Examining one's own :
Reflexivity and critique in STS

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thesis submitted to the Faculty of the
Virginia Polytechnic Institute and State University
in partial fulfillment of the requirements for the degree of

Master of Science
in
Science and Technology Studies

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february 7 2002
blacksburg, virginia

Keywords: Reflexivity, Critique, Normativity, Objectivity

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(abstract)

The principle of reflexivity, as laid out by David Bloor (in Knowledge and Social Imagery) poses serious challenges to STS - while STS analysts attempt to show the partiality of scientific claims, they simultaneously offer those analyses via authoritative pronouncements in scientific language, while claiming a scientific foundation.

This thesis questions the understanding of science as a form of inquiry distinct from other forms of inquiry, especially focusing on the elusive distinction between science and technology. The thesis analyzes Andrew Pickering's problematic attempt (in The Mangle of Practice) to dissolve the science/technology distinction through his 'Theory of Everything'/Mangle concept. Building an approach from commentaries on Pickering's work combined with resources from the STS tradition, especially from Latour and Haraway, the author proposes a new observational stance; this stance insists on the perspectival nature of all observation, and thereby claims to be reflexively robust; furthermore it maintains an agnostic attitude with regard to the science/technology distinction.

Foreword

"Science and Technology Studies (STS) is a relatively new field of academic studies. STS explores the relationship between science, technology, and society using a variety of disciplinary and interdisciplinary approaches. Research in STS analyzes how society affects the development and implementation of scientific, technological, and medical knowledges and practices and how scientific, technological, and medical pursuits affect society. The research and scholarly interests of STS faculty cross a wide range of disciplinary boundaries: some rely on fieldwork, others are immersed in historical or governmental archival research, while others develop social and conceptual analyses to answer theoretical or ethical questions." (http://www.cis.vt.edu/sts/AboutSTS.htm) The program at Virginia Tech began as the Center for Study of Science in Society in the mid-1980s; the graduate program in Science and Technlogy Studies awarded its first masters degrees in 1988, and first PhD in 1991.

Virginia Tech is a major land-grant university in south-west Virginia. Formerly the home of the 'Fighting Gobblers', it advertises itself as "a university putting the power of knowledge to work"; the sidewalks are overrun with large trucks and delivery vans, and pedestrians fend for themselves; and if vehicles are barred from some sidewalks, then the fine lawns became the new highways of this fine university. The university has instituted an 'aggressive' program to become a 'Top-30' research institution, and its main library - the Newman library - may close at 5pm on various Saturdays if it is raining.

Chapter One: Introduction

This master's thesis has been long in coming, and perhaps even long enough for me to have come to terms with its material. Over the duration, I have been asked repeatedly what my thesis is about, to which I have usually replied evasively. I have until now been unable to venture any concise description, perhaps the most serious difficulty arising from the serious shifts the thesis has undergone.

I began this thesis late in my master's program. My inability to finish on schedule led to my re-entry into the profession of software engineering, which I had left (behind, I thought) when I entered the program a few years before. Hence, as I propelled myself through this master's thesis, I was simultaneously propelling myself through several software projects. The crosstalk between these (sometimes mutually exclusive) efforts was an important part of the conceptual development of this thesis; as such this thesis attempts to interweave the narratives of these efforts. It is hoped that this ploy help to convey something of that fertile crosstalk between these coincident efforts.

The thesis had its roots in my educational background - a physics-major who, convinced of the greater market value of an engineer, switched in the middle of undergraduate studies to electrical engineering. My subsequent short career as a software engineer in a government research lab in which science/technology were not separable led me further towards a appreciation of the insubstantial nature of the boundary between science and technology.

This background thus left me unprepared for the prevalence of the sturdy and unargued science/technology distinction that was deeply embedded in the fabric of the

STS curriculum. Science and technology were thought to be distinct things, even though no one could provide a passable present-day definition of science which did not involve a reliance on technology. And the definition of technology is itself a potentially all-consuming effort. Where one would expect that such a distinction would arise out of some sense of two things and their differences, it seemed here that the definitions which were offered were instead provided to justify the distinction. It appeared to be more important to maintain the distinction than to convey the sense of that distinction.

So I began the thesis as a critique of that distinction. I proposed to develop a new perspective which I (following Hickman) called a "technological" perspective (which attempted to dissolve the distinction), opposing this to the "scientific" perspective (which maintained the distinction). I intended to ground this critique in some conception of "practice", as opposed to various "abstractions" from practice (such as "the products of scientific practice"). Following Latour, I intended to interpret the "scientific" perspective as the systematic abstraction from practice, but an abstraction which itself was a certain kind of practice, that is, a technological activity. That is, one could understand the scientific perspective as a particular and complex arrangement of technological practices. With this framework, I intended to make the case for a re-visioning of scientific activity through this "technological" perspective, one which dissolved the science/technology distinction, and would thus make possible new approaches which were not crippled by the poisonous influence of that distinction.

And so the thesis began ambitiously. But upon reading Andrew Pickering's The Mangle of Practice, I found Pickering's performative/representation distinction a much more strategic formulation of my own technological/scientific distinction - strategic in the sense that it did not try to redefine terms that most people assumed they understood quite well. While I found Pickering's presentation of his framework simplistic, I found in Mangle at least the seed of an approach, one that began without artificial categories of science or technology. In it I found a demand for attention to what scientists or engineers do, before creating distinctions around them; and I saw that Pickering had to a large extent done quite what I had initially intended to do. But I came to see this as a burden lifted from my shoulders - I now had not only the resources Pickering had provided in his book, but I also had the comments provided by his commentators; these combined could provide a much richer base of material for my examination than I could have managed on my own. Through The Mangle of Practice and the responses to it, I could find a clear formulation of the age-old question: "What is STS?"

The early work on the thesis was thus done with a focus on the major issues pursued by Pickering, the problems pointed out by his commentators as well as my own earlier ideas. The first chapter introduces one reputed hallmark of science which is said to distinguish it from technology - its ability to offer explanations. The history of the concept of explanation, in philosophy, in sociology, and in its importation into STS are briefly treated. As well, recent trends towards the rejection of the role of explanation in STS are presented. A first attempt at a thesis idea appear here as well - the suggestion that an account's conception of agency is directly related to its explanatory power.

This youthful enthusiasm was short-lived, however. I originally intended the second chapter to be an examination of Pickering's mainstream STS attempt to establish a new framework for understanding science and technology, as one which did not start with the questionable assumptions discussed in the first chapter, and which thus continued the arguments of the previous chapter. But upon a long study of Mangle and its commentary, I realized that the 'agency and explanation' problem was but an aspect of a larger problem. In Pickering's critics especially, I found a dependence on claims of external observation - that is, the claim that one's observation of another's observations might somehow avoid the flaws that one so clearly observes in the other. This then became the theme of the second chapter

Having been rerouted during the writing of this chapter, I began the third chapter equipped with a new analytical idea - the role of the claim to external observation. With this new focus, the third chapter looked at a prominent controversy in STS, the "Epistemological Chicken" discussions, which started in Andrew Pickering's Science as Practice and Culture, and which were continued in various other forms. These discussions served as a resource for an examination of the role of claims to external observation in STS. As the argument developed, it became clear that the concept of 'claim to external observation' was synonymous with the 'claim of objectivity' and with the 'ascent to abstraction' as well.

Through this discussion, I argue, we can observe much about the analytical foundations of STS; these foundations are seen to be challenged by the new approaches offered by Callon and Latour (in the Epistemological Chicken episode) and by others (Pickering) in general. I suggest an alternative approach, which while lacking the rhetorical force of appeals to objectivity, offers instead a reflexive robustness which might point away from 'Science Wars' to a new model of analysis in STS.

While reading Peter Galison's Image and Logic, a recent and notable contribution to the STS literature, I was disappointed by its discontinuities, its shifts of metaphors, and general un-edited-ness. Now, I know better - now I would like to convince my readers of the virtues of such 'lapses'. And I would like to believe that one virtue of this thesis might be just such 'lapses' and their uncanny ability to capture the process of thought. Just as STS rejects (attempts to?) the simplistic picture of progress that was long attributed to science, this thesis itself rejects the idea that one formulation is simplistically better than another. It exemplifies the finding that each step forward entails the closing of some possibilities and the opening of new ones. And while it is hoped that the movements of thought in this paper are in some sense improvements, yet it remains quite possible that there are aspects of the earlier formulations that are valuable, yet which have been lost in the 'improved' formulation. To rewrite this paper in terms of my latest inspiration would be to edit away the very sense of the earlier inspiration, as if the earlier thoughts were entirely superseded. Would this not be to reaffirm the myth of progress? How could it benefit me/STS to insist on practices which exemplify an idea that I/STS clearly rejects? While this argument should not be used to justify sloppiness, it is an argument that absolute clarity can not be the aim of STS.

Furthermore, because it is one of the aims of this thesis not to reproduce the problematic distinctions it reveals, this thesis cannot present itself as a statement of fact; instead, this thesis should exemplify the technoscientific - that is, that this thesis is not an uncovering of fact, but rather the articulation of a valuable and productive metaphor, one which can replace a now-threadbare metaphor. Old metaphors had their own ecology, but are now seen to have outlived it.

Thus it turns out, after the fact, that this thesis is an examination of the analytical orientation of STS, and of some problems implicit in that orientation. It was a struggle not to import resources from without - for there are abundant resources elsewhere, which I was far too tempted to import into this thesis. That these resources should be brought into the STS mainstream, I will argue vehemently, at another time. But I have found sufficient material within STS itself to make the problems evident. I hope that these arguments will create the ground for a re-orientation of STS, for a disruption of certain malformed disciplinary boundaries of STS.

Chapter Two: An introduction to explanation and agency

Software and STS: an introductory tale

The software was supposed to ship in the summer of 1998. Customers had been lined up, some advance orders taken. Specifications had been roughly drafted. Much preparatory work had actually been done. A programmer was hired in January 1998. It was expected that, with the previous version as an example and the rough specifications describing the desired improvements, that the programmer could finish the new version with 400 hours of effort. A bewildering succession of shipping dates had come and gone, most unnoticed and unbemoaned. Each final version engendered new questions, which engendered new final versions. The company was quickly going bankrupt; the president was forced to finance the company on his own credit cards. In the interests of saving his personal finances, the president directed frequent pep-talks at the programmers. The president enjoyed his oratory, the idea that his words would inspire heroic productivity, and hoped that his words might calm his own fears and somehow lead him to a satisfying long-term strategy. The programmers, contrarily, were consumed by their concentration on details and exhausted by the 'useless' hand-waving of the president, and grew discouraged at having sacrificed useful time for the meeting, and having surrendered the useful time afterwards because psychically numbed by the meeting. The programmer, lured into a simple, short-term, cut-and-dried project, saw it mushroom into a three year long, stressful, full-time job.

In January 1996, a person similar in many respects to the programmer above entered the Science and Technology Studies (STS) Program at Virginia Tech. This program calls for roughly two years of classes, the second year of which this student was expected to simultaneously propose and write a thesis, and finally defend that thesis. Allowing for typical delays, it was to be expected that he would finish the Master's thesis the summer of 1998. A series of deadlines for the completion of the Master's thesis had come and gone, as deadlines are wont to do. Each step towards a thesis proposal engendered a more sophisticated questioning, which then undermined the original idea, and engendered a new, more complex idea for a thesis. His funding had run out; his credit cards were carrying a heavy and heavier burden. The program's graduate committee issued evaluations, awkwardly generic, and attempting to be stern but encouraging. The student, sucked into a program whose livelihood seemed to depend on numbers, and who was becoming one of those numbers; his sense of humor was intact, however.

In neither case was the goal achieved on schedule. In fact, in each case the goal has yet to be achieved. And in fact, the student and the programmer are the same person. This then is alternately an account, from an STS perspective, of a software development project gone awry, and an account, from a software engineer's perspective, of an STS Master's program gone awry. Typical accounts would explain why schedules were missed - the problems encountered, the failures of the actors. A definitive account wrested from details, of mistakes made. The attribution of mistakes to actors. Accusations. But this account will attempt to avoid accusation, since mistakes are only seen as such retrospectively. Until difficulties are encountered, there is only the unknown. And in this account, it will not be thought a shortcoming of an actor that he or she did not know the unknown. Indeed, none of the actors in this account knew the unknown. Each actor had strategies for encountering the unknown; those encounters engendered new dynamics and new unknowns. In each case, the complexity of the project exceeded the expectations of all involved.

It is said trivially that there are facts and that science is an endeavor that discovers facts. And in this sense, a software program is not constructed like a fact. Anyone who develops software, and most people who use computers know about the tenuousness of computer programs. So one would never confuse software for a fact - there is nothing scientific about software. From this perspective, the account of software development has no relevance for accounts of science.

The Mangle of Practice

The various traditions in STS have been very successful in deconstructing the idea of scientific fact. Especially in the recent emphasis on studies of practice, STS has been able to illuminate the process of discovering facts as a heterogeneous and complex, social and technical endeavor. Through this opening up of the process of science, we come to see that the development of scientific fact shares much with the development of software. It would therefore seem that in STS there should be an openness to symmetrical accounts of software, theoretical physics, protein synthesis, stonemasonry, even bureaucratic organization. This was for me, the promise of STS. And this is what I found a glimpse of in Andrew Pickering's The Mangle of Practice, which explicitly aimed at this kind of symmetry. Yet Pickering's book was often received poorly, for whatever reasons. It seems that there are still boundaries here.

In The Mangle of Practice, Mr. Pickering turns away from thinking of science as scientific knowledge, and instead attempts to think of science as practice, that is, as human activity. As practice, it shares much in common with other forms of practice. Explicitly he attempts to avoid any distinction between science and technology. Indeed, he calls this a theory of everything, and his model of practice is formulated to capture the commonalities in all human activity. To accentuate this position, Pickering chooses his case studies from different fields - two from high energy physics, one from mathematics, and one from "technology". Through these case studies, Pickering attempts to elaborate a general framework for understanding practice, as processes of "modeling". This general framework invokes the role of contingency and temporality in all practice:

> The future states of scientific culture at which practice aims are constructed from existing culture in a process of modeling (metaphor, analogy) This ... is my basic idea of how existing culture predisciplines the extended temporality of human intentionality. ... First, the predisciplining of intent by existing culture is only partial. Modeling is an open-ended process with no determinate destination. From a given model--say a particular functioning machine--an indefinite number of future variants can be constructed. Nothing about the model itself fixes which of them will figure as the goal for a particular passage of practice. There is no algorithm that determines the vectors of cultural extension, which is as much to say that the goals of scientific practice emerge in the real time of practice.(Pickering, 1995, 19-20)

Reviews of The Mangle of Practice (which will be discussed in detail in Chapter Three) typically focus on the issues of explanation and agency. These reviews made it abundantly clear that STS is supposed to offer explanations, and visit criticism upon Mangle on the basis of what it can explain, and what it cannot explain. And while Pickering labels his approach a TOE - a Theory Of Everything, suggesting that he believes he is explaining everything, reviewers repeatedly stressed that a theory that explains everything can’t explain anything in particular. It is thus implicit in these comments that explanations of things particular are valued. And any approach like Pickering's that attempts to treat science and technology symmetrically and that makes some room for contingency at the heart of such accounts is flawed, because it cannot explain anything in particular. Since a theory, in common philosophical parlance, is supposed to offer explanation of phenomena, perhaps it would be better to see Pickering's TOE an attempt at general description. And his argument is then that such descriptions are the best we can hope for. Reviewers who expect STS to offer explanations find Pickering's book pointless. This then is a brief introduction to the problem of explanation.

Explanatory accounts are very particular accounts - they require a certain arrangement of narrative resources, among which one of the most important is the agencies that may play a part in the narrative. Explanations of scientific activity are supposed to lead to understanding - they in some sense map out the terrain surrounding a scientific finding, and show how the finding fits into that terrain, and why it must fit together in the way that is claimed. As such, they are accounts of scientific activity; and they also show how things could be otherwise - if we can vary some factors in the account, or if we find some factor to be erroneous, we can generate a critique. An explanation, an account, can function as a template for critical scrutiny.

Traditional Explanation and Agency

In traditional accounts of science, agency is not an issue - laws, and theories and facts were seen as coming from nature, things that were always there, waiting to be discovered. Accounts of science which could not be reduced to such forms were dismissable as non-scientific. The fact that human effort was required to recognize these laws and theories was to a point trivial - since these theories were thought to be implicit in nature, the discovery of all the laws of nature was thought to be inevitable.

Traditional accounts of science are thus tales of discoverers and their discoveries, but not of the work behind those discoveries. Traditional philosophical explanation is thus predicated upon the restriction of a weak agency to humans. In Hempel's classic account, an explanation is a deduction from observation and law; both the observation and the law are unproblematic, and universally accessible; any human actor could be substituted into the account and achieve the same result. There are thus no rogue agencies running around - there is the uniform behavior of nature, and the weak 'observing' agency of humans.

STS, Agency, and Explanation

Early STS positions, especially the "strong" programme" and the "Sociology of Scientific Knowledge" (SSK) programme contested traditional accounts of science, insisting on another form of agency at play in scientific activities. This agency, the agency of social forces - 'interests' and etc. - is seen to shape the outcome of scientific activities. David Bloor's ideas, as presented in *Knowledge and Social Imagery,* are the definitive statement of the strong programme. In that text, Bloor presents and argues for certain tenets of the Strong Programme. These tenets "define what will be called the strong programme in the sociology of knowledge" (Bloor, 7)

> 1. It would be causal, that is, concerned with the conditions which bring about belief or states of knowledge. Naturally there will be other types of causes apart from social ones which will cooperate in bringing about belief.
>
> 2. It would be impartial with respect to truth and falsity, rationality or irrationality, success or failure. Both sides of these dichotomies will require explanation.
>
> 3. It would be symmetrical in its style of explanation. The same types of cause would explain, say, true and false beliefs.

> 4. It would be reflexive. In principle its patterns of explanation would have to be applicable to sociology itself. Like the requirement of symmetry this is a response to the need to seek for general explanations. It is an obvious requirement of principle because otherwise sociology would be a standing refutation of its own theories.

The book figures as a decisive contribution to the STS literature, and established a perspective which later STS positions followed: It is explicit in the tenets that scientistic explanations are the goal of the strong programme's analysis. It is implicit that these explanations will invoke sociological causes - Bloor dismisses the traditional account's argument that the success of a theory is the result of its having found the correct description of the way the world is. Since reality is no longer a decisive determinant of the success or failure of a theory, explanations now require an additional element, an agency, namely sociological causes, that fills in the vacuum and provide for a decisive account. Finally, the (last) reflexivity tenet presents him with serious challenge, which he and the strong programme probably could not accommodate.

This demand for explanation with sociological causes necessarily required a different kind of explanatory account, one which includes some thorough analysis of social conditions and structures, and their effects on the conduct of the scientific activities under scrutiny. Thus the agency of humans and the power of social forces in crafting scientific theory became a central motif of STS. In their revisionist spirit STS accounts of science eschewed emphasis on individual heroic efforts, and instead emphasized the importance of social forces behind the individuals. Yet the form of explanatory account remains much the same - accounts are still framed in terms of causes, and are meant to definitively explain their subject - the difference being an insistence that the 'observing' agency of humans is filtered, distorted, even controlled by social forces. Notice that these explanations offer certain openings for revisioning - the scrutiny of social forces in scientific activities can generate abundant critique.

Later positions in STS have generally been responses to the position taken by the SSK school; these have importantly introduced new conceptions of agency, which accord to the material world some ability to resist our efforts, or some power to influence the outcome of even scientific inquiry.

In The Mangle of Practice, Pickering identifies agency as the ability to do things in the world: "...such radioactive sources are certainly instances of what I have in mind as material agency--they are objects that do things in the world." (Pickering, 1995, 9) Pickering insists that we "need to recognize that material agency is irreducible to human agency if we are to understand scientific practice: [Pickering, 1995, 53] He calls this position posthumanist; but nonetheless feels it important to capture the ability of humans to make plans and define goals. Thus human agency is special, because it is intentional agency.

Traditional accounts of science cannot be reconciled with this material agency; any change in the concept of non-human agency directly requires a serious revision of that traditional idea of human agency. We might then also accept that claims that there are really other kinds of agency active in the world, agencies excluded by the classic and SSK approaches, are also challenges to the explanations offered through these approaches. These explanations come to look 'cleaned-up', or 'simplified', once one has accepted that there are other agencies at play. Thus Pickering's idea of material agency demands an alternate form for accounts of scientific activity. Such an account must not be 'cleaned-up' - it must show these material agencies and the moves taken by particular actors to accommodate them. Pickering calls this a 'path-dependence' in the outcome of scientific activity; and he insists that social forces cannot account for this path-dependence.

One effect of Pickering's position is that this path-dependence is not easily reconciled with explanation and Pickering can not offer explanations the way his predecessors did; in a certain sense, he sacrifices explanation on the level of the specific activity for explanation on the level of the patterning of outcomes of activities. Another effect is a weakening of the critical enterprise established under the SSK programme - Pickering's claim that "the goals of scientific practice emerge in the real time of practice" is also the claim that particular aspects of practice cannot be second-guessed, and are thus beyond critical scrutiny. Lest we get too comfortable with the scientific heritage that Pickering is here defending, we should be reminded that Pickering has no account of intentionality - he relies upon traditional accounts of intentionality even as he puts under question traditional notions of agency.

Bruno Latour and Michel Callon together and separately have pioneered a more radical approach, one which contests such distinctions of agency and intentionality and the uses to which those distinctions are put. Their work suggests a symmetrical view of human versus non-human agency. Michel Callon, in the memorable article "Some elements of a sociology of translation: domestication of the scallops and the fishermen of St. Brieuc Bay" demanded symmetry regarding attributions of intentionality to scallops, to the fishermen who harvest them, and to the scientists who study them. Callon shows that the intentionality of the researchers, whatever it may be, does nothing to break the symmetry of actors:

> The scallops, the fishermen, and the scientific colleagues are fettered: they cannot attain what they want by themselves. The future of *Pecten maximus* is perpetually threatened by all sorts of predators always ready to exterminate them; the fishermen, greedy for short term profits, risk their long term survival; scientific colleagues who want to

> develop knowledge are obliged to admit the lack of preliminary and indispensable observations of scallops in situ. As for the three researchers, their entire project turns around the question of the anchorage of *Pecten maximus*.(Callon, 1986, 51)

It may be that even those who espouse such a proliferation of agencies still see themselves as offering explanations, and perhaps even on a level higher than Pickering's "Everything". But we must not let the complexities of this issue disrupt our tale of adventure any longer. And what is a "level" anyway?

We must recognize that Latour and Callon's position takes us even further afield from the notion of explanation - since agency is distributed evenly amongst humans and things, our ability to present an account that definitively resolves some activity into causes, or actions and responses is severely hampered. Instead, we can only offer an account which describes the configuration of some agents; it can not rule out the play of contingency in the outcome. An account of scientific practice is not an explanation if it insists that the observed outcome was a non-reproducible alignment of disparate agencies.

We can now see that arguments about agency are at the same time arguments about explanatory accounts and the narrative resources required for such accounts; indeed it should be clear that explanation requires an asymmetry of agency; and furthermore, to argue that there are certain agencies at play in one account is also a contestation of other accounts that do not attend to the issue of those agencies. This “explanation-agency nexus” is the issue at the heart of much debate in STS, especially the “epistemological chicken” debate between Harry Collins/Steven Yearley (defending SSK positions) and Latour (published in Science as Practice and Culture) and the recent exchange between Latour and David Bloor, from the journal Studies in History and

Philosophy of Science. And in both instances what is at stake is the status of STS as a critical enterprise.

The Appeal of Explanation

If it is accepted that the resources for explanatory accounts are also the ammunition for critique, then the unavoidable contingency resulting from the proliferation of agency suggested above has a chilling effect - if agencies are everywhere, then no critique can pretend to authority - each is partial to the extent that observes certain agencies and fails to observe others. From the perspective of those whose sense of order depends on scientistic accounts, it looks as if we have opened the floodgates of relativism - that henceforth, any crackpot idea can claim legitimacy; that the hard work of serious scientists will be considered no more important than the haphazard work of itinerant programmers. This conflict has its own fascinating history, the past decade's "Science Wars" being the most recent; and what is most notable in this episode were the attempts to avoid intelligent discussion, even to suppress discussion. This suggests that some felt that there was really something at stake, beyond the intellectual issue.

Even on the intellectual level, this championing of contingency feels unsatisfying; deep within me, and deep within STS still there lingers a scientism, a thirst for definitive and universal statements. And the universal statement has an undeniable appeal - when political decision-makers are calling on scientists to justify policy decisions on the most compressed schedules, affirmations of the particular seem to have no place. There, the clearest and most concise account is accorded the most airplay. If the aim is to intervene in the world, we seem to have come to a position that short-circuits itself. It appears that there are two currencies in a world that can't afford the time for careful deliberation - either you deliver facts, or you deliver products. And software engineers do sometimes deliver products. But what value is there in an STS programme that refuses to deliver facts? Like some cheap toothpaste commercials that use the testimony of toothsome nobodies endorsing the product, we position our product against those clad in labcoats who claim that this toothpaste was tested and found to kill germs dead. We position the pedestrian authority of direct and particular experience against the universal claims of scientific 'fact'. Even Pickering pitched his approach (in The Mangle of Practice) as a theory of everything. And even Latour, in "The Politics of Explanation" appears to explain explanation. And it seems to me that playing with the universal, even playing the universal against itself does nothing to evade the grip of the universal statement.

Which is all just long-winded way to say that this may be a waste of your time (in case you had some doubts). Perhaps also that the call to robust reflexivity threatens to lead STS into a pickle.

Explanation and agency in the literature: Philosophy, Sociology and STS

The literature touching upon the issues of explanation and of agency is legion, especially from sources in philosophy, where the topic of explanation has generated countless zillions of pages of text. The issue of explanation has also been of enormous importance in sociology, especially in responses to functionalism. The issues of explanation and agency, often intertwined, have produced some of the most intense debates in STS as well.

According to Wesley Salmon, explanation was not an issue in the sciences or in philosophy of science at the beginning of the 19th century. At that time, science was seen as a descriptive enterprise, and its merit was in providing accurate predictions. The idea that a scientific theory is supposed to explain why such predictions can be successfully made arose as a major concern especially with Carl G. Hempel and Paul Oppenheim's "Studies in the Logic of Explanation" of 1948. This is where the STS core typically begins its discussion of explanation. In Hempel’s Deductive-Nomological model, explanations are answers to questions of the form “Why P?”; a theory answers such a why question by a deductive argument based on a combination of universal covering laws and statements of empirical facts. Responses to Hempel’s formulations, and rejections and defenses of the importance of explanation have consistently been an important thread in the philosophy of science. Arguments about explanation prominently figure in realism/anti-realism debates. Realists claim that the explanatory value of theories is support for belief that the theories are true and the described processes and entities are real. Anti-realists claim that theories merely provide a description of known phenomena; these theories cannot be claimed to be true, and the described entities and processes may not be real. From this perspective, it is unimportant whether theories are explanatory.

A long succession of critics and proponents of the role of explanation in assessing scientific theory has followed. W.v.O.Quine’s naturalized epistemology is a rejection of philosophical explanation. Quine's thesis as expressed in "Epistemology Naturalized" is that there are no philosophical, extra-scientific truths that validate science. All questions and doubts are scientific and can only be answered or resolved by scientific means. While the target of Quine's attack is the topic of justification, his approach does touch on issues of explanation - the need to answer why questions no longer figures as an important goal of science. Wesley Salmon (1984) defends the importance of explanation, and attempts to couch his defense in new and sturdier conceptions of causality. Salmon believes that causality can be exHumed and recovered by attention to processes instead of events; armed with a robust idea of causality, we can evaluate theories for their explanatory value to the extent that they express causal relations of the processes involved. Philip Kitcher (1981) defends the focus on explanation based on his conception of a theory's ability to provide "explanatory patterns that can unify a wide variety of apparently disparate phenomena" (Salmon, 295). From this perspective, we can evaluate theories based on the value of the explanatory patterns they provide. Bas Van Fraassen, in The Scientific Image rejects the importance of explanation in philosophy of science, asserting that explanation neither requires nor provides anything more than predictive value. Arthur Fine’s “Natural Ontological Attitude” (NOA) allows that theories may be judged for their explanatory value. But Fine's Attitude denies "the existence of ... general methodological or philosophical resources for deciding" (Fine, PoS, 274) questions of truth. Thus Fine denies explanatory value is any argument for the truth of a theory. In this sense then, Fine short-circuits the discussion of explanation, and thus discounts the importance of explanation in science. In Science as Social Knowledge, Helen Longino presents an accessible survey of recent approaches to the issue of explanation in philosophy. She follows Fine in finding no general resources for assessing science, but pushes further into the realm of the social and the local - while Fine is content with a natural attitude, Longino defends a local, critical perspective grounded in the "engagement of intersubjectivity and a multivocal community of scientists in the resolution of theoretical ... and meta-theoretical disputes." (Longino, 199)

Regardless of the real status of explanation in the sciences, the ideal model of a science as framed in Hempel's formulation has become the paradigm for new sciences. The social sciences especially are dominated by the idea that theories are supposed to explain - that theories should answer why questions, over and above answering how questions. For example, the philosophy of science and the sociology of science both operate with the expectation of explaining science. In Functionalism, Jonathan Turner and Alexandra Maryanski discuss the controversies that surrounded functionalism in sociology, which revolved almost exclusively around the notion of explanation and the assumption that sociology's aim was explanation. Robert Merton's formulation of functional analysis includes the distinction between manifest functions (those which the agent(s) might have intended) and latent functions (those non-intended); this neatly divides those aspects of functional analysis which can be used in a causal explanatory account from those which can figure only in descriptive accounts. Merton's advocacy of the value of the concept of latent functions is thus an implicit critique of the limitations of explanation, and a championing of the value of description in science. Diesing's How Does Social Science Work presents an extended discussion of the problem of importing philosophical notions of explanation into social science. SSK accepts the goal of explanation, and has typically seen the aporia in accounts such as Kuhn's, Quine's and Fine's as expressions of the limits of philosophical explanation, and an invitation for sociological explanation. (Do I have to cover Kuhn, too?) This has engendered discussion in these fields as to whether theories must be explanatory, indeed, discussions as to whether theories can be explanatory. Joseph Rouse, in Engaging Science extends Fine's NOA, by merging a critique of SSK approaches with Fine's critique of philosophical approaches. Rouse disputes the SSK's willingness to accept the explanation as a goal; indeed, he rejects the aim of explanation altogether. Instead of global interpretation, Rouse champions local studies of practices (much as Pickering does) and descriptive cultural studies "which focus on the emergence of meaning within human practices (Rouse, 33) and which he sees as "a critical alternative to the social constructivist tradition"(Rouse, 39)(much as Pickering does). It should be hinted that Rouse's position is quite like that of Pickering, in general.

The attempt to extend "science studies" to "science and technology studies" has generated new questions - are studies of technology supposed to explain technology? Joseph C. Pitt's work in the philosophy of technology explicitly rejects the transfer of philosophical models of explanation to technology. But also in his arguments against "social critics" like Jacques Ellul, Pitt refuses not only explanations but also predictions of the results of implementing technologies. Like Pickering, Pitt allows for unavoidable contingency in the outcomes of technological processes. Furthermore, Pitt accords epistemic priority to technology before science, which suggests that contingency might also be a factor in scientific processes. (if perhaps his book Theories of Explanation were not sequestered in the library's special collection where virtually no one can peruse it, one might be inclined to offer comment upon it...) Bruno Latour explicitly targets the notion of explanation in the essay "The Politics of Explanation", in which Latour describes/explains that the desire for explanations over and above description is linked to the desire for (remote) control:

> A strong explanation becomes necessary when someone wishes to *act at a distance*. If you are in setting x' you do not need to explain it - practice and weak accounts will be sufficient. If you are away from the setting and indifferent to it, you do not need to explain it either - practice in the new setting x will do. If you are away and simply remembering how it was when you were in setting x' you still do not need powerful explanations - story-telling will do the job much better. You start to need a stronger explanation when you are away and still wish to act on the setting x'. Why? Because

> you now have to be in two settings x and x' at once. You need to be able to hold in the setting x some elements or features of x'. (Latour, 1988, 159)

An especially interesting approach to explanation in the realm of technology is presented in Latour's Aramis, an account of a fried French transportation system. Marianne deLaet's forthcoming paper - "The Zimbabwe Bush Pump" also presents a critique of explanation in the realm of technology, but one which contests Latour's network metaphors. In his interesting and frustrating book Towards a History of Epistemic Things, Hans Jorg Rheinberger both problematizes then skirts questions of explanation/description and science/technology in his account of the scientific activities of Robert Zamecnik and the pioneering research in protein synthesis conducted in his laboratory. Donna Haraway (at some distant point-in-time, in some coffee shop, the spell-check suggested "Faraway"; such occurrences are part of the richness of the experience of writing; and richness may be the most important characteristic of a writing like this), in Simians, Cyborgs and Women (especially in the Situated Knowledges section) presents a manifesto against the possibility of non-situated interpretation (which for her, includes philosophical explanation); indeed, the recent tradition of "cyborg" literature seems to me to be an attempt to refigure the notion of agency; according to my thesis, these should all at least implicitly deal with the issue of explanation.

The issue of agency has a shorter lifespan in the literature. The word 'agency' itself does not seem to have been in circulation until the 1980's, yet there were clearly some whose work laid the foundations for the new concept of agency. Certainly the pragmatists, C.S Peirce and John Dewey have created the backdrop for new conceptions of agency. Polanyi's concept of tacit knowledge figures as an important contribution from a philosopher to the concept of agency. There has for some time been in philosophy and sociology a focus on action; these theories of action seem to be oriented towards delineating and defending a human-machine boundary. It may be that this emphasis arose out of the very sci-fi idea of the threat of machine intelligence and this research seems to have peaked especially during the heyday of Artificial Intelligence (AI) research. Indeed some of the most intense philosophical discussions of the 70s and 80s revolved around such ideas. John Searle's <u>Intentionality</u> figures as one such contribution. But conceptions of 'agency' address different issues than conceptions of 'action' - perhaps it was the growing recognition that AI research was floundering, the decreasing perception of a threat from 'machine intelligence', and the increasing obviousness of the role of technology in our day-to-day lives that made it possible to recast the question from one concerned with differentiating human and machine action to one of understanding agency and its flows between humans and machines. Just as the question of explanation seems to divide STS, so too the concept of agency. Harry Collins offers commentary on action in <u>The Shape of Actions</u> following in the tradition of the philosophy and sociology of action, seemingly a rejection of the value of the new conception of agency. Collins and Yearley reject new conceptions of agency in their arguments in the "Epistemological Chicken" articles. And Bloor follows Collins and Yearley in his series of articles opposing Latour. But others in STS have pushed forward the concept; as mentioned above, issues surrounding agency arise audaciously in the work of Callon (sp. chk -> "Callow"), very directly in John Law, especially in his "Technology and Heterogeneous Engineering: The Case of Portuguese Expansion", and in Bruno Latour, especially in his "The Sociology of a Door-Closer". The various authors publishing about 'material culture', exemplified by Pickering, Peter Galison, and Davis

Baird have been important in highlighting this issue. In Image and Logic, Galison avoids treating the issue of agency directly, but when he claims that "Experiment and experimenter are bound together; their meanings necessarily change together" (Galison, 5), he is making a very Pickering-esque claim about agency. Indeed the whole book treats the issue of agency without ever using the term.

Donna Haraway's cyborg theorizing, especially in "A Cyborg Manifesto" attempts a drastic refiguring of human agency, in dissolving such distinctions as human/animal and human/machine through the concept of the human/machine cyborg hybrid. Drawing some inspiration from Haraway, the technology studies half of STS has spurred the most sophisticated work on agency, especially in the relation of human and machine agency. Gary Downey's fine The Machine in Me, not (yet) in the library's special collection, treats at length the issues surrounding attempts to differentiate human and machine agency, and argues for an alternate understanding of agency, one not premised on a human/machine distinction.

Explanation, Agency and Critique

As mentioned above, there is a vague thesis herein, that agency, explanation and critique are profoundly linked. Donna Haraway's work seems to implicitly deal with this linkage. More directly, the several discussions involving Bruno Latour and SSK personages (Harry Collins and David Bloor) explicitly treat this linkage. In these discussions, Bloor and Collins suggest that the Latourian move towards material agency is ridiculous. Latour's response is that such assertions are merely maintenance of conventional boundaries of narrative. My claim is that it is the science that SSK seeks to undermine that sets the model for narrative that SSK seeks to uphold. Thus, my analysis sees SSK seeing narrative resources as universal and therefore independent of science, whereas Latour argues that arguing about narrative resources is arguing about science. The point to be made is that Latourian/Pickeringian posthumanism and Harawayan cyborgism are similarly rejections of the resources that support the traditional model of critique.

As James Collier made clear in his dissertation defense, programmes which admit contingency and thus deny explanation "cannot motivate serious critique or progressive movement" and these, for him, are therefore failures. Yearley, in his review of Pickering, echoes the sentiment:

> The danger is that while this seems to allow us to comprehend everything it actually advances our understanding rather little. For example, since all science involves mangling, knowledge of the mangle of practice doesn't help us understand the outcome of scientific controversies. Pickering's approach would encourage us to say that the victor mangled more successfully, but this is to say little more than that the victor followed a victorious strategy. Comparing the contrasting fates of two rival experimentalists, Pickering is only able to say, "It just happened that the contingencies of resistance and accommodation worked out differently in the two instances." ... But Pickering does not help us to distinguish legitimate from inappropriate, wise from foolish mangling.[Yearley, 43]

It is in such demands for grounding the critical enterprise that the reification of contingent practice to "epistemic practices" to "knowledge" begins. This blackboxing of practice serves the political platforms of both attackers and defenders of science; furthermore, it is only through that concept of 'knowledge' that the sense of opposition between those two factionalisms can be maintained. From the perspective of Latour's thesis in "The Politics of Explanation", the emphasis on explanation over description suggests that these factions both wish to exert control over others, that is, "to act at a distance." If explanation is privileged over description, this suggests the goal is not so much to understand or persuade, as to control or critique. One of Donna Haraway's signal contributions to our wondrous planet has been her work in envisioning a different kind of critique, one that does not come "from nowhere", but which issues from an engaged and local position. As such, it does not attempt to suppress the contingencies of the particular; rather, it requires the analyst to position him/herself, and accept the contingent, situated vision from that position, eschewing the claim to have observed objectively. This kind of positionism cannot but blur the line between analysis and activism; as such, it seems to me a mistake to continue to call such work 'critique', when one has surrendered the claim to impartial judgement. Needless to say, this is an extreme challenge to academe; and it is clear from the quotes above and from the general response to Haraway, that there is a high level of discomfort with the rigors of this kind of positionism. It seems that STS is deeply attached to a traditional academic model of critique

This paper is thus a struggle against the injunction to explain, that is, to produce accounts that might provide the ground for the traditional (and illusory) panoptical critique. The attempt to control and critique requires a denial of contingency, a denial of the importance of practice; this demand for the blackboxing of practice arises not from understanding of practice, but from purposes external to that practice. In the resources I have assembled above, I believe there is a path towards the creation of an account of both writing a thesis and writing software that preserves the role of contingency in practice, and which thus refuses claims to non-situated objective observation. This thesis can be seen as an attempt to continue the work of Haraway in a new location, that is, in a particular program of Science and Technology Studies, and aimed at that specific context. It grew and continues to grow out of the continued allegiance to the critical programme here, in this place.

Chapter Three: The Mangle and its critics, mangled

Software and STS: The tale continued

In STS, it seems perfectly unreasonable to require that accounts of science and accounts of technology should take similar forms, since it is accepted that they are different 'things'. But I was convinced, especially by a few episodes in my STS education that this distinction was unnecessary, or at least, no longer useful. One such eye-opening experience occurred while studying "The Formation of Genetics", and focusing on the work of Monod and Jacob. Simultaneously employed as a software engineer, I found myself doing very much the same things as Monod and Jacob, while trying to isolate and ascertain the behavior of a few bugs in my software. Working in quite similar circumstances of not-knowing, I similarly created and refined systems of testing that would allow me to come to control the mysterious behaviors I encountered. I realized that the practices of scientists, like Monod and Jacob, might very well be indistinguishable from those of a technologist like myself. This led me to search for an approach in STS which unequivocally focused on the commonalities of science and technology, instead of starting with an unexamined distinction.

Andrew Pickering's The Mangle of Practice was the first book I encountered in the STS literature which was clearly attempting such a synthesis:

> ...while the representational idiom necessarily registers a sharp discontinuity in the move [from science] to technology...the performative idiom, in contrast, carries smoothly over into analysis of technology and production. (Pickering, 1995, 157)

It quickly became the centerpiece for my examination of the arguments that would have to be made in order to support my own project. Pickering's book is clearly a rejection of previous traditions of STS, especially the SSK tradition. And Mangle figures as an introduction of a new STS; reviews of Mangle can then be partly read as receptions of this proposed new STS. By reading reviews of Mangle I formed a clearer idea of what the deep commitments commentators have in STS; the criticisms of Pickering also point to their perceived goal(s).

In some ways, the practice of software engineering is treated as if it were a science, in the sense that it is thought that there are natural laws which govern the development of software. Many people spending many years typing randomly at many typewriters writing many pages of specifications, which purport to dictate exactly how a program should work. Similarly, people spend much time trying to schedule the production of software. None of this is surprising - businesses which must make their revenue on the basis of software must be able to plan for the future. But there is a profound tension in this - schedules and specifications are merely where such software projects begin, and they often end up in very different places. So too do thesis proposals and schedules go by the wayside, since proposals and schedules are merely where a thesis starts. In both cases, there is something that we might call 'practice', which stands between the non-trivial knowledge embodied in the schedule and specification, and the completed work. If one wishes to critique a software engineer's project by invoking the specification and the schedule as forceful documents, one can only do so on the basis of a claim that there is a foundation of something upon which the software is logically based, that is, that the 'something' between that foundation/specification and outcome is trivial. The practice of a software engineer is reduced to the following of a plan. Strangely, this is akin to certain STS accounts of science, in which critiques of science invoke foundational claims of the nature of 'society'. These STS analysts are employing the concept of society as a foundation for a specification of science. In both cases, what is rendered dismissable is exactly that which comes between what-is-said-to-be-specified and the actual outcome, that is, the 'practice' of a scientist or engineer.

The constraints of a software development project (like those of a master's thesis in STS) are almost entirely heterogeneous - which is to say, however you choose to draw boundaries between software engineering and not-software engineering, the constraints come mostly from outside. Given the time and the resources, one can do virtually anything in software. But it is not the only expertise of a software engineer to do 'virtually anything in software' - a software engineer's expertise also lies his skill in matching the value of the requirement to the cost of its realization. (It should come as no surprise that graduate students require a similar expertise.) The more closely a project follows the specification and the schedule, the more that project functions as a realization of the plan; and the less the project diverges from the plan, the less one learns about the plan, and about what variations might have been available. Thus, another part of the practice of a software engineer is an exploring outside the specification, in search of possibly valuable variations.

My initial software project started in earnest with an RFP for a data collection and analysis suite, including specifications. The authors had the clear idea that specifications could dictate important parts of a software project. They invested an enormous amount of time in specifying what was to be done, and especially, how that was to be accomplished. In practice however, there are other considerations besides schedules and specifications. There are always other documents and pseudo-documents to consider, there are tensions, squabbles, disagreements, desires to punish, and especially, changes. The value to us of completing their specification as written was negligible. The value to them of our original software plan was also negligible. Both parties were forced into a process of discovering the real product. And a specification could well have been written, one that integrated the best of both original specifications. But even this would have only been a starting point - only to the extent that they did the actual work could all the issues be discovered. The figure of Borges' map which was coextensive with the land it mapped is instructive here.

The Mangle of Practice, examined

These considerations of time and constraint lead us back to Andrew Pickering's The Mangle of Practice which (among other things) critiques traditions of philosophy of science and sociology of science for their elision of practice, and thus their ignoring the contingency involved in doing stuff.

In Mangle, Pickering characterizes these traditions as invested in a representational view of science:

> The representational idiom 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. (Pickering, 1995, 5)

These accounts ignore what scientists actually do, to peculiar effect :

> Within the representational idiom, people and things tend to appear as shadows of themselves. Scientists figure as disembodied intellects making knowledge in a field of facts and observations.(Pickering, 1995, 6)

Pickering is here following the 'laboratory studies' tradition of STS, which had already drawn attention to the irreconciliability of accounts of scientific practice with the representational accounts of those practices' outcome. Pickering follows this STS tradition in rejecting such representational accounts; but Pickering aims to advance beyond the laboratory studies tradition by his move to "rebalance our understanding of science away from a pure obsession with knowledge and towards a recognition of science's material powers."(Pickering, 7) It is through attention to the practices of scientists - the performance of procedures, the manipulation of material and machines, the mistakes, failures, re-tunings, calibrations and etc - that Pickering hopes to develop this idea of science's material powers; he attempts to elaborate "...a basis for a performative image of science, in which science is regarded as a field of power, capacities and performances, situated in machinic captures of material agency" (Pickering, 7) Pickering explains his concept of material agency thus:

> The world is continually doing things, things that bear upon us not as observation statements upon disembodied intellects but as forces upon material beings. Think of the weather. Winds, storms, droughts, floods, heat and cold--all of these engage with our bodies as well as our minds, often in life-threatening ways. The parts of the world that I know best are ones where one could not survive for any length of time without responding in a very direct way to such material agency--even in an English summer one would die quite quickly of exposure to the elements in the absence of clothing, buildings, heating and whatever. Much of everyday life, I would say, has this character of coping with material agency, agency that comes at us from outside the human realm and that cannot be reduced to anything within that realm. (Pickering, 1995, 6)

Through this conception of 'material agency' Pickering hopes to characterize the tendency of the world to resist out plans. This then is a "dance of agency" - between human agency and the material agency of the world that which resists our agency, and forces us to change our plans:

> 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 manifest itself. Does the machine perform as intended. Has an intended capture of agency been effected? Typically the answer is no, in which case the response is another reversal of roles: human agency is once more active in a revision of modeling vectors, followed by another bout of human passivity and material performance, and so on. The dance of agency, seen asymmetrically from the human end, thus takes the form of a dialectic of 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. (Pickering, 1995, 22)

But each bout of material agency is framed by bouts of human agency. Pickering thus characterizes them as emergent resistances, resistances that never could have been encountered had not the ongoing dance of agency led to the point of this resistance appearing. In this dialectic, outcomes emerge - not out of the plans of scientists, but out of the details of their responses to the material agencies with which they struggle, and the machines' reaction to the scientists' responses. The "machinic capture of material agency" is the outcome, when the instruments have been successfully tuned to capture/subdue the rogue material agencies, and produce the desired predictable behavior. Along with emergence comes contingency, since the final outcome is a path-dependent result of the specifics of the dialectic of resistance. This category of material agency is a necessary part of Pickering's move from the representational idiom to the performative idiom, and is the key that lets him advance beyond SSK's deterministic shortcomings. We must note however, his attempt to maintain a distinction between intentional and non-intentional agency. And it is possible that he herein re-constructs the distinction he thought he was dissolving-

> intentionality - a term that I use in an everyday sense to point to the fact that scientific practice is typically organized around specific plans and goals. I find that I cannot make sense of the studies that follow without reference to the intentions of scientists, to

> their goals and plans, though I do not find it necessary to have insight into the intentions of things. (Pickering, 1995, 22)

Pickering claims this approach is a theory of everything, which seems to suggest links to the concepts of explanation, as discussed previously. It is not clear how Pickering sees his project; it seems most tenable to see this as a descriptive model, which simultaneously contests the possibility of an explanatory theory.

Responses to Pickering's The Mangle of Practice, and commentary.

Many critical responses to The Mangle of Practice question Pickering's major themes. David Chart claims that the Mangle is not an analysis of scientific practice:

> Indeed, he explicitly claims that these events just happen, that no one could predict in advance what resistances would arise, and what accommodations would be made. Such an account constitutes neither an analysis of science, nor one of scientific practice. Pickering claims that his account eschews explanation, but provides understanding. I don't think it does either. As an analysis of science, the book fails. (Chart, 480)

For Chart, analysis must provide some framework for explanation. Similarly, understanding requires the banishing of any sense of contingency from that thing. And to the extent that Pickerings 'Mangle' does not provide understanding, it is not an analysis.

Steven Yearley similarly demands something more from accounts of science:

> But in the end, the extreme even-handedness which Pickering views as the strength of his approach obliges him to conclude only that things just happen. He doesn't want to give social and psychological factors undue importance, nor to give exclusive weight to the natural world; all factors are changed and reshaped as they undergo mangling...The danger is that while this seems to allow us to comprehend everything it actually advances our understanding rather little. For example, since all science involves mangling, knowledge of the mangle of practice doesn't help us understand the

> outcome of scientific controversies. Pickering's approach would encourage us to say that the victor mangled more successfully, but this is to say little more than that the victor followed a victorious strategy. Comparing the contrasting fates of two rival experimentalists, Pickering is only able to say "It just happened that the contingencies of resistance and accommodation worked out differently in the two instances." (Yearley, 43)

Yearley concludes that "Pickering's mangle helpfully directs us towards analyzing the practice of science; it is however, much less helpful as a model of how to do that analysis." (Yearley, 43) We gather from this that the role of analysis in these domains is to "help us understand the outcome of scientific controversies" (Yearley, 43) In the context of a review of Mangle, this demand for some general account of scientific controversy and for general guidelines as to "how to do analysis" seem to be an outright rejection of Pickering's notions of practice and contingency. Where Pickering clearly draws a line at the use of analysis as an aid to understanding the complexity of the actors efforts, Yearley demands more. By Pickering's research, Yearley's demand would appear to be a metaphysical or ideological concern. It seems that Yearley is eager to trivialize the actual work done by the actors, in favor of a general understanding of "the outcome of scientific controversies". It occurs to me that this follows the pattern of mathematical problem solution - using an established pattern to generate answers, all the while neglecting consideration of the work of those who actually established that pattern. Trevor Pinch's response is quite similar.

> ...it seems as if the mangle is silent on what is surely the key performative issue for scientists--whether they are performing so as to produce claims about the world which will be accepted as having veracity (Pinch, 143)

Pinch thus insists that STS analysis is supposed to observe science and adjudicate the standards for veracity.

Yves Gingras questions Pickering's performative/representational distinction, and argues that it does no work. He suggests that the only value in scientific practice is in the representational accounts of that practice:

> ...despite the performative injunction, there is no argument in his case studies showing that anything really emerges in time, and, in all his examples, 'resistances' come as effects of the actions of scientists. ... I think his discussion of 'emergence' is purely metaphysical and does not really contribute to our understanding of scientific practice... (Gingras, 323)

Here again we encounter the demand for a general account of "scientific practice", not surprisingly, since Gingras is straightforward concerning his disinterest in Pickering's performative thinkings. And while Gingras accuses Pickering of explaining nothing at all, he does recognize Pickering as offering a concept with broad extenuation when he appeals to Aristotle's authority in a rejection of Mangle - "it may be worth recalling Aristotle's observation that there is an inverse relation between the extenuation and the intention of a concept." This echoes Yearley's concern that that which allows "us to comprehend everything...actually advances our understanding rather little." For example, a theory that all perceptible objects have color is quite unexceptionable; but it tells us very little about how different things are differently perceived While there are of course, all kinds of metaphysical presuppositions underlying such a statement as Aristotle's (i.e., there cannot be general mechanisms which generate a variety of phenomena (such claims were made about evolution before mechanisms of evolution began to be understood)), yet it seems to me that Aristotle's claim is more rightly targeted at Chart, Pinch, Yearley and Gingras, though: In demanding a general explanatory account of "the outcome of scientific controversy" or "scientific practice", these fine authors are demanding a concept with broad extenuation, and not at all worried about sacrificing the intention. Not surprisingly, Gingras imagines that all of Pickering's performative-idiom could be replaced by representation-idiom - he presents an 'illustrative' example of a blind man in

> a room containing some furniture here and there... Adopting Pickering's point of view leads us to describe his movements in the following way. The goal of the blind man is to advance straight in front of him. After a few free steps, he stumbles on an object that resists him so that he must stop advancing in his chosen direction. He then accommodates that 'resistance'...by choosing to move sideways--and this choice 'just happens' in the real time of practice. After a few steps sideways, he decides to move forward again--and of course we must insist that this new choice 'just happens'--and meets no resistance for his next four steps, until he falls after having met a new resistance...After a while, he constructs a mental map of the room and locates(in his own coordinate system based on direction and number of steps) where he met the various resistances. He also notes that he meets these resistances every time he walk to a given place. He concludes that they are thus time-invariant 'resistances'. The blind man never knew the presence of these material agencies that resisted him in advance, but only when he tried to move in certain places. So, in a sense, those resistances emerged only when he happened to be in those places. But their persistence in time was such that he could not conclude that material agency really emerged in time except in the trivial sense that he perceived and felt nothing before meeting the obstacle. (Gingras, 323-324)

Gingras concludes :

> Why cannot we accept post-facto reconstructions, when they are useful in predicting other similar instances? For though the blind man certainly postulated the existence of the furniture in the room only after having interacted with it, it served him well in his future movements to take account of these persisting resistances. He could even use his map to suggest to fellow blind men entering the room to take care not to hurt themselves in certain places where there are material agencies waiting to emerge on them. In short the persistence in time of the resistances offered by material objects largely deprives the notion of emergence of any significant content.(325)

It may be that Gingras' example provides the key to understanding this set of responses to Pickering, and so a more detailed analysis of the example may be instructive. Gingras' treatment of the blind man in the room implicitly invokes an omniscient observer (the reader) who can verify the blind man's progress through the room, and who could in principle verify the blind man's map. The reader's role in Gingras' example is akin to that of scientists watching rats navigate a maze, through the maze's glass cover. In all cases, there is the image of external observation - there is always an appeal to an external observer, who can see everything the actors can see, but more. It is only the reader who can verify that the objects in the room remain even after the blind man has left. (how perverse that Gingras accuses Pickering of choosing a Berkeley-an position...) Could the blind man know his map was complete? Could he know that other blind people could navigate successfully using his map? To what extent could he observe his own observing? Could he know that there were not worse hazards in the room, from which only his mishaps with minor obstructions saved him? Gingras' arguments stand only upon the supposition of the implicit external observer, in this case, the reader. In science (and technology), we are interested in the investigation of unknown (or at least, not well-characterized) phenomena, whether it be sub-atomic particles, or experiments in software. These are investigation in which there cannot be any such external observer. In personal terms, there is no one observing my practice of software development who could assure me that my map (my software) is a reliable guide for anyone else's navigations. To the extent that I have applied my self in extending my navigations, I can offer tentative assurance. But Gingras seems to think that an account of my practice could be written, "a post-facto reconstruction" which while ignoring the details of practice, could communicate the competence that is tightly woven into that practice. If we eliminate the external observer in Gingras' example by adding the premise that all involved persons including the reader also were as blind as our protagonist, the example devolves to a man's tale of a strange room. Auditors of this account would then be free to question his account and to doubt the validity of his map. There could then be no appeal to the way the room actually is, because there is no observer who could see any better than the blind man. And it would only be our protagonist's competency in experimental navigation, his skill in drawing a map, and his ability to convince others to use it, that would lend credence to his account. This would then be a better analogue for scientific practice.

While Gingras claims that Pickering's performative idiom does no work, it seems rather that it is Gingras' representational account that does no work. Gingras example of furniture in a room certainly appeals unfairly to our own (sighted) experience of encountering furniture, and hence his argument for the intuitiveness of the furniture's persistence sounds unimpeachable. But consider if Gingras chosen a fair example, say, travel in the fifth dimension (the specifics are clearly not important - what is important is that the reader not be given any unfair advantage over the actors - we must be as blind as the actor in order for the account to really capture the significance). Suppose a fellow explorer came back with tales of his adventures in the fifth dimension. Then, upon our own embarking (or even better, our young 12-year old son's), we might perhaps consider his map of this strange dimension. But our willingness to trust his map would probably have a lot to do with our estimation of the sophistication of his practice - we would ask him to present some account of his practice, beyond the map itself, to convince us that his map was the result of diligent effort. Gingras instead would ask to us authorize that map blindly, without consideration even of our guide's sense of proportion or ability to draw. It is the guide's competencies alone which support the map he draws, and the map, and other accounts which do not treat the guide's performance, convey nothing of these competencies.

Gingras' argument, like those of Chart, Yearley, and Pinch makes the implicit claim that such accounts, premised upon external observation are not only valid, but that they are what we seek; and much more, that it is the job of those providing accounts of technoscience to attempt to fashion their accounts in such a way as to provide this sense of external observation. These would then furnish general explanatory accounts of "the outcome of scientific controversy" or "scientific practice", and provide us with maps, which we have no business questioning.

The motif of the external observer is repeated through many STS accounts, as well as many philosophical and sociological accounts of science. Such accounts are based on the claim that the scientist's observations are demonstrably imperfect, and that a demonstration and a less-imperfect observation could be provided by this external observer. (The sense of this argument may seem to contradict the sense of the previous paragraph - but the argument is that the contestation of the guide's account can only take place on the terrain of a discussion of his practice, not in the airy realm of representation.) This schema of contestation, premised upon some claim to greater objectivity provided by external observation leads to a delicious reflexive hell, where one can contest the most skilled observer by claiming to be an external observer. Armed with outside resources, the latter can on this schema, observe better than the former. Since there is always the possibility of the positing of an extra-external observer, and an extra-extra-external observer and ..., this schema devolves to a regress of accusation, settled only be the most forceful claim as to who is blinder. It is here suggested that this regress is defused when we refuse to privilege the external observer over the internal observer. A more sophisticated theory of observation is needed, one which observes the limits of observation, without leading to new regress.

While the reviews above reject Pickering's entire perspective/reorientation of STS, there were many reviews of Mangle that were to some extent sympathetic, yet nuanced in their criticism of its flaws.

Michael Lynch, while sympathetic to Pickering's ideas of material agency and posthumanism, makes a similar criticism similar to those above: "My dissatisfaction with the book has to do with the way the mangle is so all-inclusive that it describes everything and nothing."(Lynch, 810); Lynch develops that criticism:

> ...Pickering's theory of everything is a verbal gloss that applies almost too easily in one historical example after another. Moreover, the central terms of the theory - the mangle, and the generalized conception of agency- take on lives of their own. At times, Pickering endows the mangle itself with agency: "It is the mangle that determines in time what scientific machines will look like, what scientists will believe, how they will conduct themselves around those machines ,and how (if at all) these pieces and others will hang together and relate to one another". In context, Pickering can be understood to be alluding to a complex nexus of interacting judgements, determinations, problems, negotiations, and improvisations, but it is not clear what is added by placing all of these together in a unified field of agency. I believe that it is highly misleading to suggest that a unified "something" pervades and directs an open-ended field of actions, contingencies, determinant, and judgments. Pickering may be right to be dissatisfied with static divisions between human agencies and material causes, but I believe that a more differentiated conception of practices would be preferable to a hypostatized treatment of agency. (Lynch, 811)

While the criticism of applying to everything does not seem problematic (as discussed above, this is a trivial claim, unless there is serious consideration of the underlying terrain (the practice)), Lynch is perceptive in catching Pickering on agency. Other reviewers saw similar problems - Theodore Schatzki, in his review for Studies in History and Philosophy of Science, doubts Pickering's posthumanism:

> In the neck of the academic woods through which I travel, 'posthumanism' means various things, most prominently (1) denying that human beings are special and unique, (2) 'decentering' or fragmenting the human subject, and (3) assigning human agency to the networks and assemblages that compose people. Pickering defends none of these doctrines. Unlike, for instance, actor-network theory, to which his account bears admitted likenesses, he treats humans as the unified loci of an agency whose intentionality distinguishes it from the agencies of material things. (Schatzki, 160)

In these claims, Schatzki can hardly be denied - Schatzki has caught Pickering really wanting to be posthumanist, but incapable of making the necessary sacrifices (and perhaps, not even understanding what the necessary sacrifices are). Hans-Jorg Rheinberger, while generally sympathetic, also reacts to Pickering's views of agency -

> If I see it correctly, in the perspective of the mangle, the idiom of representation belongs to the antihumanist discourse of scientists. However, Pickering's language of performance belongs, not to the posthumanist, but to the humanist discourse of sociologists. So there is a discrepancy here between the language of performance and the perspective of posthumanism. To be consequent, Pickering's mangle would need a language of action on the level of things, on a par with the language of action on the level of actors. Taken together, he would need a language of transhumanist (why posthumanist) agency. For the time being, such language remains a desideratum. (Rheinberger, 166)

These objections oblige us to examine Pickering's treatment of agency in a more thorough way. As discussed above, Pickering's performative account is supposed to be a remedy for the representational account's tendency to make "people and things" appear "as shadows of themselves." These representational accounts typically posit a world, out there, waiting to be discovered, but doing nothing to hinder our discovering; and this makes discovery a very peculiar thing. What is it that separates the moment of not knowing from that of knowing? What is it that is preventing us from knowing now what we will know in the future? Discovery marks the transition from not-knowing to knowing, but it says nothing about how that transition was achieved. Typical accounts of discovery credit scientists' ability to look closely and to think clearly; but because the world is a passive entity, these abilities are also the *only* thing that separates knowing from not-knowing. Discovery also has retrospective effects - when today the quark is discovered, then it is also suddenly realized that the quark has always existed, and thus yesterdays's nonquark knowledge is shown to have been only an incorrect, and now dismissed theory. Furthermore, discovered things are always inevitable, after the fact - in retrospect, the things discovered are obvious (they are, of course, the way the world really is.). One can only respond to one's previous ignorance with - *How could we have been so blind? All of that is so clear now...* It would therefore seem that there can be no rational account of discovery; and one effect of these peculiar properties of 'discovery' is the claim that if previous scientists had been truly scientific, they would have known then what we know now. Such accounts put us in the position of admiring the unclear thinking and poor observation of previous scientists. Discovery therefore involves a bizarrely precipitous distinction between knowing and not-knowing, such that previous versions of knowledge are obliterated by the transition.

Pickering's performative account is an attempt to rescue us from this bizarre position. Where in a representational account it appears that the only thing that prevents the perfect realization of our intentions is our own lack of understanding, Pickering's performative account adds the concept of 'material agency' as that ability of the world to resist us and thus require our efforts in order to achieve understanding. Discovery now requires not only thinking and observing, but also doing - and this extra requirement allows us to make some sense of our predecessors 'failures' - it was not previous scientists' shortcomings, it was the resistance the world offered, and their inability to craft machines and techniques to subdue that agency.

A Re-evaluation

Yet the comments above (Rheinberger, Schatzki) suggest serious problems with Pickering's mangling of agency, and the special place he reserves for human intentionality. Pickering introduces intentionality as "a term I use in an everyday sense to point to the fact that scientific practice is typically organized around specific plans and goals" (Pickering, 1995, 17); furthermore, he suggests that "One has to recognize that scientists usually work with some future destination in view, whereas it does not help at all to think about machines in the same way" (Pickering, 1995, 17) We must note that by this Pickering does not mean anything so trivial as - 'humans turn machines on; this makes something happen, which could not have happened without the human turning on the machine "to see what happens"'; intentionality can only mean that outcomes actually reflect the original "specific plans and goals", or restated, that outcomes are directly related to the "future destination" in the mind of the scientist.

Pickering is appealing to something that sounds familiar and plausible; yet as with everything in STS, we must consider whether this 'intentionality' might be an effect of certain techniques of narrative reconstruction, rather than being some fundamental property of humans.

Now Pickering's material agency is supposed to represent the resistance we encounter in our interactions with the world, and its ability to make us do things we could not have anticipated - that is, to modify our plans. Intentionality can then only be the claim that whatever modifications to our plans, the plans are still the plans of the scientist; material agency is thus reduced to something that makes things only a little difficult, something which does not significantly affect our plans. So it turns out that the world does not really resist our efforts; in such an account, the world is like a boxer paid to take a fall.

Pickering's stand on intentionality is a contradictory position - while he wishes to maintain some sense of the contingency involved in human efforts through the concept of material agency, he simultaneously reaffirms the primacy of traditional accounts of those efforts, that is, accounts which produce the effect of 'human intentionality' by purifying those accounts of any real contingency.

Another effect of Pickering's position on intentionality is that it leaves us with a singularity at the point of 'discovery', that is, the space between the moment of not knowing and the subsequent moment of knowing. At the moment of not knowing, the source of contingency cannot be specified - it could reside either in the things, or in the observation of things, in the theories which are supposed to be true, or in the experimental setup based on those theories. Material agency here does seem to capture something of the world's resistance - things are not working as we had planned, we are reconsidering our assumptions, our plans are in limbo, there is uncertainty.

But at that future point in time in which we have actually come to know, 'intentionality' is established and plans are seen to have been fulfilled. Material agency has effectively been banished; 'material agency' had been until this moment that which prevented us from knowing; but we now know, and material agency no longer plays any role. Material agency is thus only a narrative construct that bridges the gap between the instant where something was knowable but not known and that instant when something is now known.

We see here that Pickering has abandoned the performative idiom, stepped out of his dialectic of agency, and is now offering an interpretation of the dialectic. This is nothing more than a return to the representationalist idiom, which it was his 'specific goal' to reject. Pickering's demarcation of intentional agency from non-intentional agency is part of the representational idiom -- and 'Material Agency' is a codeword that signals the attempt to translate some aspect of the contingency characteristic of the performative idiom to the representational idiom. But in the interpreted categories of the representational idiom, the category of 'material agency' is not comfortable either - it simply makes no sense to attribute agency to objects. Once we have adjusted our representational categories to accommodate new objects, these objects are left cold and lifeless - what was once material agency, the resistance of the world to our own agency, is now no resistance at all. We see in hindsight that what we thought was agency was nothing but our lack of understanding. Pickering's material agency is a halfway position, uncomfortable in either side, and not sturdy enough on its own.

To return to the language introduced earlier in the chapter, we find that Pickering's claim for human intentionality includes a claim for the objectivity of observation - that is, the claim that the contingency that arises in our interaction with the world is solely due to the world, and not to our role in that interaction - that there is contingency in our observation is denied. Pickering's view of intentionality thus reinforces the traditional notion of the scientist as rational agent, which has the effect of restoring the separation between scientist and software engineer - the scientist as rational agent is not reconcilable with the engineer: where an engineer is one who builds into his work the constitutive contingency of the world - (a well-engineered solution is one which constitutively includes the contingencies of the world and of the actors), Pickering's intentional scientists banish contingency entirely. Pickering has thus redrawn the separation between science and technology.

In The Mangle of Practice, Andrew Pickering is proposing a model for the construction of STS accounts, one which is supposed to treat science and technology equally. His desire that these accounts might emphasize the importance of practice led him to attempt a 'performative' account, which conceptualizes the resistance the world offers scientists as 'material agency'. But Pickering's position on intentionality introduces a new version of the science/technology distinction he was attempting to dissolve, and he unfortunately reinstates that distinction. There is another sense in which Pickering's accounts defy his own intent - these accounts clearly cannot be purely performative accounts - they are indeed representational accounts, as any account must be. Pickering is really attempting to create a hybrid performative/representative account, one which maintains a sense of the performative by means of the category of material agency. But has he really given us resources for a better representation? Given the discussion of material agency above, it appears that Pickering has given us the traditional account, with the addition of the paradoxical concept of material agency. This shows, in a more subtle way, the continued and seemingly inescapable attachment to the objectivity of observation.

Chapter Four: Critique and Normativity, in Software and in STS

> Uncertainty is and remains a condition of structure. Structure would cease were all uncertainty to be eradicated, because structure's function is to make autopoietic reproduction possible despite unpredictability. A necessary measure of uncertainty always comes into being when structures are formed, and one can -- not without a certain malicious enjoyment -- observe in security-obsessed structural formations like bureaucracies and legal orders how uncertainty multiplies when bureaucratization and regulations increase. (Luhmann, Social Systems, 288)

Introduction

Having programmed computers for a rather long time, I have witnessed the rise and fall of many new systems and new methodologies that would promise breakthroughs in accuracy or productivity. Of these there are of course many; and as in other disciplines, novelty of approach is of paramount importance. In many ways this echoes the recent history of STS, in its period of competing methodologies, when each new author presents new frameworks for doing the work of the discipline. This reflects a scientistic streak - the belief that there can be a methodology which provides a recipe for objectivity, or at least, approximate truth. We have a double parallel, two disciplines both under the sign of scientism whose experiences lead each to attempt to reject that scientism, only to attempt that in a scientistic mode. This scientism has currency not only in various philosophies of science, but also in popular (especially political) debate, and also in the politics of academe, where departments (for example, political science and sociology) strive to establish themselves as 'scientific' by focusing on generating facts. This is a symptom of a particular view of science, one in which science is distinguished from other endeavors by a focus on the products of scientific practice, together with an insistence that these products have some philosophical weight. In this view of science, the products of science exist in a realm where also dwell things like 'objectivity' and 'truth'. For the sake of this chapter, let us refer to this view of science <science>; it is important to distinguish this view from the practices of scientists, which we awkwardly continue to refer to as (plain) science. Scientists may or may not hold the view of science that I refer to as <science> - it is probably not at all necessary to their work. While I am quite sure there are some scientists who believe in <science>, yet I am also sure there are others who do not. Indeed it might be claimed that science means different things to different scientists, especially as their methods and approaches vary by discipline and specialty. And in that sense, this paper is not concerned with science per se. It is concerned with <science>, and the effects of that view on both the fields of software engineering and science and technology studies.

STS has blazed a path in making the gap between science and <science> apparent; it has therefore had to contend with its own scientism - this is the question of reflexivity in STS. Likewise, the field of software engineering accepts the inevitability of flaws in its systems - it accepts that the goal is to minimize their impact and to maximize the flexibility to revise, update, or improve software. Software engineering has become a discipline that attempts to rigorously constrain error. At the same time, there are proposed new models, new frameworks, which claim to guarantee some improvement in practice, some systematic reduction or elimination of error. And these claims may have merit - new systems can improve practices. But I will argue that it is clear from the history of software engineering that new systems complexitize their domains; that is, they create new realms of error that arise out of those same technological advances the systems offer to deal with previous realms of error.

(These new realms of error then invite those inclined to believe that there can be a final abstraction to conceive new systems to manage that error; these new systems complexify their domains, generating new realms of error, engendering new systems...). The complexification thesis I am presenting here suggests that new systems and new abstractions constitute new ways of observing; and they therefore also create new forms of blindness.

It is not at all a question of rejecting new systems, not about rejecting ascent to abstraction; it is instead the insistence that abstractions come at a cost; and that cost must be balanced against the advantages that abstraction provides. But it is also that the costs of new systems are not fully known until that system has been intensely exercised, and that balance probably can not be known except in hindsight. It is telling that software engineers are frequently tantalized by the prospect of redoing their software - it is only when a system is understood (that is, employed as a tool) that one comes to understand its costs. (But those costs are not the system's; they are an observation of the system, and each observer may well have their own estimation of the costs). This then is an argument for immanent critique; but at the same time, it is an argument that what is immanent is up for grabs.

This thesis aims not to condemn such abstractions; it aims instead to undermine their dominant and to-some-extent unquestioned position in our intellectual approaches to the world, and thereby make room for other, more down-to-earth approaches in our intellectual economy.

Yet this analysis itself cannot claim to operate on the level of a higher abstraction - it cannot ascend to authoritative claims; it must instead be self-contained;

Niklas Luhmann, in a similar predicament, insists that the only way to proceed is to mount an analysis which does not claim any clearer observation than that of its subjects - "...this logical form is the foundation of productive analyses that can resolve their own paradoxes...This analysis therefore claims for itself the characteristics of its object of study..." (Luhmann, Observations on Modernity, 1)

So, this undermining cannot claim to come from a higher position, a position of more objectivity; rather it comes from a particular situation, a situated observer, a small 'laboratory'. It invites comparison to other observation; it foregrounds the circumstances of its production, and it succeeds or fails based not on its rightness or wrongness, but on its ability to offer successful dealings with the world. It may or may not have relevance outside the 'laboratories' where it was produced. (And this document is not any more objective for having foregrounded itself...)

Its value lies in the intensity of its observation of its particular site; in this it is no different than the value it attributes to the efforts of scientists and engineers - Monod's groundbreaking work, a finely-crafted piece of software, both reflect their craftors' intense observings, and manipulations of technologies to tune and improve those observings.

It is suggested that <science> has no place in such a cosmology if we follow Winch's point - "Science, unlike philosophy is wrapped up in its own way of making things intelligible to the exclusion of all others. Or rather, it applies its criteria unselfconsciously; for to be self-conscious about such matters is to be philosophical." (Winch, 102-3) Winch is here describing what I have chosen to call <science>. A <science> that claims to un/dis/cover the world objectively is fundamentally committed to the argument for higher abstraction, to external observation. While this definition of

<science> may be disputed, I offer that it is only intended as a working distinction, intended to foreground a particular sense of science - its claim to objectivity and the poisonous influence that idea has had. In general parlance, the phrase 'to be scientific' connotes a rigorous pursuit, a systematic examination, but most distinctively, it connotes 'finding the right answer'; and it is in this particular sense that I see the twin poles of this paper - software engineering and STS, dangerously entwined.

If this sense of objectivity is excluded from a definition of science, then it seems to me that <science> devolves to science, and neither picks out anything in particular - the activities and products of auto mechanics, engineers, scientists then all exist on the same level.

It may be argued that the word science should be understood as picking out certain communities of inquiry; specifically those communities that maintain a very high level of inquiry through certain conventions of publication of results and public critique of those results. But there are communities not-thought-to-be-scientific which practice similar conventions, especially internet communities of software engineers. And it seems in the present day that even in scientific communities, those standards of publishing and open critique are falling by the wayside; only those communities with no place in the 'modern' economy of knowledge continue to observe those conventions. Which is to say that there may have been a point in time where 'science' did denote certain communities with distinctive practices. It does not continue to do so.

It is left to the next chapter to argue that STS is best served by a dismissal of <science> as a constitutive category; a philosophy of technology is instead needed. The 'history of science' can then be interpreted as a particular chapter in the history of technology.

Software

I see a parallel in the fields of software engineering and STS, especially since I simultaneously engineer software while I write this thesis. And at the same time that my studies of STS suggest approaches in software engineering, my work with software also suggest approaches for STS. To the extent that STS abandons its preoccupation with objectivity and critique, it becomes something similar to software engineering; except that STS may never produce something that will be bought and sold in any volume, and will never generate an IPO. Indeed this thesis arises from my incomprehension of the purported substantive differences between the two.

Through a brief examination of the history of object-oriented programming, I hope to illustrate the 'complexification' thesis mentioned previously- that is, the claim that new systems and new abstractions constitute new ways of observing; and they therefore also create new forms of blindness; more simply put, it is the claim that new systems introduce new realms of error. This is by no means intended as a proof of a thesis - it is instead the observation of a problem; what follows is an attempt at an alignment of resources to address that problem.

But before we dive into the deep end, we shall first dive into the shallow end, and attempt to introduce object-oriented programming languages. Programming languages are very precise syntactical things - so-called higher languages have rather small syntax sets, of English-language commands. These commands typically signal the performance of some action on some specified variable. Computerized interpreters parse the language syntax, and generate the long chains of 0s and 1s upon which the computer functions. 'Procedural programming languages' perhaps represent the most primitive form of higher-language programming - such programming consists of the manipulation of numerical variables and of character strings; all concepts must be rendered in terms of numbers, arrays of numbers, or character strings. 'Object-oriented programming languages' arose in the 70's and 80's as a response to and an evolution out of 'procedural programming languages'. Object-oriented programming introduced various structural operations that would facilitate the creation of object structures - such objects encapsulate the behavior of some perhaps real-world object, allowing us to think about that object in the programming realm in similar ways to how we might think of the real-world object - for example consider a satellite - an object construct might encapsulate orbit behavior, uplink and downlink interfaces, etc. This partitioning allows other programmers to treat the satellite object as a black-box (of sorts), that is, as if it were a satellite, without worrying about the details of its implementation. Thus, object-oriented programming facilitates and encourages a partitioning of projects that reflects something of their real-world counterparts. Furthermore, object-oriented constructs allow one to create chains of similar objects, that is, those that share some attributes; it provides structures for the sharing of those attributes, minimizing the repetition of common features. As such, it encourages a certain analysis of the things that will be involved in a programming project, an analysis that attempts to establish hierarchies and relationships, so as to take advantage of the benefits of object-orientation. For example, one could create a generic satellite object, which specialized satellite objects include, but to which they add their own specific behaviors.

Note that one can attempt to perform object-oriented programming with a procedural language; in fact, the object-oriented language in question below, C++, was itself written in C, a procedural language. In the same way that one could hammer nails with a pen, but would probably quit; whereas one would continue quite happily if one had a hammer; so would one be much more successful performing object-oriented programming with a suitable tool. And that thing on the other side of the hammer that helps you pull out nails when you mess up.

In the publication The C++ Programming Language which christened the C++ computer programming language, Bjarne Stroustrup introduces the language with the retiring claim that it was "designed to make programming more enjoyable for the serious programmer." (Stroustrup, iii). Afterwards he declaims the value of the "object-oriented" abstractions his C++ grafts onto the traditional C language - "A programmer can partition an application into manageable pieces by defining new types that closely match the concepts of the application....When used well, these techniques result in shorter, easier to understand, and easier to maintain programs." (Stroustrup, iii) The modesty of these claims is refreshing; many other authors made much less restrained claims. But note the ambiguity of the statement "when used well" - one will look in vain for the specification of how to use the language well. Indeed, this simple statement has engendered a wealth of subsequent publication which try to specify "how to use the language well". A few pages later the author defends the flexibility the language allows (Note: this is actually a continuation of a long-running discussion about flexibility of programming languages in software engineering/computer science), and presents the following argument -

> The connection between the language in which we think/program and the problems and solutions we can imagine is very close. For this reason restricting language features with the intent of eliminating programmer errors is at best dangerous. As with natural languages, there are great benefits from being at least bilingual. The language

> provides a programmer with a set of conceptual tools; if these are inadequate for a task, they will simply be ignored. For example, seriously restricting the concept of a pointer simply forces the programmer to use a vector plus integer arithmetic to implement structures, pointers, etc. Good design and the absence of errors cannot be guaranteed by mere language features. (Stroustrup, 7)

This is indeed refreshing, especially in light of the claims of authors to follow; the last sentence especially expresses something profound - the author's belief that the structures of programming cannot guarantee optimal outcomes. This does not prevent other authors from proposing new structures in and around the language which purport to guarantee good design and the absence of errors.

Khoshafian and Abnous' book Object Orientation introduces itself this way - 'Object orientation' promises to deliver more functionality through simple, easier to use computing environments:

> The need for object orientation therefore is rather simple: Users are demanding more functionality from their computing systems. They are asking for simpler, easier-to-use computing environments. Increased functionality and easier-to-use computing environments come at a price, however. They demand more complex underlying systems (There are no free lunches) This means more lines of code to be organized, managed and maintained... This book is about the object-oriented concepts, analysis and design techniques, languages, databases, user interfaces, and standards that attempt to solve and satisfy the computational needs of the 1990s. Object orientation provides better paradigms and tools for: • Modeling the real world as close to a user's perspective as possible. • Interacting easily with a computational environment, using familiar metaphors. • Constructing reusable software components and easily extensible libraries of software modules. • Easily modifying and extending implementations of components without having to recode everything from scratch. (Khoshafian, 2-3)

To summarize the authors' claims, object orientation promises a modularization of computing resources that will allow end-users as well as programmers to quickly tailor existing resources to their specific purposes. While these ideas clearly echo Stroustrup's earlier claim, the authors are here engaged in an attempt to specify "how to use an object-oriented language well". But it may be worth remembering Khoshafian and Abnous' caveat - "There are no free lunches", which suggests that the additional complexity they impose on computing practices so as to ensure proper modularization - that additional complexity itself requires its own "how to use ...well" book.

By the mid-1990s, much work had been done with C++, and many problems (poor design and presence of errors) had been encountered. These encounters led to books which attempt to address those problems and systematize solutions. Grady Booch's Object Solutions: Managing the Object-Oriented Project presents new techniques, designed to address the problems experienced in the formative years of object-oriented programming. Booch is forthright in confronting the excesses of early promises: "Early adopters of object-oriented technology took it on faith that object-orientation was A Good Thing, offering hope for improving some ugly aspect of software development" (Booch, vii) Booch then summarizes what he has learned:

> There exists an ample and growing body of experience from projects that have applied object-oriented technology. This experience -both good and bad - is useful in guiding new projects. One important conclusion that I draw from all such projects is that object-orientation can have a very positive impact upon software development, but that a project require much more than just an object-oriented veneer to be successful. Programmers must not abandon sound development principles all in the name of objects. Similarly, managers must understand the subtle impact that objects have upon traditional practices. (Booch, vii)

This discussion is interesting both for its modesty and for its recognition that new programming structures require different institutional structures. Indeed, Booch's book is predominantly devoted to project management issues - his distillation of the book is "To be successful, an object-oriented project must craft an architecture that is both coherent and resilient and then must propagate and evolve the vision of this architecture to the entire development team" (Booch, 28)

It is clear in this statement that one of the requirements for 'using object-orientation well' is a complexification of the management of software development.

We also see new software systems proposed - new abstractions around the original object-oriented abstractions, addressing the shortcomings of the previous generation. For example, the problems that arose with attempts to use objects developed for one project in another (i.e. Abnous and Khoshafian's reusable software components) concerned the brittleness of the developed programs with regard to later changes. Any modifications required by one program would necessarily affect the objects if used in other programs. The promises of reuse, as presented by Abnous and Khoshafian turned out to be more difficult to achieve than expected. In <u>Adaptive Object Oriented Software,</u> Karl J. Lieberherr sets out to solve this problem. In this book, the author examines the problems of reusability involved with object-orientation, and proposes new meta-structures that focus on reusability. His method

> enhances and complements other object-oriented methods ...by lifting object-oriented software development to a higher level of abstraction by considering entire families of object-oriented designs and programs. (Lieberherr, xxv)

And it is later stated straightforwardly, "The higher the level of a programming tool, the clearer and simpler are the programs"(xxvi). Yet it should be noticed that this new abstraction --"adaptive object-oriented software" -- exists only to address the problems with the previous abstraction --"object-oriented software". Reusability was supposed to be a benefit of adopting object-orientation; now it is unblinkingly stated that it requires yet another abstraction. This brings to mind an anecdote about the Golden Gate bridge (possibly entirely false, perhaps 'approximately true')- the story is that as soon as the painters finish the task, they go back to the other side and start all over again. For software though, we always get the promise that this last round of paint will be the last; then we will be there, done. I'm impressed by the Golden Gate bridge effort - it does not try to shift or hide the costs in the delusion that somehow, a higher level of thinking will overcome those costs - it simply accepts those costs.

> Object-oriented programs are easier to extend than programs that are not written in an object-oriented style, but object-oriented programs are sill very rigid and hard to adapt and maintain. A key feature of most popular approaches to object-oriented programming is that methods are attached to classes...or to groups of classes. This feature is both a blessing and a curse. On the brighter side, attaching methods to classes is at the core of objects being able to receive messages, different classes of objects responding differently to a given message, and the ability to define standard protocols. On the darker side, be explicitly attaching every single method to a specific class, the details of the class structure are encoded into the program unnecessarily. This leads to programs that are hard to evolve and maintain. (Lieberherr, 6)

In the first generation, the attachment of methods to a class was touted as the major benefit of C++, the tool which promised great new advances; in this argument, Lieberherr undermines those claims for the value of C++. This is not a logical contradiction; it is instead a paradox - there is a regress or oscillation or paradoxification of abstractions happening. Which is just to say that new structures create new contingency and thus generate new practices, which generate new methodologies. One would think that with all the historical consciousness of previous languages, the failed promise of the first generation of object-orientation, and the rejection of the major tenet of the first generation, the second generation would be more reflexively aware of its own claims. But Lieberherr is optimistic without restraint:

"Adaptive program focus on the essence of a problem to be solved and are therefore simpler and shorter than conventional object-oriented programs." (Lieberherr, 16) This simplicity is a result of this approach:

> Conventional object-oriented programs consist of a structural definition in which a class structure is detailed, and a behavioral definition where methods attached to the classes in the class structure are implemented. Likewise, adaptive programs are defined structurally and behaviorally. What makes an adaptive program different is that class structures are described only partially, by giving a number of constraints that must be satisfied by a customizing class structure. In addition, behavior is not implemented exhaustively. That is, methods in an adaptive program are specified only when they are needed, when they implement an essential piece of behavior. Constraint-based partial specifications can be satisfied by a vast number of class structures which, when annotated with essential methods and automatically generated methods, denote a potentially infinite family of conventional object-oriented programs. (Lieberherr, 7-8)

This brings us to the seeming-contradiction - something that is simpler and shorter than its predecessors, yet capable of generating a potentially infinite family of such predecessor programs. Some readers might think that the author has miscalculated somewhere. And it is of course possible that the author is simply inflating his claims in the interests of selling books quickly. But it seems to me that this is a persistent theme in both software engineering and in science and technology studies - that somehow there is a meta-description which actually simplifies. What is clear in software engineering, from this brief tour, is that such simplicity is gained only at the cost of complexification elsewhere - witness Booch's emphasis on organizational structures; it is embarrassing that none of the other books even consider the organizational costs of their simplification/complexification exercise.

Wolfgang Pree, in <u>Design Patterns for Object-Oriented Software Development</u> gives us an approach technically similar to Lieberherr's. He too focuses on metapatterns. But his language is more precise, for my purposes:

> Both reduced internal complexity of software systems through well-thought-out structuring and better reusability are viewed as key factors in improving software quality from a technical point of view" (Pree, 1)

Pree's language foregrounds certain distinctions he is employing - '*internal* complexity', '*technical* point-of-view'... Yet the line between technical and non-technical is conveniently never drawn. It is likewise never clearly decided what is internal, what is external to his systems.

Pree's language helps to make it clear that these books are implicitly systems theories; to the extent that they remain implicit, they lead to a neglect to examine some of the important distinctions upon which the system is based. This makes them bad systems theories. So this is bad systems theory, pretending that reduced internal complexity is an unproblematic good - that the increased external complexity does not generate potentially worse problems. It can not be simply claimed that reduced internal complexity justifies increased external complexity - I suggest that the history of software engineering is a demonstration of this. What is clear is that new abstractions may promise reduced internal complexity, yet often require many potentially costly iteration before that can even be achieved.

The presence of evolutionary metaphors in software engineering are another illustration of this problem - these typically suggest that evolution is a process of complexity elimination - which suggests that their model cannot be evolution as it relates to nature; such mottoes as 'Reduced complexity through structure', and other such simplistic appeals to evolution ignore the realization that evolution does not controvert entropy- evolution is in general, a process of complexification - an increase of internal order at the expense of increased external disorder.

The pattern is clear in software engineering-systems theories that appeal to the ideal of the highest abstraction, and which all the while neglect the rising costs of external complexity. I suggest that this pattern can be observed quite similarly in STS. In some corners of STS, it too is believed that a new abstraction can be free of its own blind spots.

Epistemological chicken

STS has been a very successful programme, even with all its internal divisions, some of which will be displayed shortly; it has accomplished the most intelligent and sustained critical studies of science of all such attempts. Especially the work of Bruno Latour and of the various SSK programmes, and specifically the work of Harry Collins and David Bloor have powerfully displayed the distance between science and <science>, and persuasively argued for the need for re-examination of our simplistic notions of science. And Harry Collins' work continues to make the case more and more compelling. So it is particularly interesting that the arguments between Collins and Latour in the volume Science as Practice and Culture, (the "Epistemological Chicken" debate) should display the issues discussed in the previous two sections, but arising within STS. This debate involves these and other notable STS scholars, all who agree on the rightness and impact of the inquiry STS has generated; yet they disagree to such an extent over the consequences of those accomplishments that it is quite difficult to see that they agree on anything.

The Epistemological Chicken discussion involves Collins and Yearley's article

"Epistemological Chicken", Steve Woolgar's response "Some Notes about Positionism", Michel Callon and Bruno Latour's response "Don't Throw the Baby out with the Bath School!", and Collins and Yearley finish "Journey into Space". Steve Fuller later added commentary in the volume The Disunity of Science with an article entitled "Talking Metaphysical Turkey about Epistemological Chicken, and the Poop on Pidgins" David Bloor picked up where Collins and Yearley left off, with "Anti Latour" in the journal Studies of History and Philosophy of Science. Latour responds in the same issue with "For David Bloor... and Beyond: A reply to David Bloor's 'Anti-Latour'", Bloor responds, again in the same issue, with "Reply to Bruno Latour". This literature frames question of objectivity, systems theory, of internal complexity and external complexity, in similar ways to the previous discussion of software engineering.

To summarize, Collins and Yearley (henceforth CandY) begin the episode with their championing of the technique of alternation - the sociologist's task of suspending the observance of the rules of his own culture when observing another, so that observation is not poisoned at the outset by unexamined assumptions.

In deriding Latour's approach as 'semiotic', Collins makes a clear statement of his commitments:

> The net effect of treating the whole world as a system of signs is, however, to come full circle back to the prosaic world of John Doe. As we will explain, the philosophy may be radical, but the implications are conservative. Where there are no differences except the differences between words there are no surprises left--no purchase for skeptical levers to shift the world on its axis. (Collins and Yearley, 303)

Regardless of the merit of the claims against Latour, the comments signal the claim that the analysis is not supposed to be prosaic - while certain aspects of the world are prosaic, John Doe for sure, scientists probably as well, the work of sociologists can not be prosaic. Furthermore it is claimed that the implications of the philosophy can be used to condemn that philosophy - an extremely anti-scientific position, it should be said. That philosophy should be guided by a commitment to establish "skeptical levers to shift the world" is putting the egg before the cart, if it is assumed that there can be such levers, and that the aim of this enterprise is to delineate them and instruct in their proper use.

CandY's claim is that there can be achieved some sort of ascent - that there is some sort of observation that avoids the very kinds of limitations that it observes in others' observations. (For the purposes of this paper, I will refer to this claim for perspective-free observation as the ideal of external observation) This 'ideal of external observation' is very prominent in CandY's statements in comparing the knowledge of sociologists to biologists: "Sociologists don't know anything in quite this way; they only know how it is to know"(301) But elsewhere, CandY dispute that there could be such levels -

> The discourse analysts...had failed to appreciate the universal vulnerability of knowledge making to the methods of SSK. They also failed to understand that social analysis of science does not show the science to be wrong; by the same token, discourse analysis does not show that something other than SSK is right. In all case validity is the outcome of social negotiation; the absence of social negotiations is not a condition of validity. (Collins and Yearley, 304).

--so we can only conclude that the validity of SSK is validated only by social negotiation. CandY's' 'universal vulnerability' suggests that even SSK's knowledge making has no special claim to validity, and that therefore it cannot claim for itself a distinction of level. But the paper is replete with such claims - CandY speak of methods of knowledge:

> Scientists, technologists, philosophers, builders of intelligent machines, and social

> scientists all struggle in their different ways to make progress along the route. The builders of intelligent machines have one method; they try to model human beings; the scientists, technologists, and philosophers work hand-in-hand using another method: it is called natural science. What sociology of scientific knowledge provides is a third method, no longer subservient to accounts of the work of scientists and technologists and the stories of philosophers but rooted in a special understanding of social life.(Collins and Yearley, 321)

CandY finish by claiming of SSK: This is a "new way of comprehending the world" (Collins and Yearley, 321) This re-erects the levels they have claimed cannot be justified. Collins is claiming for his meta-alternation more than just the value of a new scientific method - this "new way" is not prosaic; it is an ascent to another level.

CandY attempt to escape this contradiction with a pragmatic appeal: "In the absence of decisive epistemological arguments, how do we choose our epistemological stance? The answer is to ask not for the meaning but for the use." (Collins and Yearley, 308) It is thus CandY's claim that 'use' is less problematic than 'meaning'; this is claimed without any justification - there is no discussion of whose 'use', nor about the possibility of differing perceptions of the value of the 'use'. I suggest that CandY's belief that 'use' is less problematic than 'meaning' can only mean that CandY assume some kind of foundation to which their concept of use contributes unproblematically. This sounds suspiciously like an appeal to an enlightenment narrative; and there is some evidence to support this claim - CandY chastise Latour for giving "no purchase for skeptical levers to shift the world"(Collins and Yearley, 303); this is of course nothing other than the claim that, for CandY this STS enterprise aims at establishing such levers. But such levers require pivots - there can be no claim to shift the world without an accompanying claim that a pivot has been established. And for CandY, this foundational pivot is not acknowledged. As suggested above, it would seem that this foundation is none other than an enlightenment narrative; it is these same narratives which also anchor the authority of science. And it is clear that CandY are comfortable appealing to these narratives. It would seem that CandY are really just demanding a second-order application of that narrative.

In "Anti-Latour" David Bloor takes a similar position; nonetheless it is distinctive enough to merit discussion. Bloor seems to knows what sociology is, and knows that its foundations are secure. Bloor uses terms like social context as if a 'social context' was a real thing. Even as he contests scientist's statements as to what is real, Bloor insists that 'social context' is real. While he says of Millikan "It is better to say that he observed something he attributed to, and explained by, a postulated entity he called 'an electron'" (Bloor, 93), Bloor himself refuses to question his use of 'social context'. Again and again, Bloor reiterates the real-ness of social context:

> Of course, the real concern will not be with individual, psychological responses as such, but with those responses as mediated by a collective understanding, with its shared traditions and conventions. (Bloor, 92)

In so doing, it seems that Bloor is claiming that his sociology is more scientific than science itself. Sociology, in his hands, is an exact science, fully capable of dissecting the practices of scientists of lesser sciences:

> The important point is to separate the world from the actor's description of the world. It is the description that is the topic of enquiry, and the proposed separation is one of our resources. This is all just another way of saying we must respect the distinction between the object of knowledge and the subject of knowledge. (Bloor, 93)

Curiously though, that separation if applied to Bloor's own position would problematise his claims too. More importantly, this demand for separation defies sense - when engineers pioneer a solution, when scientists 'make a discovery', they have left the arena of shared convention - this is instead where new conventions are forged.

As with CandY, Bloor, in the midst of his penetrating observations of science, demands a scientific, foundational position, from which to issue critique; he insists that sociology is that foundation. By doing so, he is insisting that sociology exists on a higher level, that it is an observing system with no blindnesses.

There are many responses to the problems posed by such positions. Michael Mulkay makes a simplistic and direct commentary - in The Word and the World he demonstrates rather convincingly that such claims to undermine the authority of science from STS can be met with third-order counter-claims, which would undermine the authority of STS. Steve Woolgar and Malcolm Ashmore attend to these problems in their investigations of the limits of reflexive narrative. But most significantly (for this paper at least), Bruno Latour specifically attends to these problems, and he attempts to defuse the paradox by problematizing some of the assumptions which lead to the paradox; perhaps this is the reason why he is the target of the criticisms mentioned in previously.

Discussion

It may be objected that these controversies are old, and that the problems are so visible that no one could fail to avoid these problems. Nonetheless, STS is stuffed full, like some jelly donuts, with claims of sturdy skeptical levers. The problem is not at all limited to SSK; indeed I see similar problematic commitments in other STS positions as well, and encounter them repeatedly in discussions with other students. Acknowledging that these sorts of controversies can be interminable, it seems to be the response of the

STS community to pay some limited attention to these disputes, then to let them go. There is a healthy 'pragmatism' in STS - even in the face of unresolvable theoretical disputes, we can go on with our work. Yet this refusal to examine the question of metaphysical commitment is a failure - when STS attempts to intervene (which may be a praiseworthy effort; see Conclusion...) it faces this quandary: because STS has successfully deflated <science>, it can no longer attempt to ground its positive programme in foundational claims. And it is perhaps because it is STS that has made the case so well, that it is the greater failure for its unwillingness to pursue the consequences.

This may be a confusing position, as if we can pick and choose which pieces of Collins' and Bloor's approaches are acceptable - to accept their case studies as persuasive, even while rejecting their critical position. But I think that many works in STS, are argued in two-piece fashion - a first move to problematize previous understandings, followed by an attempt at a re-description that addresses the concerns raised in the first part. And while I think that STS' first move has been quite successful, it is the problems with STS' attempts at re-description that have motivated this thesis.

To demonstrate that this is not a problem for SSK only, but that STS continues to demand 'sturdy skeptical levers' consider Steve Fuller and Jim Collier. Fuller follows CandY and Bloor in demanding that STS adopt a normative stance in analyzing science; when Fuller faults the descriptive approaches of STS, he begins by

> ...observing that, protests to the contrary, a normative perspective is already embodied in the interpretive strategy most commonly used in science studies. The strategy imputes to scientists competency in whatever they are trying to do. What exactly the scientists are trying to do is still a matter of dispute among philosophers and sociologists. However, the social epistemologist challenges even this minimal notion of

> rationality because it prevents scientists from being accountable to a standard not of their own choosing and hence provides no opportunity for rethinking the ends of knowledge. (Fuller, 1992, 390)

It is implicit in this that it is the duty of science studies to question the competency of scientists and furthermore, that 'what scientists are trying to do' is something in the domain of philosophers and sociologists (and presumably, science studies) to decide. It should not be surprising that he does not discuss who would then decide what philosophers, sociologists (and presumably science studies) are themselves trying to do, nor whose duty it would be to question their competency. Fuller amplifies these comments:

> From the social epistemologist's standpoint, however, the descriptive turn has emasculated the normative dimension of science studies and in the process has limited the field's potential for radical critique and revision of knowledge enterprises. (Fuller, 1992, 391)

Fuller bolsters this statement with a quote from Marcuse; this foregrounds his sympathies with the Frankfurt school's revisionist marxism. And while I may be sympathetic to Marcuse's and Fuller's aims, yet it also remains that a critique of science may also be a critique of that tradition of progressive thought. The claim that science studies should attempt to 'rethink the ends of knowledge', without serious consideration of that possibly shaky ground seems to be a very dubious claim. I suggest that Fuller expects his intended audience to share the perspective that science studies is a discipline aiming at "rethinking the ends of knowledge" It might then be expected that anything which rejects that aim cannot count as science studies - since these figure as basic assumptions, it should not be surprising that these authors must attempt a rejection of any other voices in STS whose position questions generates a critique of those basic assumptions. This, I argue is a persistent theme in the internal squabbles in STS.

It is interesting then to read Fuller, in his later article from The Disunity of Science, in which he defends Latour's position in the 'Epistemological Chicken' debate. Fuller now seems to understand the problem, and attempts to save Marx by relativizing Marxist critique. Now, Fuller ushers critique in the back door, by appealing to democratic principles - oversight of science is now motivated by the observation that "the knowledge enterprise is most creative where boundaries are moving, because of constructive border engagements" (Fuller, 1996, 182) It is left as a problem for democracy to determine what is creative, and what is more creative, and what is creative enough. Yet for Fuller, this democratic imperative is fleshed out with market metaphors-

> Let me conclude with a methodological image. I envisage the texts of say, Marx or Freud as such border engagements, the conduct of cross-disciplinary communication by proxy. They implicitly represent the costs and benefits that members of the respective disciplines would incur from the revolutionary interpenetration proposed by the theorist. For example, in the case of Capital, the social epistemologist asks what would an economist have to gain by seeing commodity exchange as the means by which money is pursued rather than vice versa, as the classical political economists maintained. Under what circumstances would it be worth the cost? Such questions are answered by examining how the acceptance of Marx's viewpoint would enhance or restrict the economist's jurisdiction vis-a-vis other professional knowledge producers and the lay public. More specifically, it would require looking at the audiences that took the judgment of economists seriously at the outset, Marx's potential for affecting those audiences, and the probable consequences of audiences' acting on Marx's proposal. (Fuller, 1996, 185-186)

Fuller calls this an "interpenetrative approach" which examines the "transferability of Marx's principles" to other locales, and he emphasizes his interest in this as an experimental programme. And while democracy is a potentially great technology for dealing with serious problems, it is crucially important to flesh out the mechanisms of democracy. Fuller's democratic programme rests on nothing other than a market whose terms are cost and benefit. And while this certainly reflects the flavour of political debate of the past few decades, is it really how STS wishes to model intervention? In software engineering, such an approach may not present any serious problems. But STS for better or worse concerns itself with serious ethical and moral issues; when it comes to justifying intervention about serious issues in the world, will market metaphors do?

In Fuller's defense this approach at least avoids the trap of aiming at objectivity; that is, this experimental programme does not aim at establishing the truth, not even implicitly nor on some higher level (as his previous approach did). Because it is not premised on the rhetoric of objectivity, perhaps it might be better seen as an exercise in problem solution. And indeed, democracy might be best seen in this light, as a technology for solving politico-social problems. And in this sense, Fuller has achieved some internal consistency.

Jim Collier's dissertation treats of similar topics, following Fuller especially in demanding that STS reclaim a normative stance:

> In this dissertation I offer a framework for identifying, understanding, and adjudicating among knowledge claims made about scientific practice and their relationship to propositions about the world. I will refer to knowledge claims about scientific practice and their relationship to propositions about the world as 'meta-scientific claims'. I will analyze and develop criteria for determining the validity of meta-scientific claims and translating them from their disciplinary origins into a coherent epistemology of scientific practice. Meta-scientific claims are descriptive and normative statements about the conduct of scientific practice, or about how scientific practice ought to be conducted from various disciplinary interdisciplinary and contextual perspectives. I will argue that meta-scientific claims are distinct from natural or social scientific claims, therefore the means for determining the validity of these claims is also distinct. If however the claims forwarded in science and technology studies (STS) simply reduce to current empirical practices and disciplinary

> categories, no compelling reason may exist to study science outside traditional disciplinary boundaries. (Collier, 1)

Clearly for Collier, it is the uncontroversial goal of STS to adjudicate claims; and anything short of sturdy skeptical levers can only be a failure. It also seems that he wants to argue that it is only the meta-scientific claims that STS offers that provide any value - for Collier, the only other option is the 'reduction' to disciplinary perspectives - it is unthinkable that interdisciplinarity might offer instead the power of deflation, that interdisciplinary perspectives might defuse the idea of a meta-scientific claim altogether.

Collier opens his dissertation with a quote from Randall Collins in which Collins thinks it

> ...desirable to set up a higher standard of explanation of what scientists are doing than simply to offer interpretations. Why should any particular sociological interpretation be accepted over any other interpretation? If we are just privileging the force of our own rhetoric, or focusing attention on some sets of conditions and ignoring others, the best we can hope for is to stay away from intellectuals who do not already agree with us...we must be able to show why our explanations are better than other explanations. To do this, we need to be more comparative and more systematic (Randall Collins, Social Epistemology, 1992 v6,n3,267)

While this is nothing other than the central problematic of STS, both Collier and Collins think that we have only two choices - either the challenge to set up a higher standard, or instead the lukewarm invitation to "privilege the force of our own rhetoric", to focus "attention on some (things) and ignore others". One would not need to ignore intellectuals who disagree if one can compel their assent by appeal to rational arguments or objective facts. Yet the second path only seems weak if one believes in the reality of the first path; if we forgo the appeals to some objectivity achieved through some abstraction, and accept that claims will be persuasive on their own merits, and not because of some metaphysical weight they are thought to carry, then we have a different set of possibilities than those espoused by Collier and Collins. So it is suggested that appeals to 'truth' or 'objectivity' or other such metaphysical concepts are made with the aim of compelling assent, of being extra-persuasive. On the other hand, to reverse the direction of the previous suggestion, it might be said instead that metaphysical claims are only used to compel assent: if there was consensus about something, there would be no need to be 'scientific'; such things would instead be common-sense. Metaphysical appeals are only necessary when one is not content to be merely persuasive. This is implicit in a quote from Laudan, which Collier includes in his dissertation:

> "The central epistemological problem in the philosophy of science, simply put, is this: Confronted with rival claims about the world ... and a certain body of evidence, how do we use the evidence to make rational choices between those rivals? (Laudan, 1996 (Beyond Positivism and Relativism), 6-7)

Laudan's 'we' (in "how do we use the evidence") cannot be an inclusive we - there would no "rival claims about the world" if all those rivals would willingly agree to some single 'rational' choice. We must assume that each rival claim is itself thought to be a rational choice by its proponents. This is to say: There are rival claims only if there are differing senses as to what is rational. To assume otherwise would be to attribute ignorance or dishonesty to one rival or another. Thus, Laudan's 'we' excludes other 'we's - 'we's for whom meta-rational choices are made by Laudan's 'we'. And it seems, by the rhetoric in these quotes that these shadow 'we's need this kind of correction, presumably because they lack that necessary tool - Laudan's meta-rationality. In Laudan's invocation of 'we', we see the problematic of STS in its harshest light - an exclusive 'we' which claims scientific objectivity and makes choices which it labels as rational, because it is not content to be merely persuasive. In fact, this is nothing other than an attempt to avoid persuasion altogether - it is an attempt to avoid any such conversation with rivals, by presenting them with a 'truth' which they cannot 'rationally' disagree. In this sense, it is a tactic of force, of coercion.

The urgency of a battle or of a holy war is felt in other dichotomies employed by Collier:

> Assuming on the basis of philosophical consistency, the origins of social constructivism are socially constructed, two consequences follow:
>
> 1. Infinite regress;
>
> 2. Contextual realism;

Collier claims that one must choose either regress or contextual realism; the point of Collier's argument here is to justify the undeniability of contextual realism, based on the claim that the only other option, 'infinite regress', is unworkable. Infinite regress is unworkable according to Collier, because it provides no platform for judgment - it does not allow us to adjudicate among knowledge claims (This is an unquestioned assumption within Collier's entire dissertation). Collier makes the negative argument that we must accept contextual realism because we have no other choice. And these authors seem to think that 'infinite regress' is a real problem - that is, that infinite regress in a theoretical domain might be mirrored by infinite regress in the domain of practice. I, on the other hand, do not fear infinite regress, and forward the timid claim that 'infinite regress' has never harmed anyone: 'infinite regress' has never been a 'real' problem - it is a purely theoretical monster, and has no counterpart in the real world.

The similarity of Collier's dichotomy above to another western classic - 'heaven' vs. 'hell' should be obvious; like the concept of 'hell', the concept of 'infinite regress' is supposed to elicit some form of terror; each also discourages intelligent discussion or participation. The use of a distinction which paints one space as entirely unreal, fantastic and negative - like 'infinite regress', or 'hell' should be signal enough that the distinction may be of very limited value. I suggest that the appearance of 'infinite regress' in theoretical domains might lead us not to accept (by elimination) an otherwise questionable option, but to question the distinctions employed in those theoretical formulations - 'infinite regress' would then be an invitation to rethink the distinctions that frame that theoretical domain.

It may be that STS, by the light of the deflation of <science> it has achieved, might provide new ways of observing the problematics of such severe dichotomies as Laudan's and Collier's, and provide tools to aid in the choice of other distinctions. We might then choose to escape the fetish of objectivity altogether. And perhaps we might choose as our model one based on persuasion, not one based on coercion or fear; that is, one which appeals to potentially different views of the world as equally viable, instead of one which attempts to homogenize those views, by the force of facts. I quote Collier again:

> If the world, the scientific practice and the context in which that practice occurs are just social constructions, then we have no rational basis to choose among differing interpretations or explanations (Collier, 7)

It should not be surprising that for Collier, achieving that 'rational basis' is presumed to be the goal of his enterprise - Collier assumes that there is (or can be) a rational basis for choosing between differing interpretations; but this is nothing other than a claim that one or other of those interpretations was less rational than others. This form of critique - the accusation that one or other rival was less rational than the other(s) - is well established in the West. Yet it rests on scientistic foundations - it requires that the critic can summon a higher objectivity, an external observation that can see what all others failed to see.

If we decide to question those scientistic foundations, then we must also question that form of critique. If we allow that maybe STS does not have to adopt that model of critique, but that STS should explore different models of critique and intervention - that is, if we are willing to give up "The Messianic burden of STS...to lay bare the true nature of the relationship among science, technology and society to the lay public." (Collier, 7), perhaps then we can start anew. Where Collier seeks to "connect STS back to science" (Collier, 22), and Kitcher omits technology altogether - STS becomes 'Science Studies' (as often happens even in Virginia Tech's own STS Program) :

> Science seems no longer to be the principle subject (pride of place now being given to Science Studies itself) but instead we have entered a discourse as closed off from the phenomena that were once central to the field as some philosophical investigations of the 1950s with their exclusive obsessions with the blackness of ravens. (Kitcher, A Plea for Science Studies),

I suggest that STS might better profit by reversing this omission, by decentring science and its emphasis on objectivity and answers; and by focusing on technology and its emphasis on the solution of problems. I define the term technology broadly, as the structures, techniques and systems that we create and manipulate to solve problems in the world. The importance of this definition of technology and its distinction from <science> lies in the irreducible partiality of technological solutions - where <science> is thought to provide exclusive "objective" solutions because it aims at statements about the world as it really is, a technological solution is not bound by such absolutes, and does not exclude the possibility of a multiplicity of other solutions. From this technological perspective what is 'rational' must be re-understood as that which appears to present the best solution to a perceived problem; this conception of rationality then includes the possibility of other distinct rationalities; thus it allows that with rationality there is also blindness - that is, that the observations which inform one rational solution might not include other sorts of observations (which might inform other rational solutions). Within this technological perspective, the role of critique is drastically altered. If critique is supposed to weigh alternatives, then it must come to terms with the possibility that various rivals might be solutions to different problems - that is, that different solutions took into account different perceptions of what the problem actually was. A technological perspective can not divorce the problem/solution from the perceptions of those who have perceived the problem and presented a solution.

This argument unites the two topics of this chapter - software engineering and STS; in each field I have described contradictions that arise due to their misplaced emphasis on objectivity, on meta-claims etc. This technological perspective, I contend, is free from such contradictions, perhaps due to the modesty of its claims.

In the practices of software engineering, I contend that we will profit from abandoning models of objective software development, models that promise a march toward perfection. In their place, I suggest we adopt models of software development which focus not on the abstract approaches of computer science, but instead on the local aspects of the problem present. This focus will include consideration of the various definitions of the problem(s) to be solved, and an attempt to match the complexity of the solution to that of the perceived problem. This discounts the priority of meta-analysis -

while a meta-analysis (like those of the authors discussed above) may make some issues of development visible, yet their importance can only be decided with respect to the particulars of the problem-at-hand. The judgment of the balance between the cost of increased external complexity and the benefit of decreased internal complexity is an irreducibly local judgment. While these authors may appeal to science in their approach, the applicability of their facts is not at all universal.

In the realm of STS, a decentring of science and an emphasis on problem-solution might help us to reconceptualize the history, sociology and philosophy of science not as the central focus of STS, but as a small but formidable chapter in a larger context of technological studies. From this perspective, we might rework our historical, sociological and philosophical approaches, without the metaphysical baggage attached to <science>. Once freed of this messianic burden, our analyses might take different forms, and may even shed light on new approaches for intervention in STS.

To flesh out the mind-expanding potential hidden within these simple ideas, I present two examples: (writing this, I've just realized that I use stupid stuff like this to remind myself that this is supposed to be a conversation with others, not a struggle against my computer. humor brings back to me the idea of audience. And since there will always be another iteration, I'll edit it out next time...)

If we, from the technological perspective see <science> as a particular conception of certain forms of technological activity, then we might also allow that conception (itself a technology) might change, lose relevancy, even fade away; yet those technological activities could continue on, without the overlaid conception, perhaps with a new one. We could then theorize the end of <science> - the end of a particular historical conception of certain technological activities. Already in the present day, the popular conception of <science> as an 'activity aiming at discovering the way the world really is' is on the wane - scientists are increasingly struggling to find funding for pure research, and are increasingly asked to show the economic benefits to be achieved via their research - this suggest that the value and power of the <science> are at their end - appeals to metaphysics are no longer persuasive - and scientists are more and more asked to direct their research towards the straightforward solution of problems. And yet this end of <science> in itself need bring no change to the activities of scientists. But when our conception of those practices changes there may indeed follow drastic changes in the social and political support that <science> has received. Our conception of those practices might then accord them no more weight or relevancy than the practices of auto mechanics, priests, or chorus-line girls. It is uncontroversial to claim that many occupations are engaged in campaigns to generate public support for their efforts. This argument is simply a claim that the campaign I here call <science> has exhausted its resources.

Another example, this time treating the beginnings of science: The proponents of the theory of the Scientific Revolution assert that there was such a thing, and that it had consequences; these consequences, whether construed positively or negatively, are typically believed to be various accelerations of western technology. One would be led to believe that the Industrial Revolution was an outcome of the Scientific Revolution. Where historians of science are apt to credit science for the Industrial Revolution and subsequent cultural changes, an analysis from the technological perspective reveals instead that those cultural changes were well underway, that science was itself one of the processes in those changes, and that scientists were not at all the authors of these changes, but were themselves participants in a changing culture. From the technological perspective (which does not assume the category of science), it appears that historians of science have consistently exaggerated the priority of science and scientists; the flip-side of this exaggeration is a very troubling tendency to ignore evidence that those cultural changes arose broadly out of the efforts of many now-faceless and often lower-class people. The category of science functions in these accounts to generate an elitism that fetishizes the world of ideas over the world of action; at the same time that it lionizes those privileged enough to indulge in the world of ideas, it invisibilizes those whose efforts were in the realm of practice. One is led to believe that all change in the world required an idea for its genesis. If nothing else, this technological perspective makes visible the possibility that intellectual structures might arise out of practice.

From this technological perspective, we can see science as picking out a temporally- and geographically-bounded set of technological practices, which claimed for itself the power of total observation, and thus which claimed not to be solving particular problems, but of discovering the way the world really is. This suggests an atypical interpretation of Needham's famous question "Why didn't the Scientific Revolution occur in China?": this interpretation being that the Chinese did not start the Scientific Revolution only because they did not make the same mistake as western historians in exaggerating the philosophical weight of certain technological practices that the historians were keen to fetishize as 'scientific'.

This chapter has presented in parallel two disciplines, software engineering and science and technology studies, both invested in appeals to higher abstraction; these appeals to abstraction are somehow supposed to immunize the claimants from the blindnesses of those they study. As such, they claim not the characteristics of their object of study, but rather, a more scientific, more objective stance than their object of study. While there is no contradiction in principle for software engineering, it does seem, from the history I have drawn, to be a self-defeating stance. For STS, however, this stance is poisonous. The issues discussed here have generated what seem to be interminable and bizarrely divisive disputation within STS - it seems that the belief in the power of abstraction is a metaphysical commitment, akin to a religious commitment. I have argued for another perspective, one which rejects such claims to higher abstraction, to external observation, i.e., to observation without blindness. In the next chapter, it will be argued that the value of this perspective is not limited to the 'philosophy' of STS - this approach can also shed light on the tensions within STS in its attempts to intervene in the world.

Conclusion: STS from a software engineer's perspective

One

This thesis has emerged out of a tension I see between the concepts 'science' and 'technology' as they typically inhabit the discourses of philosophy, history, sociology, and even science and technology studies.

My particular sense of this tension arises to some extent from my (comparatively) non-traditional background - I was 'educated' as an electrical engineer, and worked as a software engineer for a substantial amount of time. Software engineering was in a period of great growth, and demands for improved reliability of increasingly 'mission-critical' software led to calls for more rigorous systematics of programming. As we saw in the brief survey of a few texts, these calls were interpreted as calls for a universal systematics, that is systems which could be applied across the board, regardless of the specificities of any given project. My own experiences in the field of software engineering have thus been in the midst of early attempts to impose external 'scientific' categories, which are oriented towards objective universal criteria, and which thus provide the foundation for a certain 'external' measurement and critique. As made explicit in the previous chapter, my approach parallelifies some of the attempts to scientize the practice of software engineering with the attempts to import models of scientific objectivity from the social sciences into STS.

And while I will not argue that software engineering should or should not accept that imposition of objectivity, I will argue that STS surrenders its credibility if it accepts such an orientation towards objectivity. As suggested earlier, the move towards objectivity is explicitly seen as a prerequisite for measurement and critique. And in this thesis (especially in the Epistemological Chicken chapter) I survey the problems that arise for STS when it tries to turn that critical attention against the very source of the legitimacy of that critical attitude, that is, scientific objectivity. This thesis has in a way been a survey of possibilities for refusing the critical attitude, without thus denying the relevance of STS, that is, of finding a reflexively-robust position for STS to offer its analyses of science and technology.

Andrew Pickering's *The Mangle of Practice* impressed me as one of the first STS analyses which aimed at such a position, and it was this book which led me into this thesis project. Yet I have come to argue that Pickering's reservations about agency prevent him escaping the strong gravitational field of scientific objectivity.

Two

While I found Pickering's representational/performative distinction fruitful, it is, at the same time a bit suspect - Pickering is really offering his representation of a performative account - it clearly is not a performative account itself. Pickering therein opens himself up to a reflexive critique - if he is insisting that a performative account is in some way a better account, then he must be prepared for someone to offer a performative account of Pickering's own practice, and claim it to be a better view than Pickering's own.

While I found it important to capture some of the sense of Pickering's distinction, yet I also needed to avoid the reflexive problems therein. The pragmatist tradition seemed to me one that could robustly deal with reflexivity, and especially Larry Hickman's discussion of the technological in his study of Dewey seemed appropriate; at the same time I began to confront Wilfrid Sellars 'scientific image' from the paper

"Philosophy and the Scientific Image of Man". The distance between the two concepts was enormous; it seemed particularly clear that Sellars 'scientific image' was ill-suited to account for the technological aspects of science. (that is, Sellars followed the tradition of the philosophy of science in seeing science as fundamentally oriented towards explaining why things are the way they are; and entirely ignoring any examination of how it is that humanity has made the world the way it now appears.) And where the term 'image' suggests an objective account of something, a complete one-to-one mapping, a thing recorded and indisputable, the term 'perspective' suggests instead a partiality and an openness.

Thus the genesis of the important conceptual move of this thesis - the schema of two perspectives (which do not necessarily exclude others): the scientific, and the technological.

To summarize the discussion of the previous chapter, the technological perspective is founded on the irreducible partiality of technological solutions - as opposed to the scientific perspective, which is founded on exclusive "objective" solutions, and whose rhetoric constitutively limits the kinds of questions that can be asked about science.

So I have tried to point out possibilities for STS within this alien terrain - as these observations focus on problems in the philosophical foundations of STS, they also suggest possible re-orientations. But in the language of the technological perspective, the suggestion of a re-orientation is not grounded by any statement of fact, but grounded only in the recognition of a paradox, in the presentation of potentially more productive alternatives and the recognition of a vista of alternatives that was not there before.

STS has been a leading voice in the movement to question the foundational claims presented by philosophers of science in defense of the sciences. And it seems clear that this questioning is not limited in domain to the physical sciences. As discussed previously in regard to the Epistemological Chicken debate, and especially in regard to Collins and Yearley's position, it thus seems philosophically suspect (in the context of STS, at least) to claim that one's interpretation of another's work is any more objective than the original account, if that claim rests on the replacement of one set of foundational claims by another, equally disputable set of claims.

Yet Collins and Yearley, in describing their 'third method' insist that it "is no longer subservient to accounts of the work of scientists and technologists and the stories of philosophers but rooted in a special understanding of social life." (Collins and Yearley, 321) This amounts to the claim that their understanding of social life is more scientific than science; as such it is nothing other than a thinly veiled foundational claim, and clearly no different in kind than scientific claims they themselves undermined.

That their 'special' understanding of social life could give them the philosophical purchase to offer 'objective' critique is a claim that STS is now in a position to reject out-of-hand.

This seems to be the standard form of STS analysis - contesting the claims of science by offering counterclaims which insist on a different set of foundational assumptions. Clearly this is dubious philosophically, but it is also pointless rhetorically - science and technology are widely regarded as having immense instrumental value; even were it armed with the best foundational claims, STS could never hope to claim any such instrumental value. STS is unlikely to have the resources to weather a war of claims and counterclaims.

Those in STS who continue to offer accounts in the mold of science, defend doing so by insisting that readers can and will question those accounts' validity, and thus claim that nothing more is required of an STS account are simply reiterating the belief that this process should (and will) produce a final objective account. Such accounts thus revive the appeal to an absolute objectivity that these same accounts deny scientists in their accounts. Instead of arguing, as those STS accounts do, that all we need is to add in one more resource, sociology, to allow us to reach that goal of objectivity, it is suggested here that we recognize that goal as a myth, and refuse it altogether. It is my hope that this paper might present an plausible alternative to such myths, and one which does not reinstate those myths through some dubious foundational claims. Indeed, it is precisely the success of STS in problematizing the foundational claims of modern scientific realism that now requires STS' intense and reflexive scrutiny of all foundational claims, and the attempt to elaborate a valid critical position which does not rest on disputable foundations.
If STS is unable to sustain/withstand the kind of critical scrutiny of foundational claims which it lavishes on other disciplines, it offers no compelling reasons to value its approach over any other.

It is worth observing that such a robust reflexivity is not required from scientists themselves. Baldly asserted, the success of a scientist's work has little or nothing to do with the robustness of its philosophical reflexivity. And in the previous chapter's discussion of software engineering, it was similarly clear that the practice of software engineering is not bound to be reflexive in that way either. That is, the success of their endeavor does not ride at all on the quality of its philosophical grounding. Despite my misgivings about the textbook authors' claims, these authors are offering solutions to problems - that is, their work may be quite useful, even if their claims may not be philosophically well-grounded. The profundity of their reflexive introspectionism is simply not a measure of their success. Thomas Kuhn, in Structure suggests that some philosophical considerations may play a role during certain crises. But Kuhn also suggests that the resolution of these crises is achieved by the establishment of a new problem-solving paradigm, not by thorough philosophical examination.

In this sense, STS is much different from science; STS is not scientific, it is neither meta-scientific; it is instead a refusal to accept a scientific perspective as the only valid perspective. Given this, it cannot be about aligning resources in favor of some particular perspective, but about a ruthlessly robust reflexivity which accommodates all potential perspectives.

Standard approaches in STS, especially those that pattern their critique after that of the SSK movement, often meet with strong resistance from the scientific establishment, as evidenced by the so-called Science Wars of the 1990's. Those STS positions seem to present an open challenge to the autonomy and authority of science. And to a certain extent, several of the authors (especially Fuller and Collier) reviewed previously maintain that contestatory STS position. Yet the position I am attempting to craft here is also intended as a step away from that contestatory stance.

Nevertheless, the re-examination of scientific objectivity that I suggest here, and the move towards the 'technological perspective' that I propose could be read as yet another attempt to undermine the autonomy and authority of science/technology. This (mis?)reading of my position would suggest that if we claim that scientists and technologists are really only offering problem-solutions, then their achievements can be questioned in a new way; having dismissed the realism-relativism debate, it might be read, I have opened the gates to a relativism which ignores the honest and painstaking work scientists may actually perform.

And if the status of science and scientists in our present day societies rests on the notion of objectivity which I have been attempting to circumvent (and reflexively, I hope), then I am indeed presenting a challenge to science and scientists. But it seems clear instead that there is no such quandary. If we allow that the effectiveness of science (its successful problem-solving and predictive ability, etc.) might be entirely separable from the question of its objectivity, then we might find in the support for science not an insistence on objectivity but an appreciation of its success. And it does seem now that most support (especially in the political realm) for science arises from its instrumental value. But those sciences whose instrumentality is harder to establish (that is, those who cannot show a long record of instrumental successes) might wish to resist this move.

And clearly, appeals to the objectivity of one's claims have an irresistible rhetorical force. But it is that very irresistible force that marks appeals to objectivity as no longer discursive; indeed it is an attempt to escape the domain of discourse altogether.

While I reject Fuller and Collier's demand for a foundational basis for critique, yet their rejections of current trends in STS - what Fuller calls "the descriptive turn", and what Collier labels as the "comfortable cynicism of social constructivism"(Collier, 9) all reflect a belief which I share with them, that STS must be more active, and more activist in the world. But where Fuller claims that "the descriptive turn [in STS] has emasculated the normative dimension of science studies and in the process has limited the field's potential for radical critique and revision of our knowledge enterprises" (Fuller, 1992, 391), this thesis argues against any such 'radical' critique. This belief in a detached, or even neutral critique appears to be an unexamined premise in STS - arguments are made on its behalf, without consideration of what could justify such a position.

This particular critical attitude infuses much of the STS vocabulary, and not least the term 'intervention', the term that STS has adopted to label its aspiration to be a positive force for change. In general, the term 'intervention' suggests a third party entering a conflict in order to protect its interests. In STS usages, though, it seems to have taken on a different sense, one of a third party bringing to bear a studied neutrality to help resolve a conflict. This claim to neutrality however, would be the claim that the gifted neutral observer can do something that other participants in the conflict can themselves do - see both sides at once. This is, once again, a claim to observation without blindness Indeed, I see the term 'intervention' to be infused with the sense of neutrality I reject above; and I suggest that we might benefit by choosing another term entirely. In its place I suggest with refreshingly little argument the term 'intercede'; 'intercede', it is hoped, conveys a sense of mediation, and of reconciliation. Thus, where 'intervention' suggests an undeniable objectivity or an undeniable neutrality (the two are interchangeable), intercession suggests instead a persuasioning with no recourse to such bludgeonary devices as objectivity or neutrality; This approach is consistent with the technological perspective that I have presented herein.

It seems to me that Rayna Rapp's approach is a model for STS - while speaking of her efforts to translate a genetic counselor's work into French or Spanish for patient, she realizes "There was no neutral space from which translation could occur. Later I realized that this lack of neutrality ran through my interactions at every level" (Rapp, 19) Her book - Testing Women, Testing the Fetus is a fine example of the approach I am looking for. Yet it must be recognized that Rapp produced the book far outside the strictures of the typical academic career. She spent fifteen years researching, and her participation in the issue took her far afield of typical academic pursuits; even so far as taking telephone calls from strangers concerning the issues on which she had published. This kind of activism is one that most academic departments do not value, and for which academics would receive little credit; Rapp's work, and this thesis too, are intended as challenges to a standard academic orientation.

And although this thesis has said quite little about whatever worlds might exist outside of academia, it is paradoxically intended as a suggestion that STS must find itself in its engagement with those worlds and with other disciplines, instead of continuing an illusory search for a scientific foundation for a discipline oriented to an illusory critique.

As I worked towards producing the final draft of this thesis, I came to realize that I enjoy this work. This may have been conditioned by the assurance that I was not contemplating a life of doing this - it appears to me still to be a digression, from whatever comes next. I have become quite aware of the incredible luxury of being able to spend time reading, thinking and writing. All the ridicule I thought might be hiding around the corner concerning the amount of time I have expended on finishing a masters degree cannot compare to the pleasures of intellectual contemplation and discussion; nor to the flexibility in thinking that I have gained. But this is all quite indulgent, unless I attempt to make a difference.

Future Directions for Research

> ...since for mathematicians the principal activity is proving new theorems, what they will ask of any description of their subject is: Can it be the source of new mathematical material? Does it suggest new notational systems, definitions, assertions, proofs? ... If it does not engender theorems, then mathematicians will be little interested in its project of redescribing their subject --"the queen of sciences"--via an explanatory formalism that (for them) is in a prescientific stage of arguing about its own fundamental terms. (Rotman, 2)

1. Methodologies of software development and methodologies in STS.

Recent developments in software engineering seem to follow along the lines of this thesis - proponents of new 'agile' or 'lightweight' methodologies, reject what they call 'monumental' methodologies (exemplified by authors Pree, Lieberherr, and Khoshafian above). Martin Fowler, one of the leading voices in this new movement, describes the monumental methodologies as being focused on the predictability of the software project; whereas the agile methodologies are focused on accommodating unpredictability. Fowler attributes the 'monumental' methodology approach to the desire to treat software development as an engineering process similar to that of civil or mechanical engineering. In keeping with this emphasis, the monumental methodologies attempt to make software developers interchangeable - programmers are akin to construction workers - they perform roles (since the designers have specified everything, the design is set, and can supposedly be easily translated into code). Agile methodologies reject this engineering metaphor; they thus reserve much of the design decision for the programmers. Alistair Cockburn, another leader in the agile methodology movement offers this:

> In the title, [of his article] I refer to people as "components". That is how people are treated in the process / methodology design literature. The mistake in this approach is that "people" are highly variable and non-linear, with unique success and failure modes. Those factors are first-order, not negligible factors. Failure of process and methodology designers to account for them contributes to the sorts of unplanned project trajectories we so often see. (from Fowler)

These methodologies have been surprisingly controversial in the world of computer science and software development; perhaps they are as controversial as Latour's ideas were to Collins, Yearley and Bloor. There are many fascinating parallels - the simultaneity of the development of software development and of the development of STS; and the similarity of the turn away from monumental/academic models predicated on 'external' observation, among others. I would write it myself if there was any ink left in my computer.

2. The problematics of appeals to democracy in STS.

Throughout discourse in STS, there appear certain appeals to democracy as the solution to problems both in science and in technology. Especially a predominant theme in the science, technology and society movement, it also appears in the work of authors such as Steve Fuller, discussed above. These appeals often differ and conflict as to what a democratic process is or should be; and as discussed above, what is described as a 'democratic' process can often appear as something quite distant from democratic ideals. Intellectual integrity should demand that appeals to democracy should be as carefully scrutinized as appeals to any other utopian fantasy. It seems somehow that this scrutiny has been lacking in STS

3. An examination of theses and dissertations for the framework of intervention.

The brief discussion of intervention in the conclusion scarcely begins to examine the concept of intervention, and the problems it presents within STS. Increasingly STS has had to contend with issues of activism, with its growing alignment with other movements, such as environmental justice studies. Indeed, the thesis above might serve merely as an introduction to that topic.

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