of knowledge construction are achieved within the Candidate's research where these texts have been observed to constitute a series, in which the transformation of the same-but-different object(s) takes place, and in which, in turn, the transformation is recognised as a process of knowledge construction. This achievement remains, however, open to be analysed again. You are participating in the story in order to analyse-back, so that this process of knowledge construction in endothelin research will ongongly proceed, along with what we know about it, which will, of course, also be in a process.

But next let me tell you the second story.

**Story 2  The Process Wherein the Notion, or the Discovery, of Reflexivity Becomes Integrated into SSK Knowledge**

One of the basic assumptions made in the first story was that the scientific texts and the knowledge-claims made in them are "isomorphic" (Woolgar 1980). That is, it was assumed that the existence and nature of endothelin are reflexively constituted in and through the analysed scientific texts. This assumption is often referred to by SSKers with the term 'reflexivity' (e.g. Woolgar 1988a: 31). In its simplest form, it is the view that object and text are isomorphic (showing OHP sheet, Fig. 5-4). For this presentation, and for the time being let me call this kind of reflexivity reflexive-constitutedness (see also Chapter 1, p. 5).

There is another kind of reflexivity, which refers to the potential self-referential nature of SSK studies. SSKers study scientific knowledge. But when they consider their own achievements and claims, how do they separate these from those made in science, and their own activities from scientific activities (Ashmore 1989). For
Fig. 5-4. OHP 4: Reflexive-constitutedness
example, it becomes an issue whether the Candidate's research on 'the process of knowledge construction' in endothelin research is itself in a process of knowledge construction.

The second story is about how the notion, if not the discovery, of 'reflexivity' becomes integrated into SSK knowledge. Here, I want to emphasise that telling this story is not simply a matter of talking about reflexivity, but, as far as possible, engaging in it in this session (Woolgar and Ashmore 1988: 4).

As I have discussed elsewhere (i.e., Chapter I), SSKers' commitment to reflexivity is often that of a combination of reflexive-constitutedness and self-referentiality; particularly in the subgroup called 'reflexivists' who follow the 'reflexive programme'. This enterprise has received a variety of criticisms from both inside and outside SSK (Ashmore 1989) and has resulted in a number of works of, about and with 'reflexivity' in SSK and its related research domains.

Following a procedure similar to the one used in the first story, the texts of 'core' papers and books of reflexivity studies were listed and collected; these are in Table 5-5 of your handout. These texts were analysed in terms of the process whereby reflexivity becomes integrated into SSK knowledge. In reading and analysing the texts, the 'textual organisation' which constitutes the notion (or 'discovery') of reflexivity was examined, and then the textual transformation among the texts was analysed in order to interpret how the same 'reflexivity' shows differences in and between the texts. As a result, SSK reflexivity studies were categorised into five types, which are briefly described in Table 5-6 of your handout. They are Post-hoc Reflexivity, Argued and Counter-argued Reflexivity, Reflexive-constitutedness, Self-referentiality (and Self-exemplification) and Reflexivity as Textual Forms.

Now, can we see from this, just as we have done in the first story, 'the process whereby reflexivity is integrated into SSK knowledge'? One may trace back the origins and/or roots of these five reflexivities in terms of their SSK advocates. David Bloor is the first proposer of a kind of Post-hoc Reflexivity as one of the tenets of the 'Strong Programme' (1976, 1991). Steve Woolgar introduced notions such as 'The Problem'
Table 5-5. A Variety of Texts of Reflexivity Studies


<table>
<thead>
<tr>
<th>text</th>
<th>year</th>
<th>bibliographic data</th>
</tr>
</thead>
</table>

Reflexivity (Self-Reference in SSK) — Discovery of 'The Problem' (1978-1989)

<table>
<thead>
<tr>
<th>text</th>
<th>year</th>
<th>bibliographic data</th>
</tr>
</thead>
</table>
## Reflexivity (Self-Reference, Reflexive Constitutedness, Reflexivity as Textual Form) -- Research Programme and Criticisms (1985-)

<table>
<thead>
<tr>
<th>text</th>
<th>year</th>
<th>bibliographic data</th>
</tr>
</thead>
<tbody>
<tr>
<td>BC4</td>
<td>1988</td>
<td>Woolgar, Steve. Reflexivity is the ethnographer of the text. IN: BK8: 14-36.</td>
</tr>
<tr>
<td>BC11</td>
<td>1988</td>
<td>Pinch, Trevor and Trevor Pinch. Reservations about reflexivity and New Literary Forms or Why Let the Devil have All the Good Tunes? IN: BK8: 178-199.</td>
</tr>
<tr>
<td>OA7</td>
<td>1992</td>
<td>Hamlin, Christopher. Reflexivity in technology studies: toward a technology of technology (and science)? Social Studies of Sciences 22: 511-44.</td>
</tr>
</tbody>
</table>
Miscellaneous Related Research and Criticisms

Reflexivity in Ethnomethodology (1982-1996)

<table>
<thead>
<tr>
<th>text</th>
<th>year</th>
<th>bibliographic data</th>
</tr>
</thead>
</table>

Others

<table>
<thead>
<tr>
<th>text</th>
<th>year</th>
<th>bibliographic data</th>
</tr>
</thead>
</table>

172 The genre of this text is hard to define. Here, CH is chapter in a handbook. But this text can be regarded as, by the authors' implication (Ashmore, Myers and Potter 1995) as a paper in a "vademecum"; a "vademecum" itself being a review or diary.

173 Conference proceeding.
<table>
<thead>
<tr>
<th>Reflexivity</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Post-hoc Reflexivity</strong></td>
<td>Critics' term referring to David Bloor's reflexivity (Woolgar 1984; Pinch and Pinch 1988); this reflexivity is advocated as &quot;a requirement&quot; for SSK, but it does not seem to be a requirement of practice in that it is not evident in Strong Programme SSK work</td>
</tr>
<tr>
<td><strong>Argued and Counter-argued Reflexivity</strong></td>
<td>Refers to the content of SSK work which discusses reflexivity — for example, work which establishes 'the problematics' of reflexivity, which proposes or bans reflexivity as a topic for SSK, which argue over the degree of reflexivity in other work, etc.; this work of arguing and counter-arguing about reflexivity may or may not be reflexive in form (see Reflexivity as Textual Forms, below)</td>
</tr>
<tr>
<td><strong>Reflexive-constitutedness</strong></td>
<td>Refers to the idea that reality and text are &quot;isomorphic&quot; and the arguments related to this idea; also, refers to work which studies how reality is constituted within text, as well as how text can be recognised as this reality</td>
</tr>
<tr>
<td><strong>Self-referentiality</strong> (Self-exemplification)</td>
<td>Refers to the assumption of the similarity between the object and the form of investigation; in SSK, this similarity is established when one recognises &quot;the inherently self-referential character of modern sociology of scientific knowledge&quot; in which its topic and methods are strongly related (Ashmore 1989, xxiii); and that this may or may not be a 'problem' of SSK's self-exemplifying character</td>
</tr>
<tr>
<td><strong>Reflexivity as Textual Forms</strong></td>
<td>Refers to the performance of a work in which its own textuality is displayed for the reader to be aware of and engage with. Includes those works which are called New Literary Forms (NLFs) or Alternative Literary Forms (ALFs), and those which display a variety of textual devices. However, work featuring such devices can only be reflexively successful when the reader recognises, engages in, and enjoys such a performance in his/her reading. Such work does not necessarily engage in discussion about reflexivity — it only needs to be reflexively written and read with a celebration of its textuality</td>
</tr>
</tbody>
</table>
which can represent Argued and Counter-argued Reflexivity, Reflexive-constitutedness and Self-referentiality. Malcolm Ashmore's 'wrighting' (1985, 1989) can be evaluated as a radical and epoch-making textual experiment (i.e., Reflexivity as Textual Forms) on behalf of Self-referentiality (and Self-exemplification) in and for SSK. Michael Mulkay initiated reflexivity as 'self-reflection' (Mulkay 1985) and proposed 'New Literary Forms' (i.e., Reflexivity as Textual Forms). Interacting with SSK, Garfinkel and his students employ reflexivity as one of the programmatic and technical terms of ethnomethodology (Garfinkel 1976; Leiter 1980; Heritage 1984; McHoul 1982; Lynch 1993, 1996; Slack 1996). Then, of course, these SSK reflexivities are constructed in relation with other kinds of reflexivity established within sociology in general, or, even more generally, within the social sciences, arts and humanities.

But can we observe the same textual transformation that we have observed in the first story? That is, can we observe reflexivity as an object which has been 'discovered' by an SSK discoverer? Can we assume that this 'discovered' reflexivity has been 'integrated into the body of SSK knowledge' in successive texts? Here, please note that my emphasis is that reflexivity is constructed in these SSKers' texts. Indeed, Post-hoc Reflexivity is often seen to be merely constructed as is Argued and Counter-argued Reflexivity. The last three reflexivities in Table 5-6 seem, however, not to be merely constructed. Presumably, due to the "reflexivity of reflexivity" (Mehan and Wood 1975), the studies which can be categorised into these types themselves acknowledge that the reality of reflexivity is elusive outside of the discussion of, and engagement in, reflexivity. These studies thus display a commitment to the constructed (as opposed to 'real') character of reflexivity. It seems to me that they consequently create some interestingly paradoxical texts which, while describing (and informing us of) reflexivity, self-consciously engage in their own textual work of constructing this 'reflexivity'. The textual forms employed then appear to be concurrently, perhaps chronically, open to their own deconstruction.
Thus, compared with the texts of endothelin research in our first story, the texts in this second story can be read as *not* establishing 'reflexivity' as a part of SSK knowledge in the sense of filling a gap in previously accumulated knowledge. Of course, these studies still give a certain definition of reflexivity, set up its history, attribute its originality to someone or some study, and attempt to integrate it into SSK. But while engaging in this work of integration (in the course of stating programmatic injunctions, policing the orthodox use of the terms, arguing for applicability or need for follow-up studies, and so on), most of the studies simultaneously alert us to the constructive nature of their own textual work. Similarly, this second story makes a report of five types of reflexivity and suggests their origins: but you (should) know that the story itself is underlining its own constructive work.

We will now move to our third story. In the beginning, I told you that the process of knowledge construction can be regarded as ongoingly taking place along the three parallel lines of science, SSK, and the Candidate's research. So, what of the last line, the one which most clearly implicates our participation here-and-now in this session?

**Story 3: The Process Whereby the Candidate's Knowledge-Claims may, Hopefully, Become Integrated into Our Knowledge**

Before we start a discussion -- or a negotiation -- about the potential knowledge-claims made in this presentation, I will tell you what has happened to the Candidate's research after the publication of the article on which the first story is based (Mushakoji 1995).

The Candidate sent the pre-prints and reprints to the participant endothelin researchers and her social scientist colleagues. The endothelin researchers were asked to comment on whether they could re-confirm what she wrote about the process of knowledge construction in their research. The Candidate also invited them to participate further in accounting for their research. Her social scientist colleagues were likewise asked to make comments, as well as to let her include their comments into her later research.
Table 5-7 is a list of some responses to Mushakoji’s 1995 article. The first six rows (1-6) are responses from endothelin researchers. The notice from the editor of the journal (i.e., *Japan Journal of Medical Informatics*) is appended to them (row 7). The last four rows (8-11) are the comments from her colleagues in social sciences.

Let us first examine the endothelin researchers’ responses. The first (row 1) is taken from a letter written by a professor in the Tsukuba Group. In the remark "I was amazed that having been written by someone from a different discipline, the paper expresses such a good understanding of its subject matter", this scientist pays the Candidate a bit of a compliment. This compliment is interesting, as its relevance seems to depend upon Mushakoji’s outsider status. Her status as an outsider of the community of endothelin research is worked up as a resource with which to compliment her on her grasp of the process of knowledge construction in their research. Were it otherwise -- that is, were Mushakoji regarded as an insider -- her work might simply fail in its adequacy. The knowledge of the "subject matter" is, in making this compliment, legitimately established as the "collectivity’s corpus of knowledge" (Sharrock 1974). However, this "subject matter" is not endothelin nor its research; rather, it is the process of knowledge construction. In that the specific topic is indeed ‘knowledge of endothelin and its research’, the endothelin researchers’ community may seem to have a legitimate claim on its ownership. Yet, is it not also our research object in SSK? Indeed, the issue of the ownership of such "cultural knowledge" can be open to negotiation. That is, what comes to count as knowledge, and who is taken as having a legitimate claim on it, can be regarded as an upshot of the kind of ‘pragmatic intersubjectivity’ (Edwards 1997: 114-141) achieved, here, in this correspondence with scientists.

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174 What counts as an insider/outsider is discussed in Chapter 3.
175 This sense of ‘shared knowledge’ is explicated by Edwards "as a participant’s practical concern; what their talk treats as shared, and when, and how" (1997: 114-141; see also Chapter 1, p. 22-23).
Handout

Table 5-7 A variety of responses to Mushakoji (1995)

<table>
<thead>
<tr>
<th>Respondents</th>
<th>Date (&amp; genre of text)</th>
<th>Excerpts from respondents' responses to Mushakoji (1995)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1/ endothelin researcher a</td>
<td>10 August, 1995 (letter)</td>
<td>&quot;I read your article which took me approximately one hour to read. I was amazed that having been written by someone from a different discipline, the paper expresses such a good understanding of its subject matter. Since I haven't yet been able to read it more closely, I cannot comment on the details, but I will mention a little of what comes to my mind based on what I have seen... What I wondered mostly after reading your paper was why you do not publish it in English. Is it not that if you do not publish it in English, nobody will read it? Since you have opportunities seeing you're in England, why don't you think about a way to publish something in English? In our field of natural sciences, currently, the articles published in Japanese would 'not count' as a contribution.&quot;</td>
</tr>
<tr>
<td>2/ endothelin researcher b</td>
<td>21 December, 1995 (letter)</td>
<td>&quot;As concerns the development of endothelin research, there is one more issue that I think I should mention... Both the activity of [the committee for the naming of receptors, in the International Pharmacological Association] and that of the International Conference of Endothelin which is held biannually takes a great role in stimulating what is to be known of endothelin.&quot;</td>
</tr>
<tr>
<td>3/ endothelin researcher c</td>
<td>5 July, 1995 (e-mail)</td>
<td>&quot;I have just received your manuscript describing the process of... How impressed I am with your logic used to analyze the world of the endothelin research (so called endothelinology, or endothelin world). Your logic is so clear cut and so precise. I would like to recommend young students in my lab to read your paper because they seem not to have their own logic (a kind of measure) to analyze or judge, although most of the established scientists have gotten such measure NATURALLY (this point is a problem from a viewpoint of officially UNTRAINED debate). The process consisting of three steps (transformations) is nicely formulated...&quot;</td>
</tr>
<tr>
<td>4/ endothelin researcher d</td>
<td>21 June, 1996 (e-mail)</td>
<td>&quot;As to our endothelin research, our group has now established a relation that the ETB receptors responsible for pharmacologically heterogeneous responses (for example, one is sensitive to some antagonists, another is insensitive) to endothelin peptides of some blood vessels, are derived to a single known ETB receptor, using ETB-receptor deficient mice... These ideas have been originally developed in other fields but never applied to ET fields. I hope that our attempt will not be a simple case only in the ET fields and bring some impacts to other fields... Again, I greatly appreciate to your warm encouragement and...&quot;</td>
</tr>
<tr>
<td>5/ endothelin researchers d &amp; e</td>
<td>8 November, 1996 (interview talk and ethnographic note)</td>
<td>(After reading Mushakoji [1995], commented that she did not know or appreciate that &quot;there is someone who studies that kind of work.&quot;) &quot;[I]t looks like that... when we tell people, we are also really like-somehow it ((the process in which endothelin was discovered and its research has developed)) comes out looking... in a pretty neat way... but in reality, it is actually rather... there are actually some things which occur almost by chance, as it were... it's like there is an- um- issue which is not very clear, and then it finally becomes clearer, and then when we look back in retrospect, we can then finally start seeing things very much like 'oh, now I see, it had been this kind of story all along'.&quot;</td>
</tr>
</tbody>
</table>

175 Since e-mail only accepts alphabetical letters, e-mail correspondence is the only occasion that we (i.e., Japanese) write messages in English. I will leave this correspondence just the way they are in English, without modifying its grammar and spelling. They are thus our prototype 'Japanese-English'. The same point applies to the correspondence with the social scientist d (row 10).
In the talk (where recording was not possible), this researcher indicated the points that Mushakoji needs to revise in her subsequent research. That is, she should treat the paper she published in the journal *Nature* as part of the textual genre of 'original article' rather than 'letter'. He points out that what is categorised as a 'letter' in *Nature* is regarded by the scientific community as an 'original article'.

<table>
<thead>
<tr>
<th>Responder</th>
<th>Date (&amp; genre of text)</th>
<th>Notice of Acceptance of the Submitted Article of Mushakoji (1995)</th>
</tr>
</thead>
<tbody>
<tr>
<td>The editor of <em>Japan Journal of Medical Informatics</em></td>
<td>28 July, 1995 (letter)</td>
<td>We are pleased to inform you that your article has been accepted for publication with the title &quot;The process of how scientific discovery becomes transformed into an established body of knowledge in medical science: the case of endothelin research&quot; as genre &quot;original article&quot;. This acceptance is conditional upon the revision to your (original) title as indicated above. Also, would you please send along a diskette of related word processor text files, as well as the list of the word processing packages you have used.</td>
</tr>
</tbody>
</table>

Excerpts from respondents' responses to Mushakoji (1995)

7 / social scientist a

5 July, 1995 (letter)

"In that the object and conclusion are not really clearly specified, it is not actually possible for me to comment further. However, on the basis of what has been written, it seems that you haven't actually stated anything particularly new. It is not clear where the originality of the author lies, and since the issue of 'reflexivity' is your conclusion, this in itself is in no way really a new thing.

.......

One is left with an impression that the assumptions you make in your writing are at best somewhat superficially arrived at. This fact seems to be reflected in the expression, composition and analysis developed in the article itself. Isn't it the case that the conclusion comes first and that only then is the analysis conducted in order to fit that very conclusion? This ambiguity seems to have some relation with the problem of 'reflexivity' stated above especially with whether the mode of proceeding [the process of knowledge construction in endothelin research] is a conclusion or an assumption.

.......

Quite apart from this, I should add that I think the only response that a reading this article will generate is that of irritation. My own feeling is that I wouldn't want to spend the time on any of your other work as well."

8 / social scientist b

5 August, 1995 (letter)

"When you state that the integration of knowledge is socially constituted (organised), do you mean that this integration is pursued only by means of text, or is the text only one of the sites for that integration?"

9 / social scientist c

17 August, 1995 (letter)

"Let's assume that your argument is sustainable -- that the emergence of various contributions to the scientific literature and 'the process of how medical information becomes transformed into a body of knowledge' are reflexively related. If a researcher accepts what is treated as pre-given knowledge in just such terms -- that is, as pre-given -- and reads the literature [...] in these terms, isn't it rather obvious that her result would be a reiteration of the common-sense view towards scientific literature which has already been stated by [the previous model]?"

.......

If you are really concerned with the possibility of deducing an ethics of researchers' practice as informed by the notion of 'reflexivity', ... I cannot see how it would be particularly fruitful as a PhD thesis, and I do not know whether these issues concerning the ethics of practice will make a meaningful contribution to human 'knowledge'."
"The pre-print was quite different from what I'd expected... To me, it looked that you actually accepted the traditional model as it was... It might be irrelevant to put it like this, but I thought that you used this familiar model because it is to be submitted to the Japan Journal of Medical Informatics,...

What do you mean when you say that 'the process of how discoveries become integrated into a body of knowledge' and that which is presented in the literature are reflexively established? On this reading, isn't it the case that all we can say is that the texts which are presented in the literature having various functions reflect 'the process of becoming integrated into a body of knowledge'? Instead of this, however, isn't it your implication that, conversely, the texts presented in individual situations themselves rise to the process referred to as that 'of becoming integrated into a body of knowledge'? Is it relevant to refer to this as 'reflection' and as rather 'reproduction'?...

Your explanation here can be read as stating that... Is that what you really want to indicate though? When you state 'endothelin' is presented in intertextuality, do you mean that its existence by saying 'endothelin'?

Doesn't your article still need some further explanation?
It is also interesting to consider the second half of row 1 in terms of Self-referentiality. This scientist's query regarding the language of Mushakoji's article suggests that her research activity has been compared with his own. As I stated somewhere else (Chapter 4, p. 212-214), the progress of her research has occasionally been calibrated in comparison with the progress of endothelin research in the mutual interactions between the scientists and herself. It may well be that such mutual comparisons between the ones' own discipline and that of others is not extraordinary -- even between science and SSK.

Rows 3 and 4 are taken from e-mail messages sent by the biochemist who played one of the main roles in the Tsukuba Group's achievement. This biochemist refers to the significance of Mushakoji's article (1995) in terms of the "logic" (row 3) she employed in it. What is referred to with this term "logic" is then paraphrased as "a kind of measure". It seems to me that with this term, the biochemist implies a set format for an accurate account of "the world of endothelin research". But then, it may be that this biochemist does not view the process of knowledge construction in endothelin research in this way, but is instead referring to the form of the account, such as that constructed in the first story of this session.

These endothelin researchers seem to be accepting the significance of the Candidate's research object, and encouraging her to pursue it further. Some endothelin researchers appear to be participating in the Candidate's research by commenting on the article (row 5) and by modifying it (rows 2 and 6). Thus, it may be that the text of Mushakoji's article (1995) and her approach have worked to set up the dialogical space in which the Candidate's research object can develop as 'a becoming' (Chapter 4, this thesis) along with endothelin research. In this sense, the constitution of endothelin, endothelin research, and 'the process of knowledge construction', are mutually pursued by these researchers and the Candidate herself. The Candidate's research object can

177 My acknowledgement to the participants at the DARG session on 21 February 1996, for providing a useful commentary for the analysis of these comments. See also Section 5-3.
thus be claimed as potentially and ongoingly proceeding in a process of knowledge construction.

Next, let us examine the response of the journal editor. Although *Japan Journal of Medical Informatics* is the major journal of Medical Informatics—a research domain belonging to medical science—it nevertheless accepted Mushakoji's SSK-informed article (1995) outright, with only one condition: the deletion of two sets of quotation marks in the title. The title in the initial submission appeared as "The process of how 'scientific discovery' becomes transformed into 'an established body of knowledge' in medical science: the case of endothelin research". The quotation marks were intended to indicate that those quoted terms do not unproblematically refer to some ontologically independent reality, suggesting that discovery and knowledge are 'socially constructed'\(^{178}\). In terms of 'modality' (Latour and Woolgar 1979, 1986; see also Chapter 1, p. 25), whether a referent appears with or without quotation marks does a lot of rhetorical work on its state of facticity and independent reality (Woolgar 1988a). Thus, the required revision seems to have economically (and seemingly 'mildly') delivered a death blow to such an intention\(^{179}\).

This modification can also be understood as an instance of "translating interests" (Latour 1987: 108-121; see also Chapter 1, p. 27-30). That is, in this case, it translates the research objects originally constructed in Mushakoji's article into ones acceptable in the field of Medical Informatics\(^{180}\). But the whole event can, conversely, be regarded as an instance of "colonizing the mind" (Ashmore, Mulkay and Pinch 1989) of Medical Informatics with social constructionism. (But then, given that I am here "translating

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\(^{178}\) My knowledge-claim on this point is meant, as a social constructionist, to maintain these objects as 'socially constructed', but this knowledge-claim itself is made, as a reflexivist, *within quotation marks*.

\(^{179}\) My acknowledgement to Dr. Katie MacMillan for her comment on this revision at the DARG session (21 February 1996). Her suggestion is that despite taking this event as 'a death blow', I can turn it into an opportunity for taking the next 'reflexive turn' by analysing the constructed nature of this event, as well as, perhaps, exposing the constructed nature of such an analysis itself. That is precisely what I am now attempting.

\(^{180}\) The fate of the title was not finalised at that point. It again underwent further change in and through the process of a professional translator's correction. Such a sequence of modification may merely be a practical matter, at least from those who ontologically commit themselves to the knowledge-claim made via the text, it would have nothing to do with the 'content' of the article. However, for those who take the 'splitting and inversion' model (Latour and Woolgar 1979, 1986; Woolgar 1988a) or 'Reflexiveconstitutedness' (the above second story), these modifications are not simply a matter of practicality through a publication process.
interests" back to SSK in problematising the revision, perhaps the minds of both social constructionism and Medical Informatics are being colonised!

Finally, I have to admit that the Candidate has been having a hard time with her social science colleagues (rows 7-10). Row 7 is taken from a letter commenting on Mushakoji's article. This social scientist a states that the knowledge-claims made in the article are ambiguous, ill-structured and confused; moreover, the author did not make the necessary distinction between her assumptions and conclusion. He questions whether this is caused by "the problem of 'reflexivity' " (row 7). More seriously, a strong distaste for the form of her writing is expressed.

The problems which might be caused by Mushakoji's concern with reflexivity can be examined in a more elaborated fashion with the comments of the social scientists b and d (rows 8 and 10). Their questions are concerned with 'Reflexive-constitutedness': whether the research object in the article is 'text' or 'object'. This is explicitly questioned by social scientist b. On the other hand, social scientist d's interrogation (row 10) is more troublesome, as her concern is also expressed not only in relation to the Reflexive-constitutedness between the analysed scientific texts and the research object, but also in relation to that between the text of Mushakoji's article and what she really meant to say. Social scientist d is inviting the Candidate to enter into the discourse in which texts and their meaning are dichotomised. Precisely because the isomorphic relationship between text and object is one of the essential points in Mushakoji's article, this invitation seems to generate an interesting paradox: for glossing what she meant by "the isomorphic relationship" in the text, is to employ a form which contradicts "the isomorphic relationship" she claims. There have been arguments in SSK about whether one can employ the (realist's) mode of analysis in analysing the (realist's) mode itself (Woolgar 1981b, 1981c; Barnes 1981a). But in this case, the trouble is the reverse: were she to take up the invitation to elucidate her mode of analysis (i.e. an engagement with the isomorphic relationship) the Candidate may need to employ the opposed mode (in which text and object are dichotomised).

Furthermore, there is no guarantee of whether social scientist d would then understand:
perhaps this course of action would only result in a proliferation of "saying in so many words just what they are talking about" (Garfinkel and Sacks 1970).

These issues can be further explored with reference to the comments of another social scientist c (row 9). This social scientist admits that the Mushakoji article at least succeeds in relating scientific texts to 'the process of knowledge construction' in terms of reflexivity. However, he claims that this effort adds nothing to what previously existing models have already stated. As he puts it, the work of introducing 'Reflexive-constitutedness' merely amounts to endorsing "what is treated as pre-given knowledge in just such terms". Writing about the relationship between the texts and 'the process of knowledge construction' is not writing about the process of knowledge construction itself.

Self-referentiality also seems to be problematised by social scientist c. The last half of his comment is a response to the Candidate's request to allow her to include the text of his response into her subsequent research. Social scientist c dismisses this request by saying "if you are really concerned with the possibility of deducing an ethics of researchers' practice as informed by the notion of 'reflexivity'", her PhD thesis is not "fruitful". For social scientist c, her Self-referentiality is merely a reduction of social science study into such an "ethics", and is insignificant for studies of human knowledge.

These criticisms are serious, as what I am doing here in this presentation is subject to precisely similar criticisms. For the sake of my writing, I will try to respond to them, again.

The Candidate's 'Analysing-Back' Again

The comments and criticisms of these participants can be regarded as their 'analysing-back' to the Candidate's analysis. They will help the Candidate to make her research ongoing in its elaboration. Moreover, this elaboration is itself a further 'analysing-back', as a next turn.

Now, I am taking one of those turns in this presentation.\textsuperscript{181}

\textsuperscript{181} And in this thesis (Mushakoji 1999).
With regard to Reflexive-constitutedness, the Candidate seems to have caused some confusion in introducing it. It is interesting that this confusion has occurred *not* in the interactions with endothelin researchers, but in those with social scientists. In two of the latter interactions, the introduction of the "isomorphic" relationship between object and text has resulted in constituting *two entities* -- i.e., 'object' and 'text'. In one case (row 10), the result looks troublesome in terms of an indexicality which here is "required to be repaired for other than practical purposes" (Slack 1996). Therefore, it may well be that her approach to Reflexive-constitutedness has not yet been successfully . . .

Yeah. *But I still claim that such an isomorphism is inherent in and through all the interactions (including those with the endothelin researchers, social scientists and our current dialogue). The situation is maybe better here, with your participation*. After discussing the problematics raised in it, it is possible for us to have an interaction where such an uptake is intelligible. *But what do you think?*

But how can the Candidate's approach to Reflexive-constitutedness be *legitimated* in continuing interactions? The criticism made by social scientist c (row 9) is a *delegitimation* of it; an argument that appears to be related to criticisms made by some ethnomethodologists (Button and Sharrock 1993; Lynch 1993, 1996; Slack 1996) of SSKers' reflexivity (e.g., Latour and Woolgar 1979, 1986; Woolgar 1988; Ashmore 1989). They criticise this Reflexive-constitutedness as problematic because the work of introducing it results in preserving the dichotomy between object and text, rather than problematising it. Indeed, this dichotomisation can be formulated as a two-step procedure: first (*temporally*) engaging in establishing the entities, and then problematising the dichotomy at the next step. This procedure of committing oneself to realism for the sake of criticising realism can be described as 'ontological gerrymandering' (Woolgar and Pawluch 1985).

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182 "and your reading/writing of this thesis".
However, this does not convince me that the Candidate's writings and presentations should be pursued without introducing Reflexive-constitutedness. Accepting that the introduction establishes the ground for recognising ontological entities, the result may be an inconsistency. The move is made, however, in order to problematise the dichotomy and to explicate both text and object as "discursive phenomena" (Edwards 1997: 73). It provides us with a way of discussing object or text as other than ontological entities. If it can have this result in subsequent interactions, we can start treating our research objects epistemologically, with respect to how they are constituted.

As regards Self-referentiality, most of her respondents seem, to some extent, to agree with the Candidate on the self-referential nature of her research. Both she and the participant endothelin researchers seem to assimilate her process of knowledge construction with theirs. However, when it comes to the initiative for declaring and inviting an engagement in Self-referentiality, the issue of legitimacy is the prominent concern for one of the social scientists (row 9). Whether this approach is just an expression of one's concern with "the ethics of researchers" and whether it can (therefore) be a significant contribution to knowledge, remains to be negotiated further.

SSK Self-referentiality has been disputed in SSK, and also by ethnomethodologists (e.g., Lynch 1993, 1996; Slack 1996). I think that the issue is, firstly, whether one recognises the sameness between the topic and the resource of one's research, and, secondly, whether one treats this similarity as a problem or an opportunity. Self-referentiality can be treated as "irrelevant", "problematic" and/or "interesting". Which, perhaps depends upon the kinds of texts (and readers) one is prepared to trust.

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183 I am grateful to Prof. Derek Edwards, for his explication of this issue at a session of the Discourse and Rhetoric Group of Loughborough University (19 July, 1995). His comment in the session helped me to understand the reflexivity between text and object and the related disputes in SSK and ethnomethodology.

184 See the range of stances on this issue in the writings included in Knowledge and Reflexivity (Woolgar ed. 1988b). Latour seems to trust a wide range of texts and readers, who he claims are not naive (Latour 1988). In contrast, most other authors in this volume use more experimental texts with lots of invented devices in order to point out the textuality of their own texts (and, perhaps, to correct the naivety of their readers).
As I stated at the beginning, the three stories I told today are not proceeding so independently as each of them intermingles with other lines. The Candidate has tried to illustrate the knowledge construction in endothelin research proceeding in process. This illustration is based on the knowledge developed in SSK. Namely, SSK has continuously informed the Candidate how to make her research in progress. For example, it has ongoingly offered her a criteria for how to engage in relativism, social constructionism, and reflexivity, informed her of what is relevant and legitimate for her research (such as that to engage in a 'tu quoque' or 'ontological gerrymandering' is negatively valued) and what would be the originality and significance of her research within this research domain. As the Candidate's attempt is to keep up and contribute to the SSK programmes, both the construction of knowledge in SSK and in her research are treated as proceeding in process. Thus, for the Candidate, her illustration of the process of knowledge construction in endothelin research is developing along with the progress of SSK knowledge and her own knowledge, which are in process. But it is not only that SSK and her PhD research are in the process of knowledge construction.

Obviously, the Candidate's knowledge-claims themselves have also relied on how the endothelin research has been developing in process. For instance, the original 1988 article and the endothelin researchers' accounts, both of which were collected at the beginning of her research (in 1994), have informed her how a research object in science is known to exist (Chapter 2): the endothelin researchers' accounts of endothelin which she collected during 1994-1997 have updated her knowledge of how a research object in science is known in process (see Chapter 4). Moreover, their sociologies have

185 In the viva of this thesis, I was asked by one of the examiners how and to what extent my choice of endothelin effects the general argument about the possibility and desirability of the tripling process. To talk about the generality, endothelin research is the science I have happened to encounter and to be acquainted with (see Chapter 1, p. 3), and the endothelin researchers I met make accounts for their activities as a science. The science of endothelin research is of course not the same as those I read in the other SSK case studies and popular science books. How it is to be science is a local matter here. But it is also genuinely a science throughout the Candidate's research (and in general).

Considering this choice's effect on the tripling process, as I states above, by choosing endothelin research, I could find that to introduce a self-reference aspect for the process of knowledge construction is not an extraordinary leap of my self-complacency. As well as the scientist in extract 4P (p. 212-213), the endothelin researchers occasionally ask me the progress of my research and give comments on it by drawing the analogy from how their research is progressing and from their own efforts for contributing to their science. For instance, when I showed my JJMI article to one of the researchers, he questioned why I
informed her analysis of scientific discoveries (Chapter 3), and the endothelin researchers' concern for her research has endorsed her own understanding that it is not irrelevant nor extraordinary to draw the self-reference to her research and SSK. In this way, their activities encourage the Candidate to make the processes of knowledge construction in endothelin, SSK and her own research as a triple process, even though the alignment of these different research domains is a target of criticism (Lynch 1993, 1996). Finally, not only is the Candidate's PhD research about the process of endothelin research and based on SSK knowledge, but also an attempt to contribute to present SSK knowledge as well as to present her understanding of the knowledge construction in endothelin research to the endothelin researchers. Throughout the Candidate's research, I have observed that the processes of knowledge construction in endothelin research, SSK and her own research are in fact proceeding as a triple process. None of them are merely a construction in one of these three research areas: all of them are interlinked and ongoingly proceeding in process.

For this process further to proceed, I will keep on opening the Candidate's research to the processes of knowledge construction in endothelin research and SSK. That is, the Candidate's research has a research object and background not only constructed by herself but updated by those in endothelin research and SSK along the progress of her analysis: her analysis resonates with the progress of those research areas, and develops itself with the responses raised from the endothelin researchers and SSKers.

As I am doing right now, I will keep on 'analysing-back' to any responses to the Candidate's research, and thus perpetuate dialogues. The Candidate's research of the processes of knowledge construction is still becoming in the continuous responses, reconfigurations, etc. made by the endothelin researchers, social scientists or indeed any of us who participate in this session and who read this thesis. According to some of the

did not write it in English so that more readers will be informed how endothelin research is developing in process. This is contrary to Lynch's ethnomethodological field study of work in science, in which he found that the scientists' shop work and shop talk are disengaged from the work of sociologists (Lynch 1982, 1985, 1993).
responses she has received, the Candidate's approach is subject to criticism and its legitimacy remains to be negotiated. But it is these criticisms which will make successive 'analysing-backs' possible, and it is precisely this mechanism that will create her research as a process of knowledge construction.

As you may have recognised, you are participating in the continuation of this dialogic interaction. Even if you disagree with my approach, this too is a feature of our mutual process of knowledge construction. For the sake of this story, though, I hope that we are dialogic in our mutual knowledge construction; that we are wrighting together.

And now I am (offering you an opportunity which will then become integrated into the ongoing research))

5A 548. Sumiko Whichever uptake is attributed as the relevant formulation of our participation of this presentation, I would like to ask you for letting me include that into my successive research on the processes of knowledge construction. (holding the microphone of the tape recorder which is set to record the session))

5A 550. Audience (laughs)

5A 551. Sumiko Thank you for engaging in our talk 186.

5A 552. Audience (clapping)

(Mushakoji, 12 October, 1996 187)

5-2. I Will in Turn Write Her Stories

Let me now follow up on how her stories 188 continue.

186 "... and thank you for your reading/writing".
187 This and subsequent excerpts are taken from the transcript of a recording of the session, 'Reflexivity II: Infra, Meta, Supra, or Not Such a Good Idea After All?', at the Joint Conference of the European Association for the Study of Science and Technology (EASST) and the Society for Social Studies of Science (4S) held in Bielefeld, Germany, in October 1996. (Mushakoji 1996).
188 I am attempting to tell stories, and not meta-stories nor meta-meta-stories. Nowadays, the reflexivists seem to have reached the agreement that their stories, no matter if they are about reflexivity or about something else, if told in a reflexive way, are not 'meta-stories' but simply stories (Woolgar 1988b; Ashmore 1989, 1996; MacMillan 1996).
After the stories were presented, a question and answer period was provided by the organiser of the session. One speaker from the audience asked a question. I will provide the transcript of the recording of this interaction. It starts from right after the presentation:

58. Speaker (indistinguishable) If you are de- eh- this is I am [uh-] (1.0) They might keep silence a little more. 59. Speaker [(laughs)] Yeah, there he is. 60. Speaker Eh- it seems to me that you eh- want to write your story yourself. I mean, you- there- you might be thinking the talks of reflection, I mean, we think the talks of- eh- Descartes sometimes- piece of reflection? 61. Speaker (murmurs) de cort- sorry? 62. Speaker the piece of reflection. 63. Speaker Contie- 64. Speaker No, Descartes, the- the- the philosopher of: [(indistinguishable)] 65. Speaker [Descartes.] 66. Speaker (indistinguishable) Yeah. 67. Speaker Because he- he is the one explored the former (indistinguishable) 68. Speaker reflection- [of] he reflected upon (indistinguishable) 69. Speaker [Mm mm.] 70. Speaker [[this own]] presupposition of thinking.- 71. Speaker [(Mm mm.)] 72. Speaker Yes. 73. Speaker about (indistinguishable): "solid"? knowledge base. And now you choose to 74. Speaker close the story with the third person, about 'the Candidate'.- 75. Speaker Mm hm. 76. Speaker I mean, you seem that- I mean you yourself was participating in your data. 77. Speaker You could have done it with the first person. 78. Speaker Yes. 79. Speaker 80. Speaker (Mushakoji, 12 October, 1996)

The speaker's question was — it seems to me — about the use of the third person in the stories presented by Sumiko (line 560). In the stories, the one who pursues the PhD

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89 My acknowledgement to Dr. Dick Pels, who provided me with the chance to present the Candidate's research at the Bielefeld session and in this writing. I also appreciate his reflexive participation (particularly, in lines 557 and 597) in the transcribed talk below.

90 My acknowledgement to this anonymous speaker for raising a question, initiating our dialogue, and offering me a further chance to respond to his question here.

91 Sumiko and the Speaker in this extract are both non-native English speakers. In addition, the transcriber (that is, myself) is non-native. Now, how do you, whether you are a native or non-native English speaker, judge this conversation in terms of 'proper' English talk? Is it awfully ungrammatical? If so, how do you assess the speakers' competence in English? Alternatively, how do you assess the transcriber's competence in English? For whom would it be ungrammatical? This issue will be further elaborated in Section 5-3.

92 This is the end of her stories.
research is called "the Candidate" and is referred to with the third person pronoun "her" (lines 576-577), while "Sumiko" (in the above transcription) refers to herself with the first person "I". That is, "the Candidate" in the stories is distinguished from "Sumiko".

The speaker initiated the question by referring to Descartes' work on "reflection" (line 562) with which Sumiko seemed not to be familiar. Then, Sumiko's distinction between the Candidate and herself was problematised (lines 579-580). Now, let us further examine why this speaker claims this distinction to be inappropriate:

The speaker's question raises the problematics of the way Sumiko situates the Candidate as another person. The stories are told as if 'what really happened' to the heroine is related by a different person. This conceals the fact that the story-teller is telling her own stories and thus that the stories are subjective. For the speaker, such a textual device contradicts Sumiko's approach, which she claims to be open to critical reflection "from other persons" (line 582).

In answering the speaker's question, it seems that Sumiko is at first confirming (through negotiation) how to formulate the question itself, rather than directly responding to what he says:
Comment. So, how- I mean- I see your problem that you seem to define yourself as the authority of truth in your story.

Mm hm. Yeah. And I think that (indistinguishable) part of others' perspectives? I think I need it. I have to find the way. And, to find that the- my own story is authorised by myself and I cannot help being authorised by myself. But still, uh- that authorise is simultaneously- that- the- the form of this authorisation is simultaneously open to the reconstruction, or re-constit- reconstitution of the story of (1.0) others. But, is it possible or not? If- I still don't know the way-um- is it (sighs) but I, for me, that I find that if you comment, that is the- if I open to the- your comments, and to respond to it, it's- it's the way that I try to open my autho- the form of my authorisation.

(Mushakoji, 12 October, 1996)

Sumiko's question (line 592) seems to be an attempt to confirm that the speaker is himself doing "the critical reflection" (line 582). Lines 595-6 and 598-9 can be seen as a further attempt to reinstate the audience in the session as an in situ critical reflection to the Candidate's research. This is not only said but displayed with Sumiko's action of holding the microphone towards the audience to show that the recording of the session is taking place in order to be included in the Candidate's subsequent research (lines 595-596; see also Section 5-1, p. 254). By distinguishing his comment from critical reflection (line 600), however, the speaker's reply indicates his reluctance to participate in Sumiko's invitation, while the audience's laughter indicates that the in situ Reflexive-constitutedness is attended to at this point (lines 593 and 601).

Sumiko then seems to have accepted the speaker's reformulation (lines 602, 605), and starts trying to re-state her approach of being reflexive to the others' responses (lines 607-614). In line 608, the reference to "myself" can be interpreted as either referring to the Candidate or Sumiko-as-speaker. It is not clear whether the implication is that the heroine of the stories, i.e., the Candidate, cannot help being authorised by the story-teller Sumiko; or that it is not only the participant in the stories, but also the "I" who tells the stories, who cannot escape from being authorised by the one who is constituting this "I" as the self193.

In this way, at the debut of her research on the STS stage, the Candidate's analysis has been analysed-back by this speaker: he questions the multiple subjectivity

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193 Which of these is what Sumiko meant? I can of course take an 'authoritative' advantage in defining what she meant in this line, but I won't, here.
employed in her research which he claims evades both the critical reflection of others
and the author's responsibility to define "the authority of truth" (line 603). There are
some issues relating to his claim that I wish to deal with in this 'analysing-back'.

First, in these extracts Sumiko may be attending to the otherness of the self.
That is, on the one hand, the story-teller, the author of this writing/reading, Sumiko, the
Candidate, etc. may be identified as the unified Author in a particular setting, or in a
particular reading/writing. But on the other hand, any settings or readings/writings are
themselves in a process in which a certain object in ongoingly and potentially becoming
(see Chapters 1 and 4). Thus, the one who may be unified under the same Author can
also be seen as differentiated.

Furthermore, I think that the perception of one's self needs to be perceived
outside of what constitutes 'I'. As Woolgar states, "the self as a concept seemed to be
disengaged from the Self in the act of representation" (Woolgar 1995). The work of
attribution, which functions to evoke the ontological 'I', cannot itself remain in stasis
(Holquist 1990). Thus, in telling one's own story, one is persistently faced with the 'non
self-sufficiency' of the self (Morson and Emerson 1990: 50). The recognition of the
sameness of the Author can itself be understood as constructed in situ. What counts as
a self is achieved in an interactive process wherein the unifying of differentiated selves
for the sake of sameness ongoingly takes place. In the above extracts, the Candidate is
thus established as being identical with Sumiko in the interaction of this session. When
any of those identified as the same (Author) are set in an interactive series of texts, she
is -- and even I am -- talking to, and of, an otherness.

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194 Descartes, who is referred to by the speaker (line 566) also expresses the uncertainty of constituting
Self:

One must then, in conclusion, take as assured that the proposition: I am, I exist, is necessarily
ture, every time I express it or conceive of it in my mind.

But I, who am certain that I am, do not yet know clearly enough what I am; so that
henceforth I must take great care not imprudently to take some other object for myself, and thus
avoid going astray in this knowledge which I maintain to be more certain and evident than all I
have had hitherto.

(Descartes 1968: 103)
Secondly, Sumiko is charged with evading the critical reflection of others. But this issue could be viewed from another angle. In the above transcript, it can be observed that the heroine in the story (she) and the story-teller (I) are identified as the same person in the participants' (including the audience's) interaction. In other words, the sameness of person(-s) and the consequent problematics of making this same person multiple are simultaneously constituted on the spot in interaction.

The unification of differentiated persons is inherent in any text. What is collected under 'the same person' is locally constituted on and for each occasion. Unification also occurs in this reading/writing: it is not only that "Sumiko" is identified as the same person as the Candidate in the talk, but also that in your reading, you are participating in the constitution of Self. In this sense, it is in your/my reading/writing, where the identification of multiple persons as a self, and the otherness of this self is made available.

Thirdly, to make a distinction between "I" and "she" may risk complicating your reading (Pinch and Pinch 1988; see also Chapter 1, p. 52-55). But I think that the extent to which that is experienced as an irritation depends on whether such a distinction breaches the conventional unification of self. The employment of multiple subjectivity informs you, among other things, of how conventional your reading of stories presented in an STS setting is.

Multiple subjectivity has a further implication. When the pronoun "I" is used in the article, it projects the conventional agreement that the stories are presented by "me", to "you", the reader. Whether and how such an agreement is made relates to the way

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195 The talk, as represented in the transcript, already pre-defines the participants' identity.
196 The employment of multiple subjectivity might contradict the order of what Schegloff terms 'procedural consequentiality', which is explained as followed:

Even if one can show that, of the descriptions of the settings and persons which could be invoked, some particular ones are relevant to the participants in the interaction, it remains to be shown that they are procedurally consequential for the particular aspect of the talk or other conduct which is the focus of analysis — that is, that there is a consequential tie (again, for the participants) between the setting and interactional identities so understood and a particular facet of their conduct. (Schegloff 1992, emphasis original)
the interaction between the author and reader is constituted, and how the stories would then be read/written. I hope that you can participate in an interactive dialogue with me, and analyse-back to my analysis in and through your reading/writing of the stories.

Finally, I claim that I am not, in this text, merely stating the possibility of such a dialogical openness of text. I am analysing-back this criticism of multiple subjectivity in telling "her" stories (lines 603-4). The use of the third person pronoun is an attempt to illuminate the textuality of the stories where -- if successful -- I was telling how she interacts with others regarding the process of knowledge construction, while I am, in telling these stories, unfolding this very occasion of knowledge construction for others and myself. I am inviting you to perform the construction.

In the above transcript, this attempt seems not to have been successful in the sense that the Speaker, by stating that his question was not a "critical reflection" (lines 598-600) did not take up this invitation. Furthermore, this refusal to participate in "critical reflection" implicates the Speaker in a further refusal to participate in constituting "the truth" of the stories (lines 603-4) and in accepting any responsibility for them. The upshot is to strengthen his criticism that Sumiko's authority is illegitimate. However, as the above three points suggest, I believe that "there is no such thing as an entirely individually author-ised text" (Ashmore 1989: 216). Regarding the issues raised in this section, the problematics of the authorisation of subjects, knowledge and events, clearly need to be addressed, relativised and explicated.

I am not denying -- and neither is "Sumiko" in the above extracts -- that the authority and responsibility for stories relates to who tells them. The work of addressing, relativising and explicating questions of authorisation by no means denies its own positive participation in such questions (Edwards, Ashmore and Potter 1995). I pursue it by accepting them all: the self who is a PhD research participant, the self who write/reads a thesis, and the self who constitutes this multiple subjectivity "in the act of representation" (Woolgar 1995). The Candidate's research thus is open to the critical reflections of others, including my own critical reflection. However, when Authority
asserts its right to define 'what really took place' and 'what "she" really meant', and to enforce the ownership of knowledge (Sharrock 1974; Horton-Salway 1998), I am at liberty to question such Authority; even if not by seeking a less authorised textual form for telling my stories (MacMillan 1996).

By the way, who is writing these issues in this section right now?
5-3. (Footnotes) 'Analysis Being Analysed Back'¹⁹⁷

SE
1. (noises in the DARG room)¹⁹⁸
2. Sumiko I'm going to tape,
4. Sumiko for the source.
6. Sumiko Um-
7. Ann All right.
8. (2.0)
9. A Member (indistinguishable)
10. Sumiko (indistinguishable) will be a source again.
11. Ann (indistinguishable) persons to do that here again. (laughs)
12. Members (laugh)
13. (1.0)
15. Sumiko Uh- Forgive me to read my manuscript.¹⁹⁹,²⁰⁰

(DARG 21 February, 1996)

¹⁹⁷ This section will take the textual form of 'data' (transcription of the recorded session of the Discourse and Rhetoric Group [henceforth, DARG], Loughborough University) together with footnotes. The relevance of this form will be clearer as the transcribed discussion goes along (around line 25 in extract 5G and more clearly in line 104 in 5K).

¹⁹⁸ The tape recorder was positioned in the room where DARG meetings take place. My acknowledgements to all the DARG members who participated in the session held on 21 February 1996, at which Sumiko presented 'Analysis Being Analysed Back'. My special appreciation to Derek (Prof. Edwards) for his incisive 'analysing-back' of her presentation. It has worked "therapeutically" (MacMillan 1996).

¹⁹⁹ This is the very beginning of the recording of Sumiko's presentation at one of the weekly DARG meetings. All the participants in this and the following extracts were members of this group.

²⁰⁰ The above three ethnographic (foot) notes, as well as those which will follow, need special attention. To claim that they are my own construction of 'what actually took place' will not be surprising for you. But this construction has the feature of a "nested structure" (Hofstadter 1979; Ashmore 1989: 245). The construction of 'what actually took place' bolstered by the footnotes will become the topic of another occasion, in which the construction of 'what actually took place' bolstered by footnotes like those in this section will again become . . . All the constructions are thus displayed as ceaselessly connected in process and ready for "the reflexive next turn" (MacMillan 1996: 41-42); as you will see.
Sumiko: As most of you are familiar, I have started my research with the investigation of the process of knowledge construction in endothelin research. Initially, this investigation was pursued by examining how knowledge-claims are written and re-written in a variety of different scientific literature, such as research articles, review articles, handbooks, textbooks and dictionaries. Then, in 1995, I wrote the research outcome as an article for *Japan Journal of Medical Informatics*, which, from now on, I will refer to as 'JJMI article'.

My interest, however, has gone further beyond that point. As my analysis proceeds, I have started to observe that the intertextuality of these scientific texts plays a key role in recognising the process of knowledge construction. This recognition of intertextuality does not exclude the texts which I am producing. Furthermore, the texts I produce relate to the texts on which my research has been based and the texts that comment on and criticise my research. Besides, my approach is such that I try to proceed by ongoingly interacting with the participant scientists and social scientists in dialogical correspondences. I have asked them to respond to my analysis, and their reactions are taken as 'analysing-backs' to my analysis. These 'analysing-backs' will constitute the bases of my successive research, in which they will be further analysed. In short, I regard my research object -- that is, the process of knowledge construction -- as constructed through the collaborations with scientists and social scientists, and recognised in the intertextuality of all those related texts. I am hoping that this approach works for "hybridisation" (Latour 1988) of science, SSK and my research.

Of course, in such an approach, there have emerged some problems. One regards a variety of different localities where a text is made relevant and intelligible. I have to report the research outcome in, so to speak, different contexts. First of all, I obviously have a

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201 For readability, I have reproduced the draft that was prepared for reading-out, rather than transcribing the recording of her speech. What I can hear in the recording is of course different from this reproduction.
language problem, as a non-native English writer who writes her PhD research in English. This problem also raises a translation problem, of writing in Japanese and writing 'the translated version' in English, and writing in English and writing 'the translated version' in Japanese. This problem exposes a wide range of discursive manipulations in constructing my own data, making accounts in ethnographic notes and writing an intelligible thesis. In addition, I have an interdisciplinary problem, as my background was the field of Library and Information Science. In this field, I have been trying to explicate some ideas regarding SSK, DA, and reflexivity to my colleagues. This interdisciplinary problem is more acute when I study endothelin research with my expertise. I am studying biochemistry, physiology, pharmacology and genetic engineering as a candidate sociologist. In an ongoing dialogue with scientists, I am struggling to make relevant inquiries and to respond to them in an intelligible manner. In shifting from on "being there" to on "being here" (Geertz 1988), in this DARG room, I am struggling to pursue an empirical kind of DA while being aware of its own reflexive implications in the SSK framework.

Another problem relates to the extent to which my research can be reflexive. It appears that the intertextuality of the texts I produce is a crucial concern here. This is neither a subsidiary nor a 'meta' issue for the study of process of knowledge construction. It is precisely within the study of process of knowledge construction, given that I do not make my own case an exception. If I can espouse the Purity of distinction between my research object and resource, I may be able to claim that my concern is only on the former. But the texts I produce out of this study have no way to avoid interrelating with all the other texts from which my research object is constructed and within which my knowledge-claims are recognised.

But on the other hand, I did not address the JJMI article for the sake of experimenting the intertextuality of the text of this article. Of course, while writing this article, I had a plan of examining how the text of this article gets its intertextuality. But this article was written as the flesh and blood of the research on the process of knowledge construction in endothelin research. So, whenever I got a warm encouragement or cruel criticism, it strongly
affected my emotion. It has not always been easy to keep myself open and ready for being 'analysed-back'.

The last problem relevant to mention for this session is the one of being in a process. I am trying to "unfinalise" (Morson and Emerson 1990; Bakhtin 1981) the research. In a sense, no texts are finalised, as they are waiting for recognition in different localities. The handout shows how the text of the JJMI article has fostered a variety of readings/writings. By 'analysing-back' to all these readings/writings, now I am attempting to develop my research. I am hoping that with this attempt, my research can itself become a process of knowledge construction. For this purpose, I ask you to participating in my research, by 'analysing-back' to it.

5G
16. Derek If we are not so much analysing their texts as analysing yours, what we've got here is the way you have assembled these quotations, such that the implications of what they say are constructed up by you for us, and we are looking at this piece of paper.
17. Sumiko Mm.
18. Derek So what we've got here is in effect these comments, uh- which bolster for us your credibility as doing this research, you know.
19. Sumiko [who-] uh- (coughs) by- and if we start analysing your text, then some of these stuffs are your stuff, like, for instance, the footnotes.
20. Sumiko Mm.
21. Derek Like Footnote 2, for instance, uh- you provide a couple of accounts there in Footnote 2 which help us understand something, you know.
22. Sumiko Mm.

202 The handout circulated in this session was an extended and messier version of Table 5-7, p. 243-245.
203 The first 27 minutes of discussion is omitted. The members commented on the quoted responses to the JJMI article in the handout (see footnote 199). For instance, what the endothelin researcher meant by "logic" (row 3, p. 243; see also p. 246). The discussion then shifted to how to read her data in the handout -- that is, how the presented data can be interpreted in a Japanese context, and then . . .
204 "Footnote 2" (line 30) is reproduced as footnote 176.
34. Derek There are two accounts that are there. One is an account for why
you and Prof. Kimura write to each other in English,
35. Sumiko Mm.
36. Derek And the other is uh- the account for leaving it in English in this
document that's a kind of different thing. And it's own full of
why- what is accountable here, you know. What are you doing in
that account,
37. Sumiko Yeah.
38. Derek in that, say, if it had been in translation, presumably we would
have been, would we, that you- you haven't quite got it right.
39. You know, that the translation might be a little while-
[no sense of-205] So one thing which- you are managing the
40. Sumiko [No.]
41. Derek credibility of your text by that footnoted notion, "Well, this
is the original language, so nothing is changed."
42. Sumiko Mm.
43. Derek Then, the other thing is that there is an account- there is to why
you and Prof. Kimura write in English,
44. Sumiko Mm.
45. Derek which I find rather puzzling, in that you can write Japanese in
alphabetical speech. You've got some Japanese written that way
so why not write Japanese in English script?206 It can be done.
46. Sumiko Here, in DARG?

205 Sumiko has problems not only in using the translated data for DA, but also in competently making
transcriptions (see the above extracts, see also the following footnote 222), and presenting these problems
in English to DARG members and the readers.

206 My present response to this question is different from the one of "Sumiko" (in the above transcript).
After observing some similar e-mail exchanges between Japanese speakers, I have found that writing
Japanese in alphabetical letters is too cumbersome. When phonetically written in alphabetical letters, the
Japanese language, which widely uses Chinese symbolic letters, becomes much harder to read fluently
even for Japanese. Another 'reason' for writing "footnote 2" (line 30) is to account for the texts of
correspondence which may be, for native English speakers, grammatically incorrect.

207 If I can take some Authority (see Section 5-2) over what Sumiko said in this line, her question displays
a hesitance of giving the reason: Derek's request (lines 57-58) invites her to construct what is 'the real
cause' for this matter. Such a causal account imposed by the analysts' concerns has regularly been a 'bone
of contention' in the DARG meetings. The laughter that follow her hesitance (line 63) is hearable as
attending to the irony of Derek's comments and Sumiko's uptake.
68. Derek Yeah. Obviously *(indistinguishable)* [You'll be held ]

69. Member

70. Derek accountable here for [[all the tape.] *(laughs)*]

71. Members [[*(laughs)*]]

72. Sumiko [[Mm mm.]]

73. Sumiko Mm.

74. (2.0)

51

75. Humphrey Oh, see. So, perhaps, their English.*208*

76. Katie *(giggles)*

77. Humphrey Now, here I have a Japanese friend. I know very precisely what

78. Katie *(giggles)*

79. Humphrey he says by his *(indistinguishable: team)* his English is- hard time

80. than anyone.

81. Katie Yeah.

82. Ann No, that's what *(indistinguishable)* she can use what she wants.

83. [(laughs)]

84. Humphrey [Is it?]

5J

94. Malcolm Who wrote first in *(indistinguishable)* he might be in

95. correspondence, so- so, you and Prof. Kimura.

96. Sumiko Yeah. I think it's Prof. Kimura.

97. Malcolm He- he initiated it.

98. Sumiko Yes.

99. Malcolm. Right. So, that's- and that was in English.

101. Sumiko (0.5) Right. [Right.]

102. Malcolm [Right.] So, it's his fault.*209*. [[So, it's his fault.]]

103. Members [[*(laughs)*]]

5K

104. Derek I can take these things up [and hopefully] you can make these

105. Malcolm [[*(laughs)*]]

106. Members [[*(laughs)*]]

107. Derek things up*210*,

108. Malcolm Yeah.

109. Derek I must-

110. Malcolm [[*(laughs)*]][[(laughs)]]

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208 In contrast with the above footnote, this speaker's move attends to a different formulation of Sumiko's hesitation in response to Derek's question.

209 This is another formulation of Sumiko's hesitation. It works to rescue her from the position of being required to give a reason.

210 This utterance can be read as a reiteration of what was said by the same speaker in extracts 5G and 5H. But I hear the attention to the *in situ* Reflexive-constitutiveness (5-1, p. 233-234) as being stronger than before.
Indeed, even though other members of DARG have had their turns (see footnote 222), one of the apparent differences of my voice from theirs is that mine can appear in these footnotes as well as in the transcript, while the others' can only appear in the transcripts or in my quotations. But can I really assume that Sumiko's voice is the same as mine? (See Section 5-2, p. 254-261). Moreover, how far can all these participants' voices be perceived as real after the textual processing in my data construction? See also footnote 222.

212 The present end is in your reading/writing.
160. Derek So that's a dealing. It's not that something I'm after maybe you
161. know, the true reasons. I'm after the textual functioning of it.
162. Sumiko Mm.
163. Derek I can see that it is- it attends to a possible reader's objection,
164. which is "I don't trust the text. It's translated. It might be fiddled
165. or wrong or self-serving."
166. Sumiko Mm.
167. Derek you might have actually if you translated it, made it sound rather
168. different than it is original or whatever,
169. Sumiko Mm hm.
170. Derek And- and having this footnote tells us "no, that's not the case. It
171. was originally in English."
172. Sumiko Mm.
173. Derek and that stops us thinking all those things and let's just believe it
174. that this is in fact what Prof. Kimura said.

5K
175. Sumiko Right. Then, what makes you uh- (0.5) what makes you feel-
176. (0.5) what makes you say that you- this is workable and this
177. doesn't as a handout for the DARG presentation in- I mean, you
178. can perhaps dismiss that it is not-
179. Derek Oh, no, I don't I-
180. Sumiko The work worthwhile to make- to-
181. Derek No, I came the opposite. Um- Taking seriously your invitation,
182. to analyse our text, rather than to look through your text and
183. analyse the things they say
184. Sumiko Mm.
185. Derek which is all we were doing that far, I'm kind of analysing your
186. text, which I think you are inviting us to do. It's another reflexive
187. step,
188. Sumiko Mm.
189. Derek So, what we've got here is- everything here is your text,
190. Sumiko Mm.

213 A further implication of this claim is that the readers of this section have been continuously working
on a dealing; or alternatively, that there is a problem with the transcription!
214 What can be heard in lines 175-178 constitutes Sumiko's uptake of Derek's inquiry about her footnote.
It can be interpreted that she has formulated the inquiry as threatening the genuineness of her footnote
accounts. The rest of the participants successively construct her reaction as accepting that her footnote
accounts are undermined (lines 200 and 208).
215 Instead of taking Derek's request as dismissing her work, Sumiko could have recognised it as an
opportunity for taking a next turn. Presumably, due to the severity of her problems (p. 263-264), she has
been unable to take the reflexivist's stance of "celebrating" (Mulkay 1985; Ashmore 1989) her own
constructive work in the footnotes. In place of her, however, I am attempting to take what MacMillan
calls the "spiral of reflexive turns" (MacMillan 1996: 25-27; see footnote 14) here in these footnotes.
191. Derek everything they say is your text, all of your footnotes are your
text,
193. Sumiko Yes.
194. Derek the whole selection is your text,
195. Sumiko Yes.
196. Derek and this is the object we are analysing. What we're attended to is
we go through it and say "I wonder what Prof. Kimura was doing
when he said that," I'm on the one that Sumiko Mushakoji is
doing when she gives us to read, which I take it to be alternative.
200. I'm not undermineing it. I am trying to make it precisely what's
appropriate to give to DARG. Not the opposite.
202. Sumiko (1.0) Why? Uh 216
203. Derek and Members (laugh) 217
204. Katie ((To Derek)) You are the gut.
205. Derek and Members (laugh)
206. Malcolm Oh, great. Isn't that interesting 218 that- that it's-
207. Katie Yeah.
208. Malcolm so easily seen that is undermined?

5L
210. Ann On page six, footnote six 219 , you've got about that you are not
211. quite sure what Prof. Kimura means by- when-
212. Sumiko Mm 220 . (dimly)
213. Ann you are saying to us, "I'm a careful person. I'm not going to jump
to conclusions about what he mean, and um- in a way he-
because of this language difficulty, I'm not-
216. Sumiko Mm. Yeah. (fleebly)
217. Ann And you- you let us see that it's um- Japanese that was given.
218. (0.5) This is uh- it's not- it's not clear that in my reading you are
not jumped about the conclusions about what things mean,
220. Sumiko Mm.
221. Ann You know, eh- eh- in your presentation of his language to you-
222. Sumiko Mm.

216 Her "why" suggests how difficult it is to take a next turn — the next 'analysing-back'. Moreover, this analysing-back needs to go beyond a tu quoque (Ashmore 1989: 87-111) — that is, it needs to uncover the very grounds of itself, rather than merely attacking the other analyst's ground. Note how Derek has done it in lines 120 and 122-123, extract 5K. This task is nevertheless attempted here by attending to the textual form of data and footnotes, and by displaying what is recursively constructed in them.

217 This laughter and the following one (line 205) indicate that the DARG members other than Sumiko are getting the turn suggested by Derek and recognising Sumiko's difficulty.

218 This comment formulates an "interesting"-ness (Davis 1971; Ashmore 1989) in the difficulty of taking a reflexive next turn when it comes to one's own ground.

219 This speaker proceeds to point out Sumiko's constructive work in the footnotes.

220 In the recording, the voice of this speaker becomes feeble from this line on.
223. Ann which- in which you are careful in um- and that's- that to me, the
224. same, right?
225. Is that I thought with your footnote, that you have been more-
226. Sumiko Mm.
227. Ann that you are doing a bit of self-representation here.
228. Sumiko That's right. Yeah. ((feebly)) (2.0)221

5M
229. Katie With Derek here is the voice that it becomes, it won't get the
230. analysis222. He presents the text and let us be on reflexive
231. practice, and suddenly your name is up on the light. He
232. analysed it.223
233. Sumiko Mm. (1.0) Mm.

5N
242. Katie (giggles) [Asking for another account] until that Sumiko
243. Kevin [Well, ]
244. Katie (indistinguishable) get?
245. Kevin Yes. (laughs) I don't know. (0.5) Yeah. I mean that's what is
246. within you are doing. Even then we just keep-
247. ..... 224

(DARG 21 February, 1996)

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221 The voice on the tape sounds as if this speaker is not really responding to further analytical remarks on
the constructive features in her footnotes.

222 In the recording, I can hear this remark as a kind of rescue, like a fairy godmother, with a soft releasing
tone for making a reflexive turn available. The following is Dr. Katie MacMillan's further remark on this
footnote:

[I am] not sure I've ever been described as a fairy godmother, either together or separately, but
there is always a first time, but how about a fairy godmother/devil's advocate instead. . . . [Y]ou
will deny that [I have rescued you] — you, of course, make the rescue.

(MacMillan 1998, personal communication)

223 See footnote 226.

224 The discussion continues.
Twenty months later (28 October, 1998), the DARG members had a discussion about the transcript reproduced above in extracts 5G-5N. On this later occasion (see extracts 5G-5T), the method of reproducing 'what actually happened' in the transcript was at issue: Sumiko's topic was the making of 'good enough' transcriptions and the construction of 'non native-ness'.

The transcript of the earlier occasion was distributed and the members listened to the recording. Sumiko told them that the transcription has been made by a non-native English transcriber (that is, herself). She asked them whether and how it is not 'good-enough' at delivering what was said. (Most but not all of the members are native English speakers.) Some of her questions were:

1. What kind of practice are they engaging with, in listening to the recording and correcting the transcription? Is their practice of transcribing it different from hers in terms of 'competence'? (Sumiko refers to the ethnomethodological notion of competence [e.g., Lynch, Livingston and Garfinkel 1983: 207-8])

2. Is this a 'technical matter'? (She additionally informs them that as a Japanese, it is hard for her to hear the difference between the consonant(s) 'l' and 'r', British jargon, colloquial utterances, etc., as well as to 'guess' what is heard as ambiguous in ill-recorded speech.)

3. How do they transcribe the talk of natives and that of non-natives? For example, would they discriminate Sumiko's (possibly) grammatically ill-formulated utterances from a hard-to-hear utterance spoken by a native member?

4. In terms of the question of Authority (Section 5-2), if they modify the transcription and if Sumiko claims that she actually hears the talk in the way represented in the transcript, how do they defend against her claim?

5. Is 'competence' a matter of 'shared cultural knowledge' among those categorised as native? Or, is it an in situ practice of agreeing with one another regarding a proper transcription? Is it owned? Is it displayed? Is it practised in participation?

The discussion was, of course, recorded. Some of the relevant comments made at the session are reproduced above (by transcribing them) . . .

In this discussion, Prof. Jonathan Potter first suggested — "in order to have an argument" — the incompleteness of the transcription: the 'correct' version for the sentence in lines 231-232 (5M) is "and suddenly you are laying yourself on the line to be analysed".
by that version now, than your previous one?  
Well, to be honest, I'm very much persuaded and I want to write it down later.  
(Malcolm) after this session. Uh- and [I-]  
(Sumiko) (laugh) because it caused you- the change of view?  
(Sumiko) Yeah.  
(Malcolm) That would make a difference. I mean, in other words, you would expect to hear [what is] said on the tape.  
(Sumiko) [Yeah, yeah.]  
(Sumiko) Sort of ([in order for]) confirming what he has written  
(Malcolm) (indistinguishable) right or not.  
(Sumiko) So you think you need that version first, to hear what the tape said. (0.5) Normally, that's done all the time  
(Sumiko) ([indistinguishable])  
(Sumiko) [In this case,] it might be like that- (1.0) with kind of wonder that how- why am I not sort of that- taking the stand, and then still insist my own version.  
(Malcolm) Yes. (0.5) But- but you are not.  
(Sumiko) But the- (1.0) I don't know. That's what I want to discuss, because I-  
(Members) (laugh) [(laugh)]  
(Sumiko) [at least, at least,] I have to make my data, in order to write a thesis. And if the data is not proper, properly acceptable, by the- (1.0)

Some student members claimed that if Sumiko listens over and over again, and keeps modifying the transcription according to what she hears from the tape, the transcription will become more precise. One of them asked whether she had any experience of making transcriptions before the transcription (5E, 5G-SN) was made. Thus, with respect to the second question in footnote 22S, they seem to see her problem as a technical matter.

This question attends to the fourth question in footnote 22S. Namely, this speaker is asking whether "the Potter corrected version" is now Authoritative and whether Sumiko wants to defend her version.

This speaker points out the difference between (1) hearing the tape again to examine whether an alternative version is the more likely to be correct, and (2) writing down the modified version first and then confirming it through listening to the tape. As Sumiko seems to have chosen the latter case, it turns out that here she is subject to the Authority of native speakers, even though she nevertheless problematises such subjection (lines 320-329).
transcription hears correctly what the native speakers say?229

Jonathan No. (1.0) I- [I- I] hear your line 233, whether it was transcribed
Sumiko [So, (indistinguishable)]

((the sound of turning over the page))

Jonathan correctly or not, um- it seems to me to be a non-committal
"mm".230
Sumiko Mm.231

Possibly, precisely the kind of "mm" that would appear as a
rather confused ch- (1.0)
Sumiko So, she didn't hear what the participant Katie said.
Jonathan Well, I- I do not want to go that- that far. And I am still- I've still
got to listen to- I have to- in order to transcribe, some of bits,
if Sumiko, for example, is treating what Katie said as "suddenly
your name is up on the light. He analysed it", what we are
doing?232

Members (giggle)

Derek We don't know what Sumiko is responding to it, that it wasn't
this. Sumiko in line 233 isn't responding to this transcript nor to
that tape. But (indistinguishable) the time, responding to Katie
talking. And Katie's talking is taken, ((indistinguishable)]

A Member recorded transcript, and like a mundane transcript, it has been
rather clearer, you know. You might be sitting next to her when
the microphone was the other side of the room. So, when I have
the trouble hearing Katie's voice in the tape, but Sumiko on the
tape might have heard it very clearly233. And you can make
[a study of it] or do anything.

A Member [(coughs)]

Derek So, uh- Sumiko on the tape is a different entity. We've got three
Sumikos- we've got four-

Sumiko Mm hm. [(laughs)]

Members [(laugh)]

Derek Sumiko in the transcript, Sumiko on the tape, Sumiko in the

229 This speaker's question attends to the third question in footnote 225.
230 Replying to the question, this speaker's formulation is that the "mm" of 'Sumiko in the transcription' is
non-committal. This is grounded in his mastery of English.
231 Responding to Jonathan (see also footnote 230), Sumiko again says "mm".
232 This speaker readdresses the third question in footnote 225. Here, he problematises how to "listen to",
"transcribe", and analyse the speech of a non-native.
233 This speaker explicates how the DARG members are to orient towards the talk which took place in a
previous DARG session (26 February, 1996), the talk hearable from the recording, and the talk
transcribed by natives and non-natives. He tries to sort out what the members can do with the tape and
transcriptions.
original meeting, and Sumiko now sitting here talking us about it as well.

Sumiko Am I?

Members (laugh)

Derek Four Sumikos in fact.

Jonathan It's not really (indistinguishable) transcript. It's good enough to go out into the world. But its work is the idea if you do not get a rich perfection, it's just miserable.

A Member Mm.

Derek But that's not- it's not just to say it's particularly interpretative or subjective, because I think that's uh- reasonably, you know, people want some scientific practice in general-

A Member Mm.

Derek that the universe as described, which is to be theoretically explained- is transcript (indistinguishable) been described,

A Member Mm mm.

Derek uh- and that's how explanations and transcriptions will cope with each other. I think CA analysis, as well as transcriptions, are like the rest of scientific practice.

Sumiko Mm mm.

I want to ask Sumiko something about this. Is that- are you thinking this stuff primarily has interesting things for your thesis, or as a practical thing- a technical thing, that you still need to improve the transcript? (0.5) Or, are you, I mean, are you moving from the wave about this transcript to us?

Derek [Uh-] [(laughs)] to keep it.

Members [((laughs))]

Sumiko uh- in fact, both, [though.] [((laughs))] It's the process, as I told you, that one is for the data itself, and- and also I have to sort of- sort of trying it back to the DARG members. But as the data,

This formulation of "four Sumikos" addresses my own point regarding 'multiple subjectivity' (Chapter I, p. 52-55, and this chapter, 5-2).

What counts as 'data' and what counts as 'transcription' are the issues here. Presenting this extract, I seem to be problematising our grounds for displaying the world with data and transcription, yet still engaging in this display (for the sake of problematising it).

This relates to the first question in footnote 225. Rather than providing an answer, this speaker formulates it as Sumiko's concern. Sumiko expresses her concern to perform 'analysing-backs' with the participants (lines 1154-1155), and the paradoxical situation of constructing and employing data for the sake of discussing the problematics of constructing and employing data (lines 1155-1157).
1156. but (0.5) as the data, itself, to- to be- to present it as the data itself
1157. is at stake in this case.
1158. (1.0)
1159. Malcolm If- if what's through now, was the one of about the data,
1160. (indistinguishable) what counts as the data, it- it's- is it this
1161. transcription? Trans- is the transcription that (0.5) was here,
1162. before when you know the tape, just now?\(^{237}\)
1163. Sumiko Yeah.
1164. Malcolm That's the data. That's one, rather than this transcription now,
1165. Sumiko Min.
1166. Malcolm after we heard it, and have changed it. Is it wonderful now?
1167. [Is it more wonderful now?] Is it less wonderful now?
1168. Members [(laugh)]
1169. Kevin Um- (1.5) have we changed it?

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237 This question makes the point that any transcription is read and analysed in a particular setting. It points out its situatedness and contingency when it is used as 'data'.

238 Sumiko's performance in lines 1208 and 1210 is 'a projective work' (Chapter 4) for the achievement of her analysis with the data (the transcript of 21 February 1996). She projects that the transcription will be examined during and after the DARG members' discussion, and the talk which will be available from the recording of this discussion will also be analysed and read/written as/into the text of her thesis (or publication?).

239 In terms of the fifth question in footnote 225, the members did not discuss how they conceive of 'competence'. It can be interpreted, however, that the members' uptake of 'competence' is "agreement in practice" (Barnes 1995: 94-103). That is, they agree:

in ways of doing things, for without agreement of this kind we cannot say that people agree at the level of understanding: it is by taking note of agreement in practice that we attribute agreement in cognition. (ibid: 103)

To be a participant member in this discussion is to exhibit 'competence in practice'. Sumiko, who is here presenting the transcript of the recording of the earlier DARG (21 February, 1996) so that it can be
5-4. Please Do Not Finalise Paradox, Not Paralysis

Thus far, I have been taking an approach that tries to open any 'analysing-backs' to the Candidate's analysis and that enables her research to be in a process. Finally, I will deal with what is possibly "the hardest case" (Collins 1982).

After reporting her research in October 1996 at the STS Bielefeld conference (Mushakoji 1996; see also Sections 5-1 and 5-2), it was suggested by her supervisor that she should submit a paper for a special issue of a journal based on the session on 'reflexivity' held at this conference. She thus wrote a 'post-Bielefeld paper', but did not get any reply. After her supervisor's inquiry to the guest editor (who was also the chairperson of the session) she received an e-mail reply part of which is reproduced in the following extract:

grammatically corrected by the members, can (also) be seen as a competent member in this setting (DARG, 28 October, 1998); as is Sumiko (in DARG, 21 February, 1996).

In this sense, I imagine that you and I are also competently reading/writing this section in practice, even though you may (wish to) correct the grammar used in it.

I have employed the term 'finalise' in the Bakhtinian sense. That is, I do not want to preclude the 'event-ness' of the activity of your/my reading/writing from the outset. I do not take the reality of this event merely as a retrospective construction (Chapter 4). Instead, I try to make your/my reading/writing creative in an ongoing process: your/my self as an identified but differentiated participant, and our process of knowledge construction as open.

Obviously, the contingency in claiming what counts as 'the hardest case' for SSK studies is itself an interesting topic (Ashmore 1989; MacMillan 1996: 17, footnote 4).

My special acknowledgement to Prof. Dick Pels for his generosity to allow me to quote his comment in this section. As is clear in extract SU, lines 44-47, he insisted that I not include his comments. Permission to do so was granted after I asked him to cite them here, as they so outstandingly display how the Candidate's kind of reflexive approach — i.e., 'analysing-back' — can be chased into a predicament and a dilemma; and thus reveals the difficulties reflexivists may face. I feel that it is extremely generous for Prof. Pels to release his injunction and let me reveal and report the dilemma and difficulties to those who have concerns for engaging in reflexivity. I hope that this section fruitfully develops the point and the significance of, not only my stance — i.e., non-Positionism (see footnote 145) — but also his stance on reflexivity (see also Pels 1996, 2000).
1. Dear Sumiko,
2. Thank you for your nice letter (of 23. 11. 96, sorry) and paper, and a
3. happy 1997 to you! But I am afraid I will spoil your prospects of
4. happiness a little. I have great difficulty in responding adequately to
5. your paper's contents, which is very far from what I judge a fruitful
6. approach to reflexively aware social research. It is extremely self-
7. contained and self-absorbed, hard to access even for a sympa\textsuperscript{244} reader such
8. as myself, and considerably more narcissistic than I can swallow. In
9. other words: it's all about yourself and your private wordplay, and very
10. little about other people, endotherlin and/or the tradition of reflexive
11. writing in STS\textsuperscript{245}. It is very hard to make out what exactly you want your
12. paper to say, since you are always dodging away, switching roles, and
13. beating about the bush. You are very absent and play very hard to get in
14. this text about authorial presence\textsuperscript{246}. This was also a problem at Bielefeld,
15. and was signalled by some of your critics.
16. I am puzzled by the 'preludial story\textsuperscript{247}', where you have not even
17. taken the trouble to transcribe a comprehensive text and check it; even
18. from my own (faulty) memory of the session, it is easy to fill in quite a
19. number of your 'indistinguishable' and noises\textsuperscript{248}. As for the three stories
20. themselves, I have a hard time figuring out how precisely they are meant
21. to hang together in their 'sameness-and-difference' (isn't this a
22. terminological cop-out?). Your review of various reflexivity programme in
23. SSK, which purportedly examines 'the process wherein reflexivity
24. becomes integrated in SSK knowledge' (does it?), does not offer clear
25. criteria of division or discrimination (see table 5-6), and ends before
26. reaching any sort of robust conclusion (in order to make room for the
27. third story). I simply fail to understand \cite{p. 239-247}, which address,
28. among other things, the curious discrepancy of reaction between the

\textsuperscript{244} I have read this word as an abbreviation of 'sympathetic'.
\textsuperscript{245} I ought to emphasise that this criticism is directed at the \textit{then} 'post-Bielefeld paper' that the Candidate
submitted and \textit{not} at the text of Sections 5-1 and 5-2. I have sincerely taken this point into account in my
elaboration of the text of these two sections.
\textsuperscript{246} The Candidate is being faulted both for merely talking about herself (line 9), and of being absent (line
13). This is rephrased as the "simultaneous self-involvement and extreme detachment" (lines 56). In
what way does the Candidate's kind of reflexive approach appear paradoxically to engage in these two
textual modes? This is Paradox One of her approach, according to critics.
\textsuperscript{247} The submitted draft had a 'preludial story' whose reproduced version is the text of Section 5-2.
\textsuperscript{248} See Section 5-3, footnote 225, for the issue of 'competence' in making transcriptions. See also footnote
43 for the definition of 'competence' in ethnomethodology.
endothelin researchers and the social scientists (which now include me).

What precisely is your conclusion to this intriguing episode? Do the former read you realistically and skip the reflexive stuff, or what? What is the critical impact, over against scientists and unreflexive SSK, of your effort to generate objects and texts together?

Communication is an unlikely affair, says Luhmann, and he has written a pile of inaccessible books as if to prove it. 'Wrighting', reflexive style, runs the risk of stopping all communication, if the balance between self-orientation and object-orientation tips over so much towards the former. It seems to me that your version of it formulates a lot of interesting things to death. You never take a stand. Your inclusion of all reactions (even if very critical) as welcome 'interactions' rather than objections to be reckoned and argued with adds up, in my view, to a barren form of relativism (never thought I would use this word in reproach!). I would not be too sure about your ending claim that your work 'has potentiality to be developing in further interactions'. I don't think that we partake in a mutual process of knowledge construction, or even of negotiation. It may be clear that I do *not* allow you to include these comments in future papers, as you may ask. This appears a facile way of extending the reflexive loop, without the author (you) being in any way answerable or responsible for what she does to the reader (me).

If all of this sounds harsh, please excuse. But I rather be straightforward with you than myself circulate around the issue, which is to an appreciable extent moral and personal. I am sure you are a very bright, intelligent, and erudite person with considerable linguistic talent (which clearly transpire even in this hard-to-get-text), but which in my arrogant perception go to sorry waste if you continue along this path of simultaneous self-involvement and extreme detachment. Please take the trouble to say something and take a stand, for otherwise people, even sympa reflexivists, may think that you have nothing to say.

Best regards

(Pels, 16 January, 1997)

See the previous footnote.
I knew that I had to deal with this stuff.

(When I received this reply in the beginning of 1997, I suffered too much to keep working on the research with my approach which has been criticised this way. After one month blank in February 1997, with the support and encouragement of my supervisor and colleagues, I have more or less recovered and have continued working on it up to now.)

But I know that I have to deal with the emotionally 'hardest case' now. (At the very end of the last chapter of this thesis.)

No.

Actually, I still can't. It's hard to know how to reply -- i.e., analyse back to the above criticism while opening my text to the potential counter-analyses which may then be made by critics similar to the one above. This critic seems to have almost terminated my own process of knowledge construction. If I ignore it, then the immediate implication is that my text is not letting all the other voices to speak. But if I accept it, then it is I who must be silent. Either way, my approach would not be consistent²⁵⁰.

So, what can I do?

After a few weeks of suffering again, I wrote the following e-mail message to my supervisor and colleagues, in order to consult about my situation:

5V

60. Now, I am writing the final chapter and facing the difficulties. […] In this chapter, I am considering to deal with my presentation at Bielefeld
61. and the post-Bielefeld paper which I wrote and which was rejected with the criticism quoted above (extract 5-U). I think that I still hope to keep
62. my reflexive study of science with such an approach that tries:
63. (1) to welcome any texts which respond to my analysis, as their
64. 'analysing-back', by, again, analysing-back to them,

²⁵⁰ This is Paradox Two.
(2) to display that this sequence of 'analysing-backs' will ongoingly and potentially proceed, and
(3) to demonstrate that the knowledge of my research object is constructed in a process wherein the texts I produce are interwoven with the other texts of 'analysing backs'.

However, it is still difficult for me to find a way of responding to the above criticism. I want to argue that my approach is not "self-contained and self-absorbed" (lines 6-7), but open to any other texts, including those in science, SSK and those produced by the Candidate herself. Nor is it the extension of "reflexive-loop" (line 48). I believe that it is a triple process -- namely, the 'triple' of endothelin research, SSK and the Candidate's research, and the 'process' for going on by taking successive turns.

As you can expect, I will *not* take the 'last' Authoritative voice in order to argue against this criticism. My 'analysing-back' will, this time again, include this criticism, as well as those similar to it, into the further research.

I am hoping that this series of analysing-backs will not be a mere argument, but will be a process of knowledge construction. That is, by analysing-back to it, the readers will know more about my research object (i.e., the process of knowledge construction). I am ambitious of making this attempt a bit (even though far less qualified) like the one advocated in 'Beyond the Tu Quoque'

(Ashmore 1989, Chapter 3).

However, I still feel that the above criticism is too difficult (and harsh) for me to be *really* able to take its voice. The hardest part is in lines 43-49. How can I avoid including this command into my writing, even though it does *not* allow (line 46) the inclusion? Whether I literally write about it or *not*, I am unavoidably including it! (As I am now.) In line 29, this critic

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251 To those who fear the 'infinite regress', as regularly threatened in critiques of reflexive studies, MacMillan suggests 'reflexivity as therapy' (MacMillan 1996) as its dissolution. Reflexive studies are not orienting towards infinite mechanical repetitions, adding yet another meta-level, nor sinking into the self-indulgence of pathological subjectivisation. Instead, to take a reflexive approach is to take a next turn -- that is, an opportunity for enjoying another angle in and through one's analysis. See her description of the 'spiral of reflexive turns' (MacMillan 1996: 24-31; see also footnote 14).

252 In e-mail correspondences, using this notation instead of italic is conventional.
also paradoxically admits the inclusion of himself into this event. So, whether and how can I analyse-back to this criticism? I cannot include it (if I include it), and I will inescapably include it (even if I try not to include it).

Paradox.

In one of the chapters of my thesis, I have dealt with the case in which my analysis has been returned with 'silence'. But this time, it is the criticism which commands me to be in 'silence' (But is it really so, indeed?) To this situation, what would be your advice?:

(a) How shall I (do not) include the criticism?
(b) Is it ethically bad if I inevitably (do not) include it? In what sense?

For the second question, to include it anonymously might be a compromise. It may be easy to do so, as I have already employed all the participants' (endothelin researchers and social scientists) anonymously in the post-Bielefeld paper. But I am not so sure if this compromise is a good way. I have been on this matter for a week. I sometimes feel like just throwing it away to conceal the most depressing criticism I have got. I feel like just forgetting about it. But I know such concealment will breach my entire endeavour for the approach I have taken thus far. I need to get enough energy (not the kind of macho force, but the inspiring, amusing and creative one) to cope with it.

Even though my reflexive approach is criticised for "formula[ing] a lot of interesting things to death" (extract SU, lines 38-39), I believe that it brings to life many interesting things, like the emergence of this paradox. To counter-attack this criticism on the grounds that it itself produces paradoxes is the last thing I would do. I have no wish to collaborate in the drive to eliminate paradoxes at any cost, especially when it requires the creation of highly artificial formalisms, puts too much stress on bland consistency, and too little on the quirky and bizarre, which make life and mathematics interesting. It is of course important to try to maintain consistency, but when this effort forces you into a stupendously ugly theory, you know something is wrong. (Hofstadter 1979: 22-23)

So, I am interested in how this comment (extract SU) denies itself to be an 'analysing-back' while it precisely appears to become an 'analysing-back' (for the Candidate), and how it 'negotiates' (the Candidate's description) its refusal to negotiate. For the construction of the 'interesting', see Davis (1971). For the mechanisms of paradox, see Hofstadter (1979) and Ashmore (1989).

A further explication will follow (p. 286-288).

In a sense, it is inevitable that this criticism alternatively becomes a kind of invitation for a next turn — that is, an opportunity for an analysis.

But I have not, in the text of this chapter.
Please help me.

Yours, Sumiko

p. s. Now, as you know, your advice (and my integration of it into the further research) is placed in a dialogue, and will proceed our process of knowledge construction. I hope you can collaborate with me.

(Mushakoji, 1998, Personal e-mail communication)

This request was responded to by my supervisor and colleagues with encouragement to keep going. The replies all refer to the familiarity of this kind of criticism in attacks on reflexivist studies. They also pointed out the paradoxes raised in it. The issue of anonymity was addressed in various ways. Some emphatically expressed the difficulty of specifying the participants' identities, while my supervisor problematised the notion of textual anonymity itself.

And suddenly, after a long time of wondering how I could deal with it and right before really finishing the thesis, I have found that I know that the paradox can also be a construction -- that is, I can also think that the matter which stops the Candidate's

257 I am especially grateful to Dr. Malcolm Ashmore, Dr. Katie MacMillan, Dr. Mary Horton-Salway, Dr. Kiyomi Murakoshi and Mr. Nobukazu Mushakoji regarding their helpful replies on this matter.

258 If I can quote this supervisory advice directly, "On the argument about cowardice above, to anonymise would not make too much sense. And you could dramatise your 'honesty' by making Pels the *only* character in this chapter who is *not* anonymous. But again this must be addressed directly." (Ashmore 1998, personal communication)

259 To be honest, this finding can only be possible with the suggestion provided by the examiners of this thesis (my acknowledgement to Prof. Derek Edwards and Prof. Steve Woolgar). To be very honest, it is precisely one of 'the required corrections' that I am to discuss and reconsider the function played by the notions of 'paradox' and 'consistency' in this section, perhaps by recognising that although they seemed transcendent obstacles at the time, and hence the cause of action [or inaction], they can also be [retrospectively] deemed deconstructable" (Joint report by the internal examiner [Prof. D. Edwards] and external examiner [Prof. S. Woolgar], 9 February 2000). Yes! It is excellent that 'paradox' (and even 'consistency') is a construction, and thus contingent and local. They can finally be 'the hardest case' (see footnote 242) for reflexivists to be able to see and explicate their constructed nature, as the reflexivists are sharply aware of the constructed nature of 'death' [i.e., the most moral issues] and 'furniture' [i.e., the most materialistic issues] which are the bottom line for realists (Edwards, Ashmore and Potter 1995), but may tend to cherish, like myself, such constructions as 'paradox' and 'consistency' in arguments (and thus make them their own 'bottom line').

I wish that I had realised the contingent nature of the paradox earlier. (It's still powerful here and now.) But I have realised the nature of these paradoxes in time, so let me deal with their contingency, even though they are nevertheless paradoxes...

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research from proceeding can be seen as a paradox on the 'Inviolate Level' where I have been placed (see Chapter 1, p. 6-11; Chapter 3, p. 165-167). As I have written, the paradox is formulated as such (or not) in a particular setting with its own contingencies. Namely, it's here and now, as I see it so. Moreover, the negative value of these paradoxes is also contingent and local. Even though such contingency never implicate our recognition that we do not have to respect it, we can still begin to see how it is valued as such on this particular occasion. Here, "[f]t is not a matter of being correct" nor "[f]t is not a matter of solving a problem" (Ashmore 1989: 99). This thesis is constructed as a 'celebratory practical reflexive inquiry' (this thesis, Chapter 1: 6-7).

No matter how serious an obstacle it seems to the Candidate's approach, this appearance is contingent and local. As a matter of fact, it has been paralysing me from keeping on with my research: but surely, my research is still to be in a process (see what I am doing right now, right here), wherein I will again . . . how can I keep on the Candidate's process . . . without undermining the voices of the critics (and the voice of you), while letting all the voices speak?

Let Me Keep It as A Process

As a candidate SSKer who espouses relativism and social constructionism and who engages in a PhD research with reflexivity, I have tried to study the process of knowledge construction, including the concern of whether and how this research itself can be (in) a process of knowledge construction. The reflexivist's programme encourages me to employ a variety of textual forms to celebrate my own constructive practices (Ashmore 1989; Mulkay 1985) and to use language which projects the "dynamic irony" of one's own textual constructions (Woolgar 1983).

Throughout the writing/reading of this thesis, I have been trying to study the process of knowledge construction in endothelin research, SSK and the Candidate's research as a triple process. On the one hand, SSK is the research domain from which the Candidate's research is informed, and thus the Candidate's research is to be the very
site where the knowledge construction in endothelin research is to be studied. On the other hand, however, even though I am trying to write a candidate SSK study and thus the 'direct' research object is to be the knowledge construction in science, I have not rendered SSK and the Candidate's own research merely as 'the background', 'resource' and 'taken for granted'. I have not avoided including a discussion of the knowledge construction simultaneously taking place in SSK and in the Candidate's research. Such an inclusion might be controversial: critics would argue that a researcher should 'only' discuss her research object, never her resource: discussing her work is an entirely different practice from doing it. But the inclusion comes from my conviction that to engage in a science study with reflexivity -- i.e., acknowledging and explicating the self-referential characters of studying others -- does not result in a regress from a 'direct' and 'thorough' study of knowledge: it is instead precisely one further way to probe into the process wherein knowledge is constructed.

Such an engagement is particularly relevant in studying the process of knowledge construction. I have made an assumption that the research object -- that is, in my case, the process of knowledge construction -- has also been constructed in writing/reading this thesis. Throughout the writing of the thesis, I have thus inquired how its own knowledge-claims can consistently address the way these knowledge-claims themselves, through the analysis and 'analysing-backs', work as knowledge construction. In this sense, the thesis is my own wrighting (writing, righting [correcting], and wright-ing [making and working] [Ashmore 1985, 1989] of and for the process of knowledge construction.

However, it has come to be evident that wrighting the triple parallel process needs a vital feature to be successful. In order not merely to make the Candidate's research encapsulate endothelin research and SSK, it is crucial that the triple parallel knowledge construction proceed with the endothelin researchers, social scientists and the Candidate herself in dialogue.\footnote{I use the term 'dialogue', both in the Bakhtinian (Bakhtin 1981, 1986; see also footnote 53) and in the reflexivists' sense (Woolgar and Ashmore 1988: 6). Even though the reflexivists complain that the latter is sometimes narrowly understood as a device for displaying a debate between two identifiable and...}
variety of 'analysing-backs' are analysed back in a sequence. I have performed this display in the hope that my reading/writing may become dialogical with you. If and only if this reading/writing resonates with other participants, the research object of this thesis (i.e., the process of knowledge construction) will proceed in and through the continuous construction, deconstruction and reconstruction of itself. Thus I have been asking you to construct this process in your reading/writing: you have been invited to participate in the Candidate's research. Now, I have a further request: will you please analyse-back to it? To put it another way, will you let me finish the thesis by awaiting your analysing-back?

Thus far, I have found three types of reactions to this approach:

1. Agreement to participate in dialogue
2. Silence
3. Rejection of the invitation

Fortunately, the responses of most of the participant scientists and social scientists to whom the Candidate made a request for 'analysing-back' have been of the first type, as, for example, shown in Table 5-7 (p. 243-245). The type two response, 'silence', was examined in Highsmith's case (Chapter 3, p. 166-168). But there may potentially be more for this thesis. The third type of response has been those (mildly) provided by the Speaker in extract 5B-5D and outstandingly by the critic in extract 5U.

If you at least accept a role, our ground is the process of knowledge construction. That is, we can construct, deconstruct and reconstruct the research object. Alternatively, what constitutes 'the research object' and its legitimacy can be negotiated between us. But if you take the second type of response, 'silence', you will not explicitly deconstruct nor reconstruct the Candidate's research object. Your silence can, however,
be still inscribed, interpreted and included in her research. She is at least still speaking to you and including your silence as a response. Now, the third type of response, in which you declare your unwillingness to participate in dialogue generates a paradox: a refusal to respond is a response. It also places me in a dilemma: I am unable to let this paradoxical voice both speak and not speak.

In that any kinds of response provide opportunities for a further analysing-back, those responses which are antagonistic are no different from those which are supportive: the variety of responses has provided me with challenging opportunities with which the research has ongoingly proceeded. In terms of 'challenge', the supportive responses can be just as hard: for instance, when my DARG colleagues supported the Candidate's analysing-back by taking a next reflexive turn, this turned out, somewhat awkwardly, to be one that Sumiko endorsed (see Section 5-3).

To me (at least) silence and rejection seem to be part of a dialogue even while it does not show any sign of voluntary participation or it clearly refuses to be. (Are you speaking, with your voice, in refusing to speak with your voice?) I believe that the voices potentially raised in those responses have been (no matter how that is prohibited like the one in 5U) heard, included, and consulted as information and advice. For instance, although the post-Bielefeld paper was rejected, its critique has presented me with the opportunity to explicate how the Candidate's kind of reflexive approach is critical, with its unavoidable inclusion into her further research. I believe that this is my right way to cope with it. However, from the very beginning of this thesis, it tries to follow the approach of letting the other voices speak (Chapter 1, p. 4, 6-7). In trying to listen to the silence and the refusal of participation, I have clearly come to see that they have the right to not participate in the Candidates' approach. Taking their voices sincerely, now I find myself longing to leave them be and not to force her approach onto them (see also the discussion in Chapter 3, p. 167-168; Chapter 5, this section). Thus, I am wrong to drag them actually into the Candidate's approach, even though

\[262\] To be honest, however, it is still hard for me to respond to all of the responses with a celebratory style, and without feeling paralysed. But I will never be paralysed. I will take paradoxes but not paralysis.
paradoxically, to listen to their voice can only be possible by having a dialogue with them.

Let us end by returning to extract 5U, which refused to allow itself into the Candidate's approach. But now this critics' generosity has released the Candidate from this injunction and given her a chance to explicate the issue. In this extract, the Candidate's stance is so thoroughly interrogated. The critic asks the question "what is the critical impact, over against scientists and unreflexive SSK, of your effort to generate objects and texts together?" (lines 31-33). My stance is, however, not one of "being against" scientists and unreflexive SSK, anyway. What the Candidate has been trying to claim is that her research is open to other voices, including those of "scientists and unreflexive SSKers". Those other voices may be against the Candidate's voice, but my response is to include them so that she can ongoingly make her own voice, as well as her knowledge-claims, related to them. Of course, she will in turn make her own knowledge-claims by further 'analysing-back' to them, opening her research for their further 'analysing-backs'.

The writing of this thesis takes the opposite direction from one whose "balance between self-orientation and object-orientation tips over so much towards the former" (lines 36-8). My thesis is entirely oriented towards my research object (i.e., the process of knowledge construction) and the thoroughness of its investigation reaches to the extent that it includes the process of knowledge construction by self, which immediately, incessantly and inevitably relates with those proceeding in endothelin research and SSK. But of course, the critics can disagree with my claim, and attempt to set my writing in another direction. I will keep on and on setting my writing in the direction that I claim to be correct, naturally. Likewise, I hear the critic saying "I don't think that we partake in a mutual process of knowledge construction, or even of negotiation." (lines 44-46). But (as I repeat), are we not negotiating over how science studies with reflexivity ought to proceed? And are we not participating in any kind of knowledge construction?
No. "It may be clear that I do *not* allow you to include these comments in future papers, as you may ask." (lines 46-47). Yes. Very. Okay. I sincerely take that. Those who are silent and those who refuse to participate should be left in peace. I have no doubt that they have the right that their will be protected, respected and cherished. I would be wrong and insensitive if I make the Candidate's call for her kind of reflexivity to be the one and only voice, naturally. That is, now matter how carefully and deliberately the Candidate's approach attempts to open to the others, such an attempt can nevertheless be authoritative and high-handed. (My writing may thus be wrong and insensitive here.) Still, it will not be "clear" to me how I can obey this command without having any dialogue. Let me have the chance this time, to ask how I shall take the paradox in it -- that is, how I shall both include these comments and not include them in the future papers: am I only inhibited from citing explicitly? Would such an inhibition need to function as a concealed power in my future papers? Would the logical power of the paradoxes which the Candidate's approach initiates kill it off? If so, what would be the appropriate extent to which one should relativise one's own knowledge-claims with respect to other voices? How far can one be open to other voices such as this critique? I will not be able to know these questions, until (unless) I take the opportunity of asking you to read/write back.

But what have you just said? "This appears a facile way of extending the reflexive loop, without the author (you) being in any way answerable or responsible for what she does to the reader (me)" (SU, lines 47-49). Well, let me claim that this approach does not extend any "reflexive loop". It keeps taking a next turn. The turns are consistently informing and constructing the process of knowledge construction. My answer is that it is itself in a process of knowledge construction, and I am responsible for it, and to it. I am answerable and responsible, just as relativists can be answerable and responsible, perhaps in a better way than others (Edwards, Ashmore and Potter 1995). Besides, you are very wrong to see my approach as "facile". But what are you saying now?
"Please take the trouble to say something and take a stand, for otherwise people, even sympa reflexivists, may think that you have nothing to say" (lines 56-58). That reflexive studies do not take a stand may be a common reaction of their critics. However, not taking a stand is my stand. That is, my stand is to include any other voices raised from different sorts of stances, so that any single stand can be relativised (Edwards, Ashmore and Potter 1995) and hybridised (Latour 1988, 1993). (The issue of how a stand creatively constructs its object is an excellent topic for [future] analysis.) Given that my stand is 'not taking a stand', am I creating a paradox here? Maybe. But rather than allow myself to be paralysed, I will pursue my research in the full recognition of this paradox. Do I insist I am not the Author? Of course not (Section 5-2). But let me place the text of this thesis, for a final, yet unfinalised, time in a dialogical setting. It is then your reading, which will be read by me as your writing. Can you (please) see what I am saying in my doing (and doing in my saying)? My doing is for, and my saying is about, the triple process of knowledge construction. Let me take a stand for wrighting.

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263 This point, made in the criticism, was commented on by my supervisor:

I found it extremely depressing. I sometimes forget how common his kind of reaction is; I am thinking specifically about his irritation at your "refusal to take a stand": your insistence that every response to your text can add to it; rather than threaten it. 'Positionism', as Steve Woolgar calls it, runs very deep. (Ashmore 1997, personal communication)
Appendix I: Nonbibliographic Sources

INTERVIEWS

I conducted the following interviews with endothelin researchers. All of them were recorded and transcribed. Before each of the interviews began, I provided a list of my research interests and questions for the interviewees. They were thus informed that I was interested in the discovery of endothelin, the development of endothelin research, and getting the relevant literature and accounts of 'the process of knowledge construction in endothelin research'. In all cases, however, the interviewees and interviewer discussed endothelin, endothelin research et cetera in quite an unstructured manner.

In two of the cases, interviews were repeatedly conducted and in all the cases, interviews were followed up with ongoing correspondence and informal meetings. The topic of discussions then became open and varied. With continuous feedback, those interviewees informed me of their then-current interests and latest findings, and also started to provide advice and suggestions about my own research. This resulted in fruitful dialogues as follows: (See also p. 290, CORRESPONDENCE.)

Prof. Katsutoshi Goto
14 March, 1994
21 April, 1994
15 June, 1994
2 October, 1997

Prof. Sadao Kimura
1 July, 1994
26 April, 1995

Dr. Masashi Yanagisawa
26 April, 1994

Prof. Tomoh Masaki
7 November, 1995

Dr. Yukiko Kurihara, and Dr. Hiroki Kurihara
8 November, 1995

Prof. John R. Vane
17 September, 1996
INFORMAL OCCASIONS
As stated above, I have benefited from engaging with the participants in a variety of informal occasions, which could not be tape-recorded.

ETHNOGRAPHY
I have documented my observations during the various interviews, conferences, symposiums, seminars and discussions. Such records range from short scribbled memos to long ethnographic descriptions.

DIARY
In the 'Qualitative Methods' course for Postgraduate Research Training (see OTHER DISCUSSIONS AND SEMINARS, p. 294), I recorded the progress of my research for a few days as 'ethnography, and also as a 'diary'. I tried to see how I could write my account of 'what happened to me' within the two different genres. However, because this attempt was pursued as 'course work', I received feedback from an organiser of the course (Prof. Derek Edwards). His comment is a nice analytical and reflexive comment, which indicates that my attempt to construct two different versions of reality was not successful.

CORRESPONDENCE
The following scientists and social scientists have been in correspondence with the Candidate with reference to her research on the 'process of knowledge construction'. Some of their readings of the 'JJMI article' (Mushakoji 1995, see Chapter 5, 5-1) have been especially helpful, and are included due to their intertextual linkage with this article and as constructing knowledge relevant for the Candidate's research. This correspondence is not merely a past record, but provides opportunities for an ongoing process of knowledge construction (see Chapters 4 and 5) and as 'analysing-backs' to the Candidate's research (see Chapter 5).
With Scientists
Prof. Robert F. Highsmith, Prof. Sadao Kimura, Prof. Tomoh Masaki, Prof.
Katsutoshi Goto, Dr. Masashi Yanagisawa, Dr. Yukiko Kurihara and Dr. Hiroki
Kurihara, Sir. John R. Vane, Dr. Ikiko Tsuritani, Prof. Masayuki Yoshida

With Social Scientists
[At Keio University, University of Library and Information Science and others]
Mr. Nobuyuki Midorikawa, Ms. Keiko Kurata, Prof. Sadao Uematsu, Prof.
Shunsaku Tamura, Ms. Nozomi Ikeya, Ms. Yuko Yoshida, Mr. Nobukazu
Mushakoji

[At Loughborough University]
Dr. Malcolm Ashmore, Dr. Katie MacMillan, Dr. Mary Horton-Salway, Dr.
Kevin McKenzie and other DARG members

DOCUMENTS
I have had access to scientific documents provided and/or recommended by the
participant endothelin researchers. Some of them have been particularly useful for
writing about knowledge construction in endothelin. The following are a list of these
documents and a bibliography of those which are not included as References in this
thesis (* asterisked).

From Prof. Goto
Clotzel, Martine et. al. 1993.
* Goto, Hama and Kasuya. 1996.
Inoue, Akihiro et. al. 1989.
Miyauchi, Takashi et. al. 1989.
Miyauchi, Takashi et. al. 1993.
Sakai, Satoshi et. al. 1996.
Sakurai, Takeshi et. al. 1990.

From Prof. Kimura
Lecture note on 'Recent Advance in Endothelin Research'

Highsmith, Robert F. 1992
Hosoda, Kiminori et. al. 1994.
From Prof. Masaki
Lecture note for the preliminary speech at the Fourth International Conference on Endothelin. In 1995, Prof. Masaki has also given me the list of 33 reviews of endothelin authored by himself.

* Gulati, Anil. 1995.
Ikura, Tsuyoshi et. al. 1994.
Miyauchi, Takashi et. al. 1989.
Ohnaka, Keizo et. al. 1990.
Okada, Kenji et. al. 1990.
Schmidt, Martin et. al. 1994.
Takada, Junji et. al. 1991.
Xu, Dong et. al. 1994.

From Dr. Kurihara
Referee's reports to Kurihara et. al. 1994
* Kurihara et. al. 1995.

From Sir Vane and his group members
Harrison, V. J. et. al. 1995.
Vane, John R., Erik E. Anggard and Renia M. Botting. 1990.

[BIBLIOGRAPHY]
de Nucci, Gilberto, Roger Thomas Pedro D'Orleans-Juste, Edson Antunes, Claire Walder, Timothy D. Warner and John R. Vane. 1988. Pressor effects of circulating endothelin are limited by its removal in the pulmonary circulation


SCIENTIFIC MEETINGS
I have attended the following meetings on endothelin research.


Fifth International Conference on Endothelin (ET-5) September 12-15, 1997, Kyoto, Japan

The 10th Japan Symposium on ANP: Biocommunication Systems Regulating Cardiovascular Functions (C. O. E. International Symposium) 18-20 November, 1995, Senri Life Science Center, Osaka, Japan


CONFERENCE PRESENTATIONS
I have presented the Candidate's research on the following occasions. A variety of feedback from the participants has turned out to be a useful resource for the conduct of the research, and has been included in the text of the thesis.
Signature of Knowledge Societies - Joint Conference Bielefeld 1996 (European Association for the Study of Science and Technology [EASST] and Society for Social Studies of Science [4S]), 9-12 October, 1996, Bielefeld, Germany.


OTHER DISCUSSIONS AND SEMINARS

Just as with the above CONFERENCE PRESENTATION, I have been provided with a variety of opportunities to receive helpful and inspiring feedback from colleagues and friends. To list only a few, the following occasions have been particularly seminal for 'writing' the thesis.

Some of the e-mail discussions and comments scribbled on the draft of thesis are cited as 'personal communication' in the thesis.

'Qualitative Methods' course (for Postgraduate Research Training) 1994-1995
(Organised by Prof. Derek Edwards; Lectured by Prof. Derek Edwards, Prof. Michael Billig, Dr. Malcolm Ashmore, Dr. Mike Pickering and others)
I gave a presentation on methodology, 'data' collection and construction regarding my research and participated in inspiring discussion throughout the course.

Presentations at DARG meetings (Discourse and Rhetoric Group)
The following topics regarding the Candidate's research were discussed with the aid of handouts:

Models of 'Scientific Information Flow' and Intertextuality in Scientific Texts (14 December, 1994)

Object / Phenomenon / Practice and Texts in Microbiology, SSK and My Writing: Intertextuality Re-challenged (17 May, 1995)

Data Construction (13 December, 1995)

Analysis Being Analysed Back (21 February, 1996)

Ongoingly Arguing on the Truth, the Known, the Right Disciplinarity and the Potentiality in Science and Social Sciences: Interaction in Conference Talks (4 June, 1997)
Strategic Projecting (29 October, 1997)

_Competence_# [for - replace with "and"]* Being 'Good Enough' for a Transcriber in English and [insert "the"] _Construction_ of 'Non-Native-ness' (Title originally created by Sumiko Mushakoji and altered by Dr. Malcolm Ashmore for: * englishing and #typo?)

Supervisory Sessions

I was supervised by Dr. Malcolm Ashmore regularly once or twice a week from October 1994 to June 1995. After I returned to Japan, Dr. Ashmore has been supervising and encouraging me mainly by e-mails. We have also had supervisory discussions during my short visits to Loughborough as a part-time student.

Progress Report and Panel Meeting

In 1995, I presented a progress report, 'The process of how discoveries become integrated into a body of knowledge in science, the Sociology of Scientific Knowledge and a writing of PhD thesis: a reflexive, ongoing and intertextual study', to the Department of Social Sciences, Loughborough University. On 2 October 1995, the Panel meeting was convened by Dr. Alan Beardsworth (Chair), Dr. Malcolm Ashmore (Supervisor), Prof. Jonathan Potter (Director of Research) and Mr. Arthur Gould.

After the panel meeting, a report was distributed by Dr. Beardsworth. Some of the comments in the report are as follows:

A full report of progress to date was tabled and formed the basis for the discussion. Sumiko outlined the development of her interest in the incorporation of novel objects of analysis into scientific discourse, and of her interest in adopting a reflexive approach to her own attempts to describe this process.

A considerable amount of discussion was centred upon her intention to interweave three partially independent themes into the fabric of her thesis:

(i) The Scientific 'story'
(ii) The deconstruction of the scientific 'story'
(iii) The reflexive analysis of this deconstruction

Sumiko expressed a sense of confidence that she could handle this complex interweaving successfully.

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I distributed a response to this report (17 October, 1995), part of which is:

Thank you for organising the panel meeting for me and handing me the report! . . . The meeting and the report are also of help for me to engage in my reflexive analysis. The talk recorded at the meeting and the report which is to be a summary of the talk, are inevitably included into the successive writing for my thesis. . . . Viewing the report, I find that what I have explained in terms of 'three [parallel processes]' was summarised in such a way that can be said to be a summary of how my interest is interpreted and assessed from the point of view of social scientists. . . . For instance, I didn't think that I attributed the status of 'story' (sic quotation mark) only to the writing with [the process in science]. Won't it be that 'deconstruction' (quotation mark) is also a 'story'? Won't it be that writing of 'reflexive analysis' is again a 'story'?

Discussions with colleagues

I have had many helpful and encouraging discussions with my colleagues at Loughborough and Tsukuba. Amongst others, ongoing discussions with Dr. Katie MacMillan, Dr. Mary Horton-Salway, and Dr. Kevin McKenzie have been essential for me to write the thesis, and to keep on 'constructing our knowledge in process'.

And . . .

(In the forthcoming) Viva Presentation

I hope to receive really helpful comments . . .

PROOF-READING AND TRANSLATING WORK

Dr. Kevin McKenzie, Dr. Mary Horton-Salway and Dr. Katie MacMillan have assisted me by proof-reading. Basically, they have examined my writing and translation of Japanese talk into English. Often we have had long negotiating discussions about the work in order to make the English texts readable without losing the crucial points and/or the speakers' discursive work in the original texts. This collaborative work has turned out to be not merely a technical conversion, but rather an engagement in the very issue of writing (and 'wrighting') a thesis, reflexively.
Appendix II: Transcription Conventions

The transcription symbols used in this thesis are based on the system developed by Gail Jefferson and standardly employed in Conversation Analysis (e.g., Heritage 1984). However, since most of the recorded talk (other than that in Chapter 5) were originally Japanese and have been translated into English, I have not used the symbols for indicating stress, sound and intonation of English conversation. When native speakers can hear the participant speakers' connotation from phonetic stress, sound and intonation in a certain language, its translation is a very challenging task. Moreover, in the recordings of interviews, I often notice that speakers do not say or omit saying 'it', presumably, for the sake of courtesy and/or preference for vague and euphemistic expressions. In this thesis, however, I have in most cases, omitted translating them as I felt that it would require too much interpretative intervention into the original talk. In some cases, however, it was necessary to indicate participant speakers' discursive practices in their talks, and these are indicated by 'ethnographic descriptions' -- i.e., ((double bracket)).

[ A left hand square bracket indicates overlapping speech at the point where the overlap begins

] A right hand square bracket indicates where the overlap ends

( ) A dot in brackets indicates a hearable pause that is too short to measure

(0.1) Numbers in bracket measure pauses in seconds and tenths of seconds

(indistinguishable) Utterance unclear, best guess presented

(cough) Hearable sound other than speech

((double brackets)) Non-verbal action performed, and some crucial ethnographic descriptions
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