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THE ASSENT OF MEN

©

by

Edrie Sobstyl

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ABSTRACT

This thesis defends a version of feminist empiricism as the theoretical position best able to accommodate the critical and positive moods of feminist philosophy of science. Strong emphasis is given to the importance of narrative approaches to knowledge, especially to the need to interrogate the structure of existing knowledge claims in order to mount effective challenges to them. These central commitments are supported by a survey and critique of some of the central arguments of recent feminist approaches to epistemology and philosophy of science. The strong association between the core values of science and its intrinsic masculinity is examined and rejected as historically and conceptually untenable. The shift toward communally based epistemologies is received with guarded optimism. If due care is taken in the description and assessment of the scientific community and its projects, and so long as individual autonomy and responsibility are preserved, then recognition of the influence of group mechanisms in the construction of knowledge will be a positive advance for feminist theory. It is argued that treating the knowledge-seeking process as an interactive one will be a cautious and effective way to proceed. The dismissal of "bad" science as a fringe phenomenon with no serious philosophical implications is seen as hasty. One

component of feninist empiricism is its commitment to socially dictated norms governing science, so it must be willing to confront violations of norms. This confrontation is also attuned to the feminist empiricist affirmation that science must be regarded as a process deeply embedded in multiple social structures.

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The original germ that became this essay can be found in a somewhat rambling but spirited paper written almost five years ago for Mohan Matthen. I have acknowledged Mohan in his role as supportive teacher, graduate director, and valued colleague, in all of which he has excelled. He also oversaw work for some of my preliminary exams, and enabled me to pursue my interest in gender issues in that context. Mohan has fulfilled the formal and informal duties of supervisor in exemplary fashion. More than that, his interest, advocacy and counsel have been vital to my progress through this degree, the writing of this dissertation, and the academic and personal stability that such tasks demand. He has been a genuine mentor. His support of a feminist project has been especially important to me. I offer him my admiration, respect, and lasting gratitude.

Financial support for this project has come from many sources, and I acknowledge them all with thanks. The philosophy department at the University of Alberta, the Province of Alberta, the Social Sciences and Humanities Research Council of Canada, the Canadian Federation for University Women, and the Faculty of Graduate Studies and Research at the University of Alberta have all contributed to my ability to keep body and soul together, and have enabled me to take parts of this essay on the road.

With so much help from so many individuals of such insight and wisdom, it may be hard to imagine that this essay could still be afflicted with errors or inadequacies. Any shortcomings are entirely my own.

Finally, I wish to thank the main character in the story behind this story, who kept his paws firmly on the keyboard through most of the project, Dennis.

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Gosplan.[22] There is no reason whatever to think that the epistemologies of science communities are ruled by an iron fist. The claim that scientific communities can force individuals to perform work in contravention of their own epistemic goals, for the good of their communities, is far too strong.

Kitcher appears to have no views about where his "rival cognitive objects" come from, and that is also deeply problematic. If he really thinks that the dilemma he poses could arise, he must think that alternative theories or methods or auxiliary hypotheses or points of view are detached (or detachable) from individual knowers. That is, he must think that we have substantial leeway in choosing from among many possible such alternatives. Why? Without this assumption, there would simply be no dilemma. If the rival cognitive objects in question have real proponents, then the community could just assign their advocates the task of investigating these objects. Indulgence on the part of the community, and not altruism by individuals, would be what is needed. It is no objection to say that a particular alternative might not be held by any member of the community, for two reasons. For one, if no one in the community held a different view, it is unlikely that it would occur to anyone, and if it did, then let them test it.

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[22] I thank Mohan Matthen for suggesting this metaphor to me, and Kathleen Okruhlik for many other helpful remarks on this passage.

Further, questions about what counts as a community are implicated here. If we count all the labs working on the same problem as a community (which seems to be what Kitcher has in mind), and if different labs adhere to different theories or methods, then there are still real people whose individual epistemic integrity would be in no way compromised by inquiring into divergent viewpoints. The solution is simply to hire them. If we fail to take seriously the constraint that we must be talking about points of view that are really occupied, by no matter how small a minority, then we can easily push Kitcher's dilemma to an absurd extreme: who in the scientific community will investigate the flat earth option, or the influence of the paranormal? Many alternatives tend to be ruled out by the fact that no one promotes them, or, at the very least that those who promote them ignore accepted evidence and lack recognized scientific credentials.

I have indicated that it is fairly simple to defeat Kitcher's worries about discrepancies between the epistemic demands on individuals and communities, although there may be other, less easily defeasible problems. More important, however, is the fact that if one gives up epistemic individualism and the commitments that accompany it, such worries do not even arise.[23] On a more communally oriented

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[23] This is especially ironic given Kitcher's hostility toward Longino's project; see his "Socializing Knowledge" (1991), in which he accuses Longino of merely offering a

model, it is neither the case that individuals seek in isolation to maximize their own chances of getting things right, nor that they choose to belong to communities that seek to discover the greatest possible number of true beliefs. Rather, the epistemic standards available to the individual just are those that the community sanctions, or those that are introduced through communal negotiation and consensus. I think that Kitcher is right to hold that epistemology should take a more conservative approach to the question of the role of the community in acquiring knowledge, but not for the reasons he gives.

II. Why Change the Subject?

I have looked at the main reasons for regarding individualism as an outmoded stance for epistemology and philosophy of science. Since I am arguing for a double-aspect strategy, in which individuals and the community are on a par, my position has the advantage of avoiding the pitfalls of individualism while capitalizing on some of the gains of community-based epistemologies. In this section, therefore, I am going to look at what those benefits are. It is clear that the community functions as the public arena in which epistemic debate takes place, and equally true, though somewhat less clear, that the community is the generator of epistemic values.

"surrogate" for truth and well-groundedness of individual belief in her focus on the social achievement of scientific knowledge.

It is customary for those who favour the idea of a community-based conception of knowledge over the prevailing individualistic one to acknowledge their debt to C. S. Peirce as one of the earliest advocates of the idea of an epistemic community.[24] Peirce emphasized that modern science is inherently social, although he insisted that it is not its social nature alone that accounts for the distinctive rationality and success of science. He claimed that the logic of science, especially the objective validity of induction, played an equal role. It is still helpful to start with Peirce because it is in his work on communities of inquirers that important themes and questions for contemporary epistemologists emerge. Among those that I will focus on in this section and the next are:

(1) the distinction between found or *de facto* communities and constructed or chosen communities,

(2) the criteria for the identification of community members,

(3) the role of language in mediating or determining the possibilities for the construction of knowledge, and

(4) the standards of evidence to which community members appeal.

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[24] In this passage I rely on the interpretations of Peirce offered by Lorraine Code (*ER*), Cheryl Misak (*Truth and the End of Inquiry* [1991]) and W. Christopher Stewart, "Peirce on the Role of Authority in Science" (1994).

The primary advantage of a community-oriented epistemology is that it better reflects the way in which people actually come to know. Whether or not we accept Jaggar's argument for the biological necessity of human interdependence, we would certainly be negligent if we failed to notice that in practice all scientific knowledge-seeking takes place within the confines of shared conceptual schemes and epistemic standards. Admittedly, it sometimes looks as though research is being carried out in isolation from a community of knowers, but this appearance is superficial. The system of peer review and reward central to contemporary science helps to ensure communality at the institutional level.[25] At a more mundane level, shared background assumptions, conceptual schemes, goals, instruments and techniques assure that even where scientists disagree, they can make sense of one another's positions and perhaps agree on what would count as decisive evidence for a contested belief.

However, as Helen Longino is quick to point out, it is not just the fact that institutions, standards and education are shared which suggests that we ought to take seriously

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[25] It may be that the view of knowledge as communally constructed accords well with some elements of scientific practice, but not with others. Nobel Prizes, for example, may be shared among collaborators, but may not be shared with all of the technicians involved in research, and are not typically shared with the institution where the research took place. I take it that such "grey" areas bolster my argument for a double-aspect approach.

the notion of the community as epistemic agent. She argues that the application of scientific method in the contemporary context "requires by its nature the participation of two or more individuals".[26] The complexity of scientific inquiry, she claims, necessitates the breaking down of research into parts, with different individuals or groups responsible for each part. Those responsible must then negotiate the integration of their results with all the others. It is this interactive quality of scientific inquiry that motivates Longino's commitment to a community-based epistemology and, as we shall see, gives her a very strong basis for the transformation of scientific communities.[27] Cynical arguments about the lack of real consensus within scientific communities are to be resisted because the work of science gets done. But note that at this stage, the argument in favour of treating the community as knower is a descriptive one. It is interesting to note that a prescriptive argument that science must be treated as the product of collective labour, based on public, intersubjectively verifiable statements and not on

[26] Longino *SSK*, p. 6

[27] Longino observes that the communal character of knowledge is a double-edged sword that both protects knowledge from, and renders it vulnerable to, social and political interests and values. (*SSK* p. 12) She further argues that the community functions to decrease opportunities for individual idiosyncrasies to have any influence on science (in Antony and Witt [1993] p. 265). As Harding points out, if those traits are widely shared, the community will function to p ect them.

individual, private experiences, was made by Neurath in some of his debates with Carnap.[28] Kitcher's position does not deal well with such prescriptive debates.

I have argued that community-based theories of knowledge are resistant to certain dilemmas to which individualist epistemologies are prone. Longino claims that examining "individuals in interaction with one another in ways that modify their observations, theories and hypotheses"[29] gives the social approach to knowledge even greater reach. She maintains that the problems of the theory-ladenness of observation and the underdetermination of theory by evidence become more tractable from the perspective of the scientific community than from the perspective of individualist knowers. In the tradition of Kuhn, Hanson and Feyerabend, the theory-ladenness of observation is supposed to both rule out the possibility of meaningful debate between theorists with widely divergent beliefs (incommensurability), and make theory choice arational, based on individual faith or preference. Longino states, in contrast, that observers with different beliefs

not "see" the world differently, but rather attend to different aspects of it, and that this becomes apparent when we search for communal factors, namely shared background

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[28] Galison (1988)

[29] "Knowledge, Power and Science" (1991) p. 670

assumptions of any two disparate theories or theorists.[30] This will, however, be very difficult, because such assumptions may be so deeply embedded that they are invisible to members of the community in question.

Similarly, there will be background assumptions that disparate theories do not share, and these may give rise to subtly different interpretations of observation. A researcher with one set of assumptions may be inclined to count certain observations as crucial evidence, Longino claims, while another may not count those same observations as evidence at all. Thus the same body of evidence sometimes does not support a variety of disparate conclusions, as the thesis of underdetermination holds. Rather, the same event may give rise to evidence, but that event will be interpreted according to one's background assumptions, so that different features of the event will constitute evidence for different hypotheses. Even when it seems to be the case that one cannot rationally decide between rival hypotheses because of the problem of underdetermination, Longino's suggestion is that we must delve more deeply into the background until we find substantive, non-subjective differences that give rise to variations in evidential relations. However, although it is obvious that taking the community as the basis for theorizing about the construction of scientific knowledge

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[30] *SSK* p. 54

offers new perspectives on these old problems, it is not entirely clear that they are solved.

A somewhat less contentious benefit of communal epistemic agency lies in Longino's claim, cited earlier, that emphasis on the community of knowers permits a reconceptualization of objectivity as a property of critical group interaction rather than of individual cognitive practices. On the old positivist story, science had established a set of objective rules and procedures, and the individual's objectivity was only as good as his or her demonstrated ability to follow the rules. This story is undermined by historical work, such as Paul Feyerabend's,[31] which shows that many of the most important scientific advances have been achieved in less than objective ways. Longino's proposal turns this notion of objectivity on its head, so to speak, by placing the source of objectivity in the ongoing activity and consensus that results in the rules and procedures that scientists are then expected to follow. Non-compliance does not establish the non-objectivity or irrationality of science. Instead it shows that the rules themselves are part of the shifting sands of science. The novelty of Longino's approach is that it gets us away from histories that package revolutions in science into discrete segments without sufficient examination of the extent of debate and negotiation that occurs before scientists give up

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[31] Feyerabend, *Against Method* (1975)

certain commitments.

There is an objection to Longino here that points to a tension in any community-based approach to scientific knowledge. One could argue that in situating objectivity within the critical interactions of scientists, Longino has departed from what we traditionally mean by scientific objectivity, and is no longer talking about scientific communities as we ordinarily consider them. It is Longino's intent to depart from an individualist objectivity, so that is not a problem for her, but it does seem that Longino shares with Nelson a tendency to move freely between *de facto* and constructed communities. It is not always clear when either of these writers is being descriptive, and when they are offering normative proposals for better scientific communities. This slippage pervades much of the literature on community-oriented epistemology. A notable exception here is Lorraine Code, who is quite explicit in pointing out that because existing communities can be internally exploitative, she is concerned with constructing a community in which we could live.[32] Code highlights the significance of Anne Seller's observation that "as an isolated individual I often do not know what my

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[32] *WCSK?* p. 276; Code is one of the few writers who consistently shows the links between self and society as they appear in epistemology with those same concepts and ideas in moral and political theory. It must be noted that much of my criticism of Nelson's and, to a lesser extent, Longino's commitment to community primacy is an echo of problems that have been uncovered in communitarian political philosophy.

experiences are".[33] But she does not directly attend to the ways in which the power of the community to interpret an individual's experience for them has often been used to silence and confuse women. In any case, it should be noticed that there is some uncertainty as to whether the community is defended as a more appropriate epistemic agent because that is closer to how things are, or because it enables us to realize certain goals about how we would like things to be. There may also be a tendency to de-emphasize the extent of dissent within and between science communities. Some of the disagreements highlighted in chapter one are evidence of such dissent, and confrontations over "bad science", to be discussed in chapter three, will show the nature and impact of particular kinds of dissent.

III. The Uncertain Nature of Communities

It is apparent that the move to address the role of scientific communities in the construction of knowledge is an important and productive one. It must not be assumed, however, that the shift to a community-based epistemology will offer only theoretic and practical advantages at no cost. Just how well can a community focus enable us to change the way in which science is conducted? It depends in part on how we identify the community. If we opt for the broadest possible *de facto* community, as Peirce does, then

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[33] Code *WCSK?* p. 284, Seller "Realism versus Relativism" (1988) p. 180

the question of identity is easily settled. One identifies the community by looking about oneself. For Peirce, everyone (or everyone who cares about inquiry) is in the community of inquirers. Such a broadly based notion of community is unlikely to be of much help at either the descriptive or the normative level for philosophers of science, but it does give one pause. Cheryl Misak suggests that Peirce may have meant to include animals in his community of inquirers, which raises some unusual possibilities.[34] After all, this would be one way to include some of the objects of inquiry in the scientific process, thereby erasing the artificial and often criticized boundary between subject and object. We would also be remiss if we failed to notice that we often learn things, including things about ourselves, from animals and other non-humans.[35] So it seems that a reasonable notion of community might in general include animals, but is this going to be true, always or necessarily, when our interest is in epistemological communities? If our interest in epistemological and especially scientific communities relates to their standards and values, it is not clear what role non-human life could play. Is my cat a member of my

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[34] Misak, *Truth and the End of Inquiry* (1991)

[35] This theme is explored in particular detail by Donna Haraway in her discussions of simians and cyborgs, and the ways in which scientific perspectives on the human condition are informed and altered by ongoing investigation of our near relatives. See Haraway (1989) and (1991)

epistemological community? It does not seem so, and yet his behaviour does enter into the formation of my beliefs. Were he a subject in a medical laboratory instead of a house pet, how would this change his epistemic status, if at all? I raise these questions not so much because I think these worries about my own cat are serious, but because they point to a pressing set of dilemmas. Epistemology may well profit from the switch to a community-based approach, but the details of the approach, and how they may differ between knowledge communities and science communities, are not always clear.

In light of such questions, communities that consist of humans, animals, and other organisms as well as inorganic nature probably ought not be ruled out of consideration *a priori*, especially not by those who profess an interest in inclusive science. It has been suggested that intimacy with one's research material, even if it is, say, a plant, opens possibilities for understanding that go far beyond the usual detached perspective of contemporary science.[36] It has further been suggested that it is not enough for a community of responsible knowers to embrace alternative points of view as part of its critical process. Such a community must also take its environment into account and act as an advocate for

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[36] See Evelyn Fox Keller's *A Feeling for the Organism: The Life and Work of Barbara McClintock* (1983)

the planet, which cannot speak for itself.[37] As we shall see, however, such far-reaching communities, and possibly even some human communities, are discounted *a priori* by Lynn Nelson's linguistic justification for treating communities as primary knowers.

Nelson insists that the beliefs of individuals "depend on public language and the conceptual scheme it embodies".[38] The assumption that knowledge is language-dependent seems to rule out the possibility of animal or even human infant knowledge, a problem referred to as the "infralinguistic catastrophe" by Stephen Stich and others.[39] It also leaves in limbo non-discursive forms of knowledge, such as body language and even the correct use of some laboratory instruments, which may be articulable but are seldom articulated. Here one could perhaps appeal to

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[37] This is one of Code's suggestions for an ecologically viable epistemic community, *WCSK?* p. 288

[38] Nelson, *WK*, p. 256. It is unclear whether Nelson intends this to be a sufficient or a necessary condition, although the former, as discussed below, would make little sense. Even if language and community standards of warrant are recognized as necessary conditions, however, they are not the *only* conditions for knowing. As Philip Kitcher points out in another of his critiques of socialized epistemology, such positions offer no coherent alternative to the notion that knowledge involves individual belief. This objection is a bit quick, in as much as writers like Longino and Nelson do not *deny* that knowledge involves individual belief, but they do say surprisingly little about what role individual belief plays in knowledge. See Kitcher's "Socializing Knowledge", (1991) p. 677

[39] Stich, *From Folk Psychology to Cognitive Science: The Case Against Belief* (1983) pp. 214-217

the distinction between knowing that and knowing how, but then Nelson would still owe us some account of knowing how. It is evident, however, that Nelson's intent here is not to make the uninteresting claim that we ought in principle to be able to say what we know, which, by itself, hardly justifies a shift to community-based epistemology. Nor is she making the outlandish claim that shared public language is sufficient ground for individual belief. If it were, we would all find ourselves blessed with rather more beliefs than we thought we had. Nelson's appeal to "language *and the conceptual scheme it embodies*" must be seen as aiming to forge some connection between language and the world. A thorough discussion of the ways in which languages relate to conceptual schemes and to the world lies outside the scope of my thesis, but I do wish to raise some worries about Nelson's reliance on shared language as a cornerstone of community-based epistemology.

From a feminist perspective, the most obvious among these worries is the fact that existing communities are internally exploitative, and that shared (sexist) language is one of the instruments of oppression on which communities rely. It is certainly true that the oppressiveness of existing communities is in part a result of our failure to recognize the epistemic consequences of the social structure of science. We should be cautious not about attending to communities as agents of knowledge *per se*, but about the ways in which communities do and could construct knowledge.[40]

I have already pointed out that Nelson slips rather easily and frequently between existing and transformed communities without indicating when her views are to be taken as descriptive of the former and when they are intended as prescriptive for the latter. But in the case of communal construction of knowledge and epistemic criteria by means of shared languages and conceptual schemes, it is not really possible to see her remarks as applicable to anything but *de facto* communities. Nelson must surely recognize the contribution that feminist scholars have made in demonstrating that found communities and the languages they utilize are sources of women's oppression. How, then, is a conversion to epistemology in which the community is the knowing subject supposed to facilitate a transformation in the oppression of women? Helen Longino's position is superior here, because of her emphasis on the need to wrest tacit standards of knowing from the communally shared background in which they are embedded, and to scrutinize them vigourously. This approach is to be preferred because it recognizes both that these shared standards are (or have been) implicated in the oppression of women, and insists that all implicit knowledge be brought to the surface and

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[40] I thank Kathleen Okruhlik for her comments on this passage.

debated in an equitable and potentially revolutionary fashion. Nelson's approach allows such debate, but falls short of mandating it.

Nelson describes at some length the way in which the atomistic Cartesian individual became ensconced in modern western thought as the primary agent of knowledge. She does not, however, consider the distinct possibility that some of the languages and conceptual schemes on which she relies may themselves be inextricably tied to individualism, and that a full-scale reordering of language (and the conceptual scheme it embodies) may thus be necessary to the reconstruction of science. I do not wish to suggest that such a reordering is impossible, but as a proposal for change it is neither original nor post-positivistic.[41] Furthermore, Nelson needs to take seriously the observation of many feminist theorists that traditional epistemology has been constructed by men, based on the experiences of men, and in the interests of men.[42] If the language of individualism is

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[41] Noam Chomsky (1990) has claimed that *Descartes* believed language to be the unique mark of the mental, although he admits the textual evidence for this claim to be slim. (Chomsky, "Linguistics and Descartes") It would be ironic for Nelson if this were true, as it would indicate that it is possible to acknowledge the significance of language and still be an individualist about knowing. Thanks to Deborah Brown for bringing this to my attention. In any case, Nelson's observation that the ability to entertain a proposition depends on relations with others is not new. Putnam made it in "Explanation and Reference".

[42] As feminist theory has grown sophisticated, of course, it has been widely acknowledged that this observation is true of only a subset of men, namely privileged white men.

what men have come up with, then perhaps they have done so because its conceptual scheme offers the best fit with their experience of the world. Many feminist theorists maintain a radical split between male and female experience, based on the disparity in their actual or potential material connection to other human lives.[43] Such writers concede that "while it may be true *for men* that the individual is epistemologically and morally prior to the collectivity, it is not true for women."[44] If it is true that men and women experience the self/other dichotomy in fundamentally different ways (and I acknowledge that this remains to be decided), then Nelson may simply be arguing an impossible case. Women and other marginalized groups can either try to learn to get along in an individualistic world, or become epistemological separatists.

When we focus on Nelson's prescriptive proposals, language is equally problematic. (Longino, who does not place the great stress on language that Nelson does, is faced with difficulties here, too.) Nelson advocates a "global community" as the ultimate goal of inclusive science, while Longino demands that at the very least, "socially significant groups" must be included in the process of articulating and scrutinizing the standards of scientific knowledge-seeking. Both of these suggestions

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[43] Robin West, "Jurisprudence and Gender" (1991)

[44] ibid. p. 207

presume that it will be a relatively straightforward matter to translate between the ideas of both participants who speak different languages and participants who speak the same language as the majority but whose experiences differ in ways that make them "socially significant". In Nelson's case, it is possible that the conceptual schemes underlying different languages could be wildly divergent between groups, making it unlikely that we can even know when groups have managed to share their epistemic backgrounds with one another. Longino, in contrast, appears to try to avoid this problem by making it a requirement of the objective scientific community that it include members of socially significant groups who share at least some minimal standards with the majority, which may then be invoked as part of the basis for criticism.[45] But if there are linguistic and/or conceptual barriers to be overcome, this move works to ensure that whatever it is about the significant group that makes them significant is likely to be lost in translation. There are likely to be important conceptual differences between the majority and the marginalized for Longino, and those differences plus language barriers for Nelson's global community. So how do such groups communicate their differences in a way that impresses their unique perspectives upon the majority? It will not do to simply permit a pluralism of languages and conceptual schemes.

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[45] Longino, *SSK*, p. 76

This "live and let live" approach violates Longino's requirement that the objective scientific community is one that both facilitates self-criticism (of evidence, methods, assumptions and reasoning) and is responsive to it. One cannot respond to criticism of which one struggles to make sense.[46] Nor will it do to translate all such criticism into the vernacular of the majority. As I have stated above, this runs the risk of making inarticulable the very aspects of the experiences of the marginalized that are supposed to be essential to critical dialogue. It is also a form of intellectual imperialism that may ultimately strengthen the hegemony of the dominant group, while making it appear that the objections of the subordinate have been taken into account. Finally, we can by now feel reasonably assured that there will be no neutral discourse or "protocol language" into which the evidence, methods, assumptions and reasoning of all parties can be translated for purposes of scrutiny.[47]

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[46] Kathleen Okruhlik suggested that this may appear to be a worry about incommensurability of language and conceptual schemes between the majority and the margins; I intend it as a more practical concern about how to articulate difference in a meaningful way. It may be that narrative approaches can contribute to the resolution of this problem.

[47] The need for a protocol language was advocated by Rudolf Carnap as part of the positivist project; Neurath, however, rejected the idea that such a language ought to be grounded in *individual* perceptions ("spark, explosion, smell of ozone here now"), making his position in some sense a precursor to Nelson's. See Peter Galison's "History, Philosophy and the Central Metaphor" (1988)

There are further serious problems with the notion of the community as primary epistemic agent. Nelson's reliance on a fairly conventionalist view of language means that she will have difficulty dealing with novel usage. Her insistence that individual knowledge is derived from community knowledge introduces a related obstacle, namely that it becomes hard to explain how new or original beliefs, including feminist ones, could ever emerge. Nelson insists that individuals know only *because* the community knows or, to use her phrase (which she borrows, unacknowledged, from Peirce), individuals know only derivatively. "My claims to know are subject to community criteria, public notions of what constitutes evidence, so that, in an important sense, I *can* know only what *we* can know, for some we", she writes.[48] Communal standards "constrain what it is *possible* for an individual to believe as well as the theorizing we engage in together."[49] The simplest and most effective way to explain the emergence of new or original beliefs is, in my view, to attend to relevant features of individuals treated in a non-derivative way. As Lorraine Code points out, knowers are not like computers. The knowledge that we store in our heads is shaped and altered by our attitudes.[50] Although those attitudes may be formed in the crucible of

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[48] Nelson *WK* p. 255, emphasis in text

[49] ibid. p. 277, emphasis added

[50] Code, *ER*, p. 26

the community, the fact that attitudes may differ between individuals suggests that their formation is more likely to be explained by a double-aspect approach to knowledge than by either an exclusively individual- or community-based one.

Supposing that Nelson can explain how original beliefs are possible, there is still a dilemma to be overcome regarding what happens to such beliefs. If one can believe only what one's community warrants, and such warrant is not forthcoming, then one can either go looking for a new epistemic community, or try to convince one's present community of the worth of a particular belief on other grounds, thereby bringing about a change in the community's shared background. Thus, like Quine, Nelson holds that knowledge and the criteria for its justification evolve alongside one another.[51] If feminist beliefs are to have any real social and political impact, it cannot be the case that feminists are just looking to share in the building of a new community with alternative epistemic standards. We must be seeking to convince the mainstream of the worth of feminist beliefs according to epistemic standards that the community already accepts. This much is implied by Nelson's argument that empiricists must take feminist science criticism seriously as part of the evidence for science.

As the passages quoted above indicate, it is not always clear whether Nelson is committed to the view that the

[51] ibid. p. 313

community warrants styles of knowledge-seeking, or the content of individual beliefs. It strikes me that many communities do both, depending on the belief, although they may disguise an objection to content in the form of an objection to method. Since this is where many feminist claims run into trouble, Nelson ought to say more about the nature of warrant and individual beliefs. She argues that because evidence is public, and because what constitutes evidence is communally determined, the individual is constrained in her beliefs by community standards, therefore "it is clearly *we* who know".[52] At most, however, this argument establishes that it is clearly we who certify knowledge claims.

The overarching attempt to build connections between language and the world is problematic in part because of its association with foundationalist epistemological projects.[53] Quine claims that he is not a foundationalist, and Nelson simply repeats his claim. However, Donald Davidson has argued that Quine's naturalism remains, in some ways, foundationalist, and moreover essentially individualist.[54] Davidson argues that there is a blurring in Quine's work between giving an empirical account of how

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[52] ibid. p. 277

[53] I am indebted to Sharyn Clough for her insight into Davidson and the bearing of his views on Nelson's Quineanism.

[54] Davidson, "Epistemology Externalized" (1991)

knowledge claims emerge, and stating the epistemic norms that beliefs are supposed to satisfy in order to be certified as knowledge. This merging of description with justification is carried over into Nelson's position as well, in as much as she wants language to do justificatory work. Davidson has argued that because the application of concepts determines their content, and not vice versa, this sort of justification is incoherent.[55] Nelson does not discuss Davidson, nor any of Quine's other influential critics.[56]

Nelson's notion that individual knowledge is derived from the community runs aground on some of the very objections that she makes to Cartesian individualist epistemology. The Cartesian subject is rejected as passive, allowing the self-announcing evidence of the world to wash over him, and playing an active role only in the act of will necessary to the acquisition of belief.[57] But the individual whose knowledge derives from the community is similarly passive, existing as a sort of empty vessel into which the language, conceptual scheme, and epistemic

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[55] Miranda Fricker has recently offered a feminist reconstruction of Quine that is somewhat more sensitive to Davidson's concerns. See Fricker, "Knowledge as Construct" (1994)

[56] Wittgenstein's views on the connection between knowledge and language might also be helpful to Nelson, but she does not address them.

[57] as discussed in Nelson *WK*, pp. 304-306

standards of the community are poured. Nelson cannot even hold that the individual's experience plays a role in how this communal material will be utilized, because the very possibilities for experience have already been constrained by the standards of the community. Nelson's view makes us seem like victims of belief.[58] As I will argue in the next section and in more detail in the next chapter, diffusing knowledge throughout the community without insisting on equal roles and responsibilities for individual knowers is dangerous.[59]

A further peculiarity of the communities-first project shared by Nelson and Longino lies in its implications for naturalized epistemology. Nelson and Longino both make a point about the naturalist project that is rather odd. Nelson, who advocates a revisionist Quineanism, argues that even if we accept Quine's proposal (that the mechanisms of belief acquisition lie in our neurophysiology, hence epistemology ought to be done in cognitive science labs),

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[58] Sociologists of science have already noted that treating science as a communal activity is not helpful when the idea of a science community is construed in certain ways. See, for example, Karin Knorr-Cetina, "Scientific Communities or Transepistemic Arenas of Research? A Critique of Quasi-Economic Models of Science" (1982)

[59] Paul Forman examines the "trade-off" that allegedly took place when scientists sacrificed their individual moral autonomy in favour of the intellectual authority of their discipline, and states the problem very clearly: "The moral responsibility that the individual scientist lays down is not, however, shouldered by the discipline. *Only individual human beings carry moral responsibility*." Forman (1991) p. 74. I discuss this trade-off in more detail below.

naturalized epistemology still ought not look to the relationships between the neurophysiological states of individuals and events in the world.[60] Then where should it look? To the neurophysiological states of communities? That would be impossible. Communities do not have such states. Similarly Longino claims that "it is not the individual's observation and reasoning that matter in scientific inquiry - it is the community's".[61] But in what sense can it be said that a community reasons? How does a community observe? It is one thing to attempt to shift the focus of epistemology to the community, but it must be recognized that in so doing, one cannot bring along individualist notions like reasoning and observation without doing serious damage to them. It is clear that Longino and Nelson are committed to a radical reconception of what it means for someone to have experiences and beliefs, and their simple shift to a community-oriented epistemology does not comfortably accommodate such a reconception.

Endorsement of the epistemic primacy of the community is hard to reconcile with one of the central tenets of feminist epistemology, that it matters who knows.[62] The feminist conviction that this issue matters to epistemology

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[60] Nelson *WK* p. 283

[61] Longino, "Essential Tensions-Phase Two" (1993b) p. 265

[62] Nelson *WK* pp. 265-66, and Code, "Is the Sex of the Knower Epistemologically Significant?", reprinted as the first chapter of *WCSK?*

is based on an assessment of knowers which takes both individual and group characteristics into account. On the one hand, the answer to Code's question, "is the sex of the knower epistemologically significant?" must be "no" for any theory of knowledge that insists that the knower is the community, because communities do not have sexes. And if individual knowledge is derivative as Nelson says it is, then the sex of the individual knower can have at best only indirect significance, because beliefs about the sex of the knower will themselves be derived from communally shared beliefs. It will not help to shift to considerations of gender instead of sex and may in fact make matters worse. It is true that one's experiences of both sex and gender have an ineliminably social element, and that there are public standards of what it means to be both a woman, and feminine. But note that to fail (or to succeed) in adhering to appropriate standards of gender still permits one's knowledge claims to be discredited (or accepted) on account of one's sex. If I demonstrate an ability to meet all of the communal standards of so-called masculine reason, my views may still lack credibility because I am female. And although the rules for picking me out as a female may be public and shared, it is I, the individual, whose knowledge claims will be undermined. Our individual attributes, including our sex, are not incidental to our places in dominance hierarchies.

Longino has a requirement that is supposed to prohibit such failings: everyone who participates in the critical dialogue constitutive of science must be treated as equal in terms of intellectual authority.[63] In practice, however, this condition may be too weak precisely because intellectual authority is something that is supposed to accrue to individuals. The group may hold a rule about the authority of all individuals, but when adjudicating a particular knowledge claim, members of the community must put themselves in the position of asking whether the intellectual authority of this *particular* individual is being taken seriously. Otherwise, it will be too simple to just fall back on the rule and assume that because we take everyone's intellectual authority to be equal, then it must be the case that *this* person's authority is being taken in that way. In fact, something very like this occurs when scientists fall back on the assumption of the trustworthiness of other scientists, even though it is not always wise to do so. I explore this matter further in chapter three. The overlap between individual and community on the question of authority provides a strong reason to give individuals and communities equal consideration.

The identification of primary and derivative agents of knowledge is distressing because it embodies the hierarchical and reductionist approach to knowledge, and to

[63] Longino *SSK*, p. 76

science in particular, which feminist theorists have long resisted. Unfortunately, it is all too easy to repeat the sins of individualism at the level of the community. The Cartesian project may rely on untenable abstract subjects instead of actual individuals, but Nelson and even Longino sometimes treat the community very much like a Cartesian subject, as much a featureless abstraction as the individual it is intended to displace. What is needed is more attention to the details of both individual and community behaviour as it figures in the acquisition or construction of knowledge. I conclude this chapter by considering what I take to be some of the more interesting details of individual behaviour as it bears on the feminist project in science.

IV. Why Do Individuals Matter?

In order to fully support my contention that a double-aspect strategy, in which both individual and community have equal significance to knowledge, is the superior approach, I will look at some reasons why individuals are still too important to be thought of as secondary. The most obvious point to be made here is that the notion of autonomy that customarily goes along with a Cartesian model of individuals is an ideal that feminists ought not sacrifice, or at least not yet.[64] Although there is much debate in feminist politics as to the efficacy of using patriarchally derived

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[64] Code makes this point in *WCSK?*

concepts like autonomy and agency to promote the cause of women, we can acknowledge their value as part of a transitional scheme in working towards the liberation of women.[65] It is important to establish the bodily autonomy of women in order to secure reproductive rights, their civil autonomy in order to secure legal rights, their intellectual autonomy for education rights, and so on. It is possible that the social and political equality of women can be accommodated within a framework of knowledge where the individual loses ontological and logical priority to the community, but at the very least, pro-community epistemologists owe an account of how social and political beliefs about individuals are to be derived and in what their special force for women will consist. The problem is not that community-based accounts of knowledge pose an automatic threat to individual autonomy.[66] Under some constructions such accounts may threaten autonomy, while under others they may not. My point here is that holding the community and the individual on a par for the purposes of epistemology should heighten our awareness of the importance of individual autonomy from the outset.

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[65] Audre Lourde sums up this worry succinctly in the title of her article, "The Master's Tools Will Never Dismantle the Master's House" (1981) from *This Bridge Called My Back: Writings by Radical Women of Color*, Moraga and Anzaldua (eds), pp. 98-101

[66] My thanks to Kathleen Okruhlik for pressing me on this question.

Secondly, while it may be true that there are public standards, rules, and concepts, the learning of which constitutes one's apprenticeship in an epistemic community, it is also true that the ability and commitment to perform according to these rules and standards is an individual matter. Recall Lorraine Code's point, mentioned above, that we are not merely storage bins for beliefs and epistemic background assumptions. Stored knowledge is shaped by the attitudes of the knower. The community may be the source of many of those attitudes, but it is unlikely to be the case that no one ever transforms or executes their beliefs in new or unique ways. If it were, then we might in principle be able to determine in advance many of the beliefs possible in a given homogeneous community.

Both Nelson and Longino appear to regard some stored knowledge, namely the shared epistemic background assumptions of a homogeneous community of knowers, as immune to further attitudinal shaping, but it is not clear why this should be so.[67] Even if all members of a group of knowers

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[67] There are echoes here of the Churchland-Fodor debate on the theory-ladenness of observation, which remains unresolved because the participants mean different things by theory. Churchland has in mind information of the sort that is likely to alter one's ability to perceive things in a certain way, while Fodor means the perceptual algorithms hard-wired into the brain. Neither of these is so embedded in our epistemology or neurophysiology that they are in principle closed to discovery, but only what Churchland means by theory is available for the kind of communal negotiation that Nelson and Longino have in mind. See Fodor (1984) and (1988) and Churchland (1988).

share a belief in the value of, say, a certain rule about evidence, it does not follow that all members will apply that rule the same way in every case, nor even that they are as strongly committed to the rule. To presume otherwise is to treat the community as a substitute for a featureless Cartesian subject. While Nelson and Longino are correct in arguing that it is the community that adjudicates knowledge claims, even though it often appears otherwise, they must also heed the degree to which following or departing from the rules is a matter of individual behaviour. The performance of the individual is important, because it is individuals who do the learning, observing and reasoning that knowledge requires. What is more, they sometimes do it in very innovative and unusual ways. Thus if there is to be any room at all for creativity in knowing, it must come at the level of the individual. It is hard to see how there could be rules governing creative thought, much less communally negotiated ones. It is not helpful to claim that creativity consists in innovative but acceptable departures from communal norms.

Unfortunately, it is also the case that individuals sometimes do not perform as the methods and criteria of their community require, and try to stake knowledge claims anyway. The commitment to community priority in epistemology seems justified only under the assumption that individuals by and large learn what they are supposed to learn and follow the rules. But this assumption is wrong, as individuals often depart, sometimes dramatically, from the sorts of knowledge-seeking behaviour that their community warrants. Some such cases are relatively benign, because everyone in the community departs from a particular standard. This amounts to saying that although there may be an explicit requirement to follow certain rules, that requirement is almost always waived by tacit agreement. In such cases there is a disparity between what the community says the rules are and what they really are, but so long as everyone recognizes this gap, the rhetoric of the community will not prevent anyone from achieving knowledge. Should the community, or the majority of its members, use rhetoric to conceal the real rules from certain of its own members or to outside observers, then such departures from community standards become far more ominous. I will have more to say about this in the next chapter. Other instances of failure to respect the group criteria for knowing will be evidence that community practices undergo continual change and revision, as both Nelson and Longino acknowledge. A healthy scientific community will therefore be one that leaves room for difference and creativity, qualities that inhere at least in part in individuals.

Some violations of communal norms of knowledge are plain chicanery. Where there is serious epistemic wrong-doing in a community, even by its own genuine standards, the explanation may sometimes be that an individual decided to deliberately overlook customary norms, and made this decision not in order to challenge communally held principles, but for some other unsavoury end.[68] For example, such a person may feel forced by the pressures of his or her profession to depart from accepted practice (e.g., pressures to publish frequently, to provide evidence for widely favoured hypotheses, to conserve scarce laboratory resources, etc.). Although these are surely forces to which the community contributes, it cannot be the case that the *community* sanctions epistemic deception in order to meet its non-epistemic goals, because not every individual cheats, and the community does not overlook every case of cheating. (To her credit, Longino does discuss the ways in which epistemic and professional/commercial goals and norms are entwined and sometimes in conflict.[69])

If fraudulent behaviour by individuals is to be considered wholly derived from communal rules and permissions, then epistemic "free riders" may be given the opportunity to sin with impunity. The community can be used to conceal or excuse the failings of some individual, by making individuals harder to find. A community model could in principle add a further burden of social responsibilities

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[68] Those scientific frauds that have been exposed virtually always involve individuals acting alone. See Broad and Wade (1982) and LaFollette (1992).

[69] Longino, *SSK*, p. 91

to the individual's moral load, but as things stand, real scientific communities do not do so. As will be seen in the next chapter, communal concealment of individual failings does occur and, not surprisingly, gives rise to passionate disavowals and bitter recriminations within the scientific community. The problem is that the model of community primacy to which Nelson and Longino are committed is ill-equipped to deal with these dilemmas. For one, the community-first approach does not enable us to discern readily between those incidents where flouting of customary norms is harmless or even beneficial, and those where it conceals oppressive exclusions from epistemic negotiations, or the blatant pursuit of self-interest. Attaching epistemic primacy to the community suggests that individuals cannot oppose the general consensus with any authority, even where there is general deceit and the individual knows it.[70]

I am not arguing that the scientific community as a whole should not be held accountable for transgressions that occur within its boundaries, just that it is not only the community that should be held accountable. Code argues for a model of knowing in which members of a community are not absolved of responsibility for their lives, but in which the responsibility is not theirs alone.[71] I am making the same

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[70] I am indebted to Rich Campbell for this point.

[71] Code, *WCSK?* p. 276

point, from the opposite direction. A double-aspect approach ensures that neither the community nor the individual will be permitted to fall through the cracks in the construction of knowledge, nor in the responsibility (and sometimes blame) that goes along with such knowledge.

The question of trust is pivotal in this respect. In order to become productive knowers in an epistemic community, each individual must face numerous sometimes conflicting evidential, methodological and doxological demands, and often must decide between them. One factor that may be implicated in such deliberations is the source of one's potential beliefs. If an instruction or proposition comes from a reliable source, it is likely to be taken up, otherwise not. And just as in the case of what counts as gender, there will be social standards for what counts as reliability.[72] Science is supposed to be a reliable source. Parents and teachers, for example, are thought to have a special duty in this regard, as they are responsible for the forming of young minds. Doctors, lawyers, and accountants occupy roles in which it is crucial that they elicit our trust. But when it comes to the individual, who must decide on the strength of another person's testimony whether to adopt a particular belief, method or rule, it will not do to base one's decision only

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[72] Hardwig, "Epistemic Dependence" (1985) and "The Role of Trust in Knowledge" (1991)

on the social criteria of credibility. One must consider carefully whether this person's testimony is reliable because, as was the case for cognitive authority, taking trust into account means taking individuals into account. While it is clearly the case that some individuals are trusted as a consequence of the social roles which they occupy, it is equally clear that some of the individuals in such roles ought not be trusted. Assessments of trustworthiness and reliability must include a sense of what sort of person this individual scientist (doctor, lawyer, judge, accountant, etc.) is.

Finally, although I hesitate to offer such an overtly individualist argument, it is plausible that one's response to Nelson's claim that knowledge is communally held will be conditioned in part by one's own status and even character. Nelson endorses Kathryn Pyne Addelson's account of the importance of social arrangements to the privileging of the cognitive authority of scientists, especially elite scientists, over others.[73] A scientist at the top of what Addelson calls a "prestige hierarchy" will have greater exposure to the work of his or her underlings, the connections between the work done in his or her lab and that done in other labs, the influence of that work on the institution's reputation, funding, on social policy, and so

[73] Addelson, "The Man of Professional Wisdom" in Harding and Hintikka (1983)

on. Hence members of the scientific elite may be more likely to assent to the claim that scientific knowledge is communally constructed. Similar hierarchies exist within feminist social arrangements. Academic feminists often have greater access to the institutional resources (books and journals, the internet, conference announcements and travel funding) that enable them to participate in, feel part of, and sometimes enable others to feel part of, a feminist knowledge community.[74] But many women who identify and sympathize with feminism, restricted here to academic feminism, may lack a strong sense of community, and thus may feel less convinced that knowledge is held by communities. In Canada, for example, there are still many philosophy departments that employ few if any women, offer few if any courses in feminist philosophy, and lack strong cross-disciplinary connections with women's studies departments (where they exist). The further complications of Canada's geography and regional politics may also affect how likely one is to see one's own efforts as continuous with those of the feminist community. There may be very credible individual reasons why these obstacles to the perception of community will be difficult to overcome. The internet may be a great place to make contacts and discuss issues, but some people are technophobes. Others are just shy. Since Nelson is a strong advocate of broadening our consideration

[74] Nancy Tuana made me aware of this point.

of the science community to include our entire community, including its values and politics, it strikes me that she ought to take factors of this kind into account.

There are real gains to be made by bringing the community within the purview of epistemology and philosophy of science. I have suggested in this chapter that it is futile to consider the community in isolation from the individual, because this neglects the extent to which the community and the individual must interact. Nelson thinks that her model, in which individual belief is derived from community knowledge, captures the magnitude of such interaction, but I have argued that it does not. The very idea of derived-ness is at odds with authentic interaction, as it permits too little input into the construction of knowledge by the individual, and severely curtails the influence that individuals are permitted to have. A more robust form of interaction will be one wherein the individual's more extensive role is acknowledged. Many communities overlap, and the individual is often the pivot between them. However, it must also be recognized that on some levels and with respect to some questions, it is not the case that every individual interacts with his or her epistemic community as an equal partner in the knowledge-seeking enterprise. It is in this light that we must emphasize Longino's observation that the social character of knowledge functions both to insulate knowledge from interests and values, and to make knowledge vulnerable to interests and values.[75] By combining this insight with my argument for a double-aspect epistemology, we gain an understanding of the community in which the strength and nature of interaction with individuals varies along several dimensions. This will in turn begin to give some depth and distinctiveness to communities themselves, rather than treating them as abstract pseudo-Cartesian agents as Nelson and Longino sometimes do. I will resist saying anything more general about epistemic communities because it is now apparent that nothing in general can be said about them. We must observe them in action.

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[75] Longino *SSK*, P. 12

CHAPTER THREE

BAD SCIENCE: IT'S BETTER THAN YOU THINK

Criticism of "bad science" has long been a (perhaps the) noteworthy feature of feminist approaches to science. Feminist philosophers and feminist scientists have examined, rejected, and corrected false, biased claims, especially those comprising the bulk of knowledge about women in the social sciences and in biology and medicine. This remedial work will continue to challenge widely held assumptions well into the future. But many feminists have also noted a tension between this corrective agenda and a more sweeping project that would question science at a deeper level, or science-as-usual, as Sandra Harding calls it. Harding outlines what she thinks are the sources of this tension, and defends the need for dialogue between the two approaches. To some extent, Lynn Hankinson Nelson accepts and reacts to Harding's characterization.

In this chapter I am going to try to contribute to a dialogue between these two approaches by plumbing the murky depths of the problem of bad science. Although I agree with Harding and Nelson that merely remedial criticism of bad science is inadequate, there can be a great deal more to such criticism than simple correction. I will argue that bad science is rather more perplexing and more revealing than is often acknowledged, and that a richer understanding of it will enable us to see many of the inadequacies of science-as-usual (and our understanding of it) in sharper relief. As I see it, bad science is not a practice different in kind from good science. It is rather an exaggeration of tendencies present in science-as-usual. Bad science is continuous with science-as-usual and cannot be criticized in isolation from it. As I have suggested in the previous chapter, bad science is also a focal site for learning more about the interplay of individual scientists with their communities. The question of individual motivation cannot be removed from criticisms of science. Confronting bad science can thus be an important instrument in resisting and disrupting science as an oppressive force in women's lives.

I. BS vs. S-A-U

Sandra Harding distinguishes between critics of bad science and critics of science-as-usual, and points out that although a few feminists explore the relationship between the two critiques, most pursue only one.[1] She is careful to point out that there is nothing wrong with having two points of view, even if they sometimes seem to be in conflict, because the goal of the feminist critique of science is plausibility, not "a mystically transhistorical

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[1] Harding, *WS?WK?*, p. 54

epistemology",[2] and what is plausible will vary according to one's audience. Nevertheless, she appears to acknowledge, at least tacitly, that the friction between the two kinds of critics can be resolved. I think that we can achieve this resolution by exploring the ways in which our very understandings of science-as-usual are informed by confrontations with bad science.

Harding's reading of the tension between the two critical strategies is a good starting point. On the one hand, she claims, critics of bad science see science as a formula-driven activity rigidly constrained by methodological precepts. Bad science occurs when scientists fail to follow the rules and, it is assumed, such failures are normally caught. Built into the formulae upon which science relies is a set of self-correction mechanisms, designed to minimize the potential for errors of haste, intellect, or instruments. For example, experiments are subject to numerous controls, including repetition and publication. Experiments must be repeated in order to establish that observed evidence is not the result of faulty experimental design, equipment malfunction, failure to control variables, accident, and so on. Results must be published, together with such information as is necessary for the interested reader to reconstruct and verify the experiment independently in her or his own laboratory, and

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[2] ibid. p. 114

such publication is peer-reviewed.[3] The structure of science thus ensures that errors will not stand, but, argues Harding, this structure will eliminate only individual biases and assumptions, and not culture-wide ones.[4]

In her earlier work,[5] Harding tended to be quite hostile to the idea that challenging bad science could be of any value as a strategy for feminist critics of science. It would not be unfair, I think, to suggest that she characterizes her own initial attitude toward bad science critics when she remarks that they are a distraction to critics of science-as-usual. More recently, however, she has conceded that the attack on bad science has at least instrumental value in that it is more likely to be palatable to audiences antagonistic to feminism.[6] For these audiences, feminism turns out to have not whatever alarming goals they had imagined, but much the same praiseworthy goals as good science.[7] This is, however, a strategy with limited applicability, and may perhaps reassure scientists

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[3] It is worth noting that Harding does not explicitly refer to any of these as norms of method, nor does she specify what she takes the norms of science, of which she is critical, to be. I discuss this further below.

[4] Harding *WS?WK?* p. 115. A careful study of bad science indicates that the existing structures do not catch many problems at all, whether individual or culture-wide, fraudulent or accidental.

[5] Harding, *SQF* (1986)

[6] *WS?WK?*, p. 66

[7] *WS?WK?* pp. 112-115

in an inappropriate, if not downright misleading way. That this issue must be confronted by feminists and critics of bad science indicates where the ground for further debate over the standards of good and bad science lies.

The critic of science-as-usual is not interested in eliminating bias through stricter adherence to existing methodological norms. She wishes to examine the ways in which the norms themselves "have been constructed primarily to produce answers to the kinds of questions an androcentric society has about nature and social life, and to prevent scrutiny of the way beliefs that are nearly or completely culturewide in fact cannot be eliminated from the results of research by these norms".[8] For Harding, then, only critics of science-as-usual possess an adequate awareness of science's social embeddedness, and only they can use this awareness as a liberating resource in questioning the ethics, goals and functions of science. Nelson echoes this stance with her argument that it is only from a consciously political perspective that one even notices the inadequacies of, for example, research into sex differences, and may begin to call such research into question.[9] (On the face of it, a consciously political perspective is not open to the empiricist, so Nelson must find some way to revise empiricism for feminist purposes. I discuss her efforts to

[8] ibid. p. 117

[9] Nelson, *WK*, p. 202

do so in chapter four.)

Harding objects that those who think bad science is the only problem support the goals of value-neutral, objective and impartial scientific inquiry. She does not specify to whom this objection is meant to apply, so it is difficult to assess her claim. Are there any feminist science critics who believe that bad science is the only problem women must confront? Perhaps it would be more appropriate to chide such critics for their general naivete than to find fault with their alleged commitment to the "principle of abstract individualism that grounds conventional theories of science".[10] In any case, the dialogue between critics of bad science and science-as-usual can only be advanced by expanding our understanding of the notion of bad science.

While it is true that many critics of bad science do tend to regard science as the method-driven process that Harding describes, especially those who are themselves scientists of a conventional empiricist brand, they need not accept the rigid limitations Harding thrusts upon them. She insists that critics of bad science are wedded to the view that science is its rules, and that such critics belong to a historical tradition in which products of the mind, like theories and hypotheses, are regarded with suspicion. This caricature is based upon a terribly narrow reading of Bacon and Newton and their proposals for sound induction. As I

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[10] Harding, *WS?WK?*, p. 62

argued in chapter one, it would be a grave mistake to take the pronouncements of Bacon and Newton as definitive for empiricist science, because their views were the subject of widespread and ongoing disagreement, and were also peculiarly British.[11] Moreover, Bacon and his fellows could hardly have foreseen the opportunities for doing science badly that have arisen alongside its contemporary institutions and structures (grant agencies, tenure, peer review, etc.). Certainly contemporary scientific method takes the design and testing of hypotheses to be one of its most central goals, while still recognizing that there are limits on how and why hypotheses may arise.[12] Harding's tendency to speak in general terms about the norms of scientific inquiry, without specifying what she takes those norms to be, makes it more difficult for her to recognize disputes over and changes in those norms. The criteria for what counts as bad (and good) science are in continual flux.

What is more, the criteria for what counts as good science also vary over time and between disciplines. A belief or practice in or about science may be regarded as bad at one time or according to one set of standards, and

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[11] For example, when Darwin published his *Origin of Species* in 1859, debate in Britain coalesced around the question what kind of hypotheses were acceptable, not whether they were acceptable at all. See Hull (1973) and Ellegard (1958)

[12] This is made abundantly clear in the case of cold fusion, discussed below.

good at another time or according to another set of standards. I (in common with a great many others) would insist that there is good, even excellent, science as judged by current standards. Harding seems to want to resist this view. She allows that feminist work that has reevaluated existing research on women is "true, or at least less false" than the views such work displaces.[13] That work shares in some of the normative assumptions that she identifies as problematic, although probably not in all of them. The possibility of genuinely good science, if it exists at all, seems for Harding to be far in the future.[14] This gives rise to something of a paradox. On what grounds are we to convince "other Others", i.e., those whose interests and experiences have historically been excluded from science, that it is worth their while to add their voices to the new scientific dialogue, as Harding argues it is? The appeal of such participation relies very heavily on its newness, and little else.

After painting a very limited picture of what the critique of bad science is or can be, Harding goes on to

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[13] Harding *WS?WK?* p. 112

[14] The distinction between good and bad science is too simplistic anyway. If one takes seriously the "Ortega Hypothesis", i.e., that the vast majority of published scientific work has virtually no impact on the growth of scientific knowledge, then one should concede that science ought to be distinguished into its good, bad and inconsequential forms. See Cole and Cole, "The Ortega Hypothesis" (1972)

extend this limitation to empiricism, especially feminist empiricism, by conflating it with such criticism. She writes that "feminist empiricists argue that sexist and androcentric biases can be eliminated by stricter adherence to *existing* methodological norms of scientific inquiry",[15] as though feminist empiricism could have no program for the improvement of scientific norms, and as though empiricist philosophies of science are in general incapable of such refinement.[16] Feminist empiricism accepts the view that science is a norm-governed process wherein the norms of inquiry are variable, and that part of the process of science includes the negotiation of what the governing norms will be. This position is combined with the conviction that feminist insight is an indispensable instrument in such negotiation. Feminist empiricism is not just the elimination of bad science by appeal to an already existing and static set of rules. For many empiricists, including and perhaps especially feminist ones, the norms of inquiry are part of the empirically contested ground of science. Should any methodological rule or assumption prove

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[15] *WS?WK?* p. 111, emphasis added

[16] Harding introduced the distinction between feminist empiricist, feminist standpoint, and feminist postmodern epistemologies in her influential book *The Science Question in Feminism* (1986), and has continued to refine it in subsequent work. Although I do not address feminist standpoint and postmodern epistemologies in this essay, I note that it is Harding's characterization of the strengths and weaknesses of all three positions that has defined feminist work in this area.

inadequate to the goals of science (themselves subject to ongoing adjudication), then it must be improved or rejected. The fact that empiricist norms (and goals) of science have changed over time supports this view.[17] Harding misses the possibility of such change through her inclination to treat empiricism and its standards as a monolithic, unalterable given. The norms of science emerge through the process of doing science, including doing it badly.

However, Harding's approach to the tension between criticism of bad science and of science-as-usual contains a valuable, albeit somewhat hidden, insight. Although Harding herself never states the case in this way, her discussion highlights a crucial deficiency in standard defenses of scientific practice. Scientists and scientific institutions do not, for the most part, spend much time trying to articulate theories or philosophies of science. When science is threatened, however, scientists do tend to respond along the traditionally empiricist lines that Harding has described.[18] That is, they appeal to adherence to the right rules of scientific method, especially its mechanisms of self-correction, as providing the warrant for

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[17] Contemporary theorists of science are not even in agreement over such fundamental questions as the level at which norms of scientific inquiry operate. I discuss this further in the next chapter.

[18] Such a response can be found in Gross and Levitt, *Higher Superstition: The Academic Left and its Quarrels with Science* (1994)

their claims.[19] By setting up the opposition between critics of bad science and of science-as-usual in the way she does, Harding throws light on the failure of the two parties to engage over the same issue. There may be plenty of peer-reviewing and attempted replication going on, but where is the active identification and questioning of background assumptions on which Harding, Nelson and Longino all rightly insist? Self-correction is not self-criticism, and while empiricists cannot legitimately claim to have fulfilled demands for the latter by providing only the former, non-empiricists cannot take the empiricist provision of the former as evidence of their failure to provide the latter. Science requires both self-correction *and* self-criticism, and the dialogue between the two sets of critics will be improved by recognizing that each contributes to the satisfaction of a different set of demands. This recognition can break the impasse and permit both projects to move ahead, sharing concerns and, it is hoped, solutions. I suggest that the lack of self-criticism which plagues science accounts for the failure of scientists (and their critics) to take the crucial task of self-correction

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[19] Feminists must be very careful not to disavow scientific method altogether. Anti-method rhetoric has been used to defend some pretty unsavoury research, including Gordon Freeman's infamous work on feminism and the deterioration of modern society. ("Kinetics of Nonhomogeneous Processes in Human Society: Unethical Behaviour and Societal Chaos", *Canadian Journal of Physics* vol. 68 no. 9, Sept. 1990) One reason to preserve the importance of rule-following is to enable sharp, rapid criticism of shoddy research.

seriously.

Nelson is careful to use the label "bad science" with the further proviso "as that charge would currently be understood".[20] Her arguments about bad science range over a number of issues, some of which bear on the strength of the feminist empiricism she defends. In several places, she shows that it would be inappropriate simply to "write off" biased research as bad science.[21] Her argument underlines the difficulty that feminists face in identifying what it is about bad science that is objectionable. Feminists cannot, for example, claim that gendered political perspectives are inherently distorting, because feminist political perspectives have enabled both criticism of science and the proposal of alternative theories.[22] Nor can feminists object to the ways in which some especially androcentric theories have been constructed, without placing unreasonable limitations on science as a whole. For example, sociobiology borrows the tentative hypotheses, observations, and results of other fields, and builds on them to draw new conclusions.[23] But as Nelson points out, we cannot reject

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[20] Nelson, *WK*, p. 189, 223

[21] Nelson, *WK* pp. 204-205, 212, 223

[22] ibid. p. 212; Donna Haraway (1991) makes a related point, that simply noting a connection between biological and political/ economic discourse is not a good argument for dismissing such biological argument as bad science or mere ideology. (*Simians, Cyborgs and Women*, p. 98)

[23] Nelson, *WK*, p. 201

the practice of intertheoretic borrowing as a mark of flawed science, because it is a commonplace practice and often leads to important breakthroughs.

While Nelson is correct to draw our attention to these important issues, her own brand of feminist empiricism provides the tools to interrogate Harding's quick dismissal of bad science in more depth. We cannot reject sociobiology just because it borrows from other fields. But with her insistence on the interconnections between common sense, standards of evidence, and going theories, Nelson could easily argue that it is not the borrowing that is problematic for sociobiology, but rather what is borrowed. Sociobiologists proceed as though the interconnections to which Nelson and Quine refer do not exist, that sociobiology is not bound by the evidential constraints of the disciplines from which they borrow, and that it is under no obligation to acknowledge that the hypotheses and results it borrows are often highly tentative and controversial.[24] We can extend the importance of the Quinean web to bad science in general, and argue that no research program can be judged in isolation from other going theories and political context. As Nelson puts it, "the judgements 'good science' and 'bad science' are warranted not by the passing or failing of some simple test but by careful, multifaceted

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[24] These issues are considered in Howe and Lyne (1992)

evaluations".[25] Harding's claim that the norms of science have been constructed both to advance and conceal androcentric interests, if true, is itself suggestive of a norm of science. Nelson's feminist empiricism enables us to challenge that norm, provided that we take seriously the emergence of norms from practice.

II. How Far Can Rules Take Us?

Even if empiricists did adhere to the overly simplistic model of good science as rule-following, it is worth examining the likely impact of the enforcement of science's rules on its results. It is my view that a demand for better rule-following will take us further toward improving science than is often acknowledged to be the case, especially we expand our understanding of what it means for scientists to follow rules. Harding recognizes that "research intending to reevaluate women's nature and roles and the social dimension of gender certainly meets overt standards of 'good research'".[26] Like virtually all feminist theorists, she understands the value of the corrective claims of feminist research in biology and the social sciences. Like many feminists, her frustration is evident when she remarks that "women's movements have been removing covers and blinders from eyes in the West at least since Christine de Pisan wrote *The City of Ladies* in the

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[25] ibid. p. 205

[26] Harding, *WS?WK?* pp. 112-113

fifteenth century, yet we still live in a world ruled by powerful old naked patriarchal emperors."[27]

It is frustrating that more sweeping changes to science have not been brought about by the successes of feminist remedial work. But it is also easy to underestimate how much of that work remains undone, and how much of it has yet to surface in the relevant mainstream. It will no doubt become dull, and may even feel somewhat uncharitable to pick at every loose thread in every gender-laden scientific study (including historical ones); nevertheless the consequences of gender and other forms of bias are politically so serious as to demand eternal vigilance. It is often the initial articulation of feminist insights that later proves responsible for the displacement of long-held assumptions, even though scientists may not even be aware of feminism as a source of inspiration, much less inclined to comment upon it. For example, the feminist challenge to "Man the Hunter" models in anthropology gave rise to the inclusion of women in hunter/gatherer models,

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[27] ibid. p. 66; Pisan's *The Book of the City of Ladies* (1404), is a spirited, if essentialist, argument against the usual justifications of women's inferior status. One of Pisan's translators, Sarah Lawson, remarks that "in a striking passage that must be unique in all Medieval literature, Christine advises women to rely on their own experience for knowledge of the feminine condition and not on the ignorant scribbles of men". The sequel, *The Treasure of the City of Ladies* (1405), is a rather more depressing manual of feminine etiquette, containing such chestnuts as pretend not to notice a husband's infidelity, be sweet to your in-laws, and do not dress extravagantly.

but also to a series of questions and doubts about hominid diets and hunting practice and prowess. The current movement toward consideration of "Scavenger" models is thus a logical consequence of a chain of reasoning set in motion at least in part by feminist scientists.[28]

The scavenger case is a good one for illustrating a further point about the need for good science. Scientific hypotheses frequently fail by science's own lights, and these failures are worth noting. "Man the Hunter" models were developed in response to the need for some explanation of human tool and language use, evolution of upright posture, and so on. These skills and physiological traits supposedly contributed to the ability to hunt more successfully, and were thus selected for. "Man the Hunter" models failed by the acknowledged standards of science because they did not offer good explanations of the phenomena they were intended to explain. The development of language, or any noisy form of communication, would be more likely to hinder than to help hunters in a task demanding stealth and the element of surprise.[29] Upright posture was thought to enhance speed and free the hands so that weapons

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[28] See "Scavenging and Human Evolution" by Robert J. Blumenschine and John A. Cavallo (1992)

[29] Ian Hacking, *Representing and Intervening* (1983) p. 135. Hacking rejects the argument that language developed for practical purposes such as hunting and farming with characteristic impatience: "Scholars who favour such rubbish have evidently never ploughed a field nor stalked game".

could be used and spoils carried away. It has since been shown that erect posture limits speed, and that apes can effectively haul food and young, often at the same time, without benefit of vertical deportment.[30] The contribution of tool use to effective hunting is a fine example of anthropological question-begging, since artifacts are given the status of tool by virtue of their usefulness to hunters.[31] In spite of the obvious, in some cases acknowledged inadequacies of these explanations, they were retained because they adhered to the underlying assumption that it was the hunting activity of males which played a focal role in the emergence of distinctively human characteristics. That assumption kept investigators from seeing both that it might not be the manly activity of hunting that was important, and that activities in which females engaged might also be important, even though the evidence suggested as much. Conceptual bias leads to science that is bad by ordinary standards.

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[30] Elaine Morgan discusses the widespread acknowledgement even by evolutionary biologists that such explanations are inadequate in *The Descent of Woman* (1972)

[31] John Maynard Smith and E.O. Wilson, for example, think that the division of useful artifacts into tools (made by men) and pots (made by women) is obvious and unproblematic. Thinking of pots *as* tools would certainly expand the range of possible explanations for the evolution of tool use: gathering, storage, grinding, cooking, making offerings, collecting rainwater, appreciating beauty, etc. See Maynard Smith's review of Wilson's *On Human Nature* in *Did Darwin Get it Right? Essays on Games, Sex and Evolution*, p. 84. Thanks to Pam Martin for drawing this example to my attention.

Simple logical consistency is an excellent norm for scientific inquiry, if scientists would only use it. Londa Schiebinger's work is a rich source of historical examples of muddled logic. In the eighteenth century, relative hairlessness was still regarded as one of the traits distinguishing humans from animals. One might reasonably think that from this assumption, plus the ongoing effort to rank humans in order of excellence, it would follow that the less hairy, the more human. Women and native American men ought therefore be considered more noble than European men. Instead, the absence of chin follicles in native American men was taken as evidence that they belonged to a lower class of humans, and possibly even a separate species. Women's beardlessness was taken as confirmation of their less noble character.[32] It is indisputable that contradictions of this kind (and there are many more examples) are bad science when judged from the point of view of widely-held methodological norms.

One might object to such examples that they have little to do with rule-following and everything to do with background assumptions. Robert Richardson has argued along these lines that the presence of an ideological component can explain the persistence of certain explanations despite their lack of warrant.[33] But this objection holds by fiat

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[32] Schiebinger, *Nature's Body* (1993) pp. 120-126

[33] R.C. Richardson, "Biology and Ideology" (1984) p. 418

that social background assumptions and the methodological norms of science do not come into contact. Such a premise replicates the context of discovery/context of justification distinction that was rejected in chapter one, in that it holds background assumptions to be influential only in the context of discovery, while standards of scientific method are all that is relevant in the context of justification. But when theories tainted by androcentric bias are justified, they become part of the body of knowledge which gives rise to, and helps to justify, further equally biased hypotheses. Social bias thus plays a justificatory role. The socially conditioned assumption of male dominance on which "Man the Hunter" models and beliefs about the glories of the beard are based becomes the scientific standard by which further discoveries will be judged.[34]

There is a further issue hidden in the background here. Another factor contributing to Harding's frustration must be a commitment to the efficacy of a certain style of reasoning. It is not unreasonable to suppose that stating a general hypothesis and giving a sufficiently large number of incontrovertible examples of it should be sufficient for people to learn and accept that principle. One feminist hypothesis is that the social sciences are systemically biased by a male perspective. Feminist social scientists

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[34] Richmond Campbell, "The Virtues of Feminist Empiricism" (1994)

have amassed large quantities of convincing evidence in support of this hypothesis. They have also offered sophisticated arguments about the source of masculine bias and the reasons for its persistence. Surely there is no need to go through every page of every article, and every text, both ancient and modern, in order to expose every single instance of inconsistent, nonexplanatory, or otherwise fallacious reasoning! Unfortunately, the assumption of efficacy here leaves feminists in a quandary.

It may be the case that at some point, the remedial project in feminist science will gain sufficient momentum that it will no longer be necessary to scrutinize every word published or spoken in the name of science. We have not yet reached that point, if the rancour with which feminist correctives are received in many quarters is any indication. Perhaps more important, however, is that while critics of science have established the impact of resource allocation on the practice of science, those same critics are now faced with the problem of allocating their own resources as well. The remedial project is time-consuming and tedious, lacking in prestige but not especially costly since it involves dealing with already disseminated material. Original work, on the other hand, may be more exciting and status-enhancing, but also more expensive and more likely to require competition with other projects for scarce funding. Because the number of feminist scientists is still small, the problem of where their energies are most effectively directed is pressing. And, as Harding points out, there is even some doubt among the critics of science-as-usual as to the propriety of increasing the number of feminist scientists by advocating greater access to female-friendly science education.[35] On the other hand, wider exposure of young women (and young men) to some of the androcentrically biased claims of science has value as a means of consciousness-raising. There is no easy solution to these dilemmas.

III. Bad Science Reconsidered

In chapter two, I claimed that an examination of deliberate violations of the accepted norms of scientific inquiry would be revealing. Such an analysis is useful in bringing a scientific community's embedded background assumptions, including their assumptions about the character of individual scientists, to the fore. In this section, I will therefore depart from Harding's assessment of bad science as failure to follow the rules and discuss it instead in the context of flouting them. Given the current

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[35] Harding *WS?WK?* p. 54; the objection is that such education will serve to encourage more women to contribute more actively to their own oppression. Harding writes that women scientists are "complicitous with male domination" (p. 68), intentionally or not, when seen from the perspective of the critics of science-as-usual. I think that the force of this objection is mitigated somewhat (although replaced, perhaps, with even greater unease) by asking ourselves which women *are not* complicitous with male domination, intentionally or not.

climate in which the exposure of scientific scandal is an almost weekly occurrence, I think that a study of scientific misconduct from a philosophical point of view is particularly timely. If it is to be objected that a discussion of cheating in science is not relevant, I would respond that it is when they are breached that the tacit rules governing scientific practice become most clear. Notions of what the norms of science are ought to be informed by a consideration of the way those norms are created and altered in response to things that actually go wrong. It is here that the dialogue between critics of bad science and of science-as-usual is most likely to advance.

Progressive philosophers of science are committed to the view that science is a socially embedded process. Often, such philosophers have been satisfied that this commitment is accommodated by reliance on sociological or anthropological studies of laboratory behaviour, such as the ones offered by Karin Knorr-Cetina, Bruno Latour, Steve Woolgar, and Sharon Traweek.[36] On this model, the best way to learn about science in its natural state is to follow scientists around universities and other research institutions and observe their ongoing activities and the ways in which they interact with experimental subjects,

[36] See Traweek, *Beamtimes and Lifetimes: The World of High Energy Physicists* (1988), Latour, *Science in Action* (1987), Latour and Woolgar, *Laboratory Life (1979)*, and Knorr-Cetina, *The Manufacture of Knowledge* (1981) and *Science Observed* (1983)

instruments and equipment, students, co-workers, administrators and staff. But this approach serves to reinforce the assumption, found especially in Kuhn, that science is driven by factors internal to it. Too little notice is taken of the points at which day-to-day scientific practice intersects with extra-scientific factors. An examination of science that is committed to taking science as a socially embedded process cannot overlook such phenomena as plagiarism and deceit, for such incidents are part of the process, and it is around these incidents that the intersection of science with publishing, the law, universities and other research institutions, government, the public interest and moral values becomes most apparent.

Sandra Harding makes the very strong claim that if pressed, critics of bad science "tend to be reluctant to own that they have a theory of science at all".[37] Again, Harding does not specify who she takes these critics to be, nor does she say why they need a theory of science. Nevertheless, on this question it appears, at least on the surface, that Harding is right. Internal critics of science, that is, scientists themselves, do often express theories of science which are in some respects much like Harding describes. Many defenders of the scientific method do regard science, at least in the abstract, as a rule-driven, self-correcting enterprise. However, whether

[37] *WS?WK?* p. 64

science is actually conducted in this fashion is in no way established by the fact that scientists sometimes express such theories. Further, examination of the details of such theories as are expressed reveals that there is rather less agreement on the nature and application of the rules than one might expect. A conspicuous illustration can be found in the work of John Huizenga, a distinguished professor of physics and chemistry at the University of Rochester. Huizenga was co-chair of the US Department of Energy's panel on cold fusion following the 1989 announcement by two University of Utah chemists that they had achieved a sustained nuclear fusion reaction at room temperature.[38]

In a book written shortly after the conclusion of the DOE investigation, Huizenga's ostensible topic is the social, political, economic and scientific issues surrounding the cold fusion controversy. Huizenga argues at length, and with much repetition, for certain inviolable standards in science, and insists that it is to the credit of those standards alone that the purveyors of cold fusion were eventually perceived to be charlatans.[39] He contends that "the scientific process works by exposing and

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[38] The Utah Two were Martin Fleischmann and Stanley Pons, both of whom continue to pursue research into cold fusion abroad.

[39] John Huizenga, *Cold Fusion: The Scientific Fiasco of the Century*, (1992). Some commentators, foremost among them Eugene Mallove (*Fire from Ice: Searching for the Truth Behind the Cold Fusion Furor*, 1991), insist that the jury remains out on cold fusion.

correcting its own errors". "The scientific process is self corrective. This unique attribute sets science apart from most other activities." "The foundation of science requires that experimental results must be reproducible." "The first instinct of an experimental scientist, when confronted with an unexpected far-out result, is to try to make it go away. Every effort has to be made to track down all possible conventional explanations." "Experiments must be repeated a sufficient number of times to make sure the results are free of the obvious systematic errors and are reasonably accurate." No researcher is permitted to violate well-known principles of established disciplines, nor may miracles be invoked in order to account for unusual results. "Experimentation is the final authority in science."[40] And so on, *ad nauseam*.

Huizenga makes informal as well as formal demands of the scientific process. The right way to proceed, he says, is to present one's ideas to small gatherings of one's colleagues, permitting them to ask questions and point out errors, and then moving off campus to larger groups, and only then to the formal procedure of publication with peer review. Huizenga's suggestion is that because Fleischmann and Pons worked in isolation, even in secret, from their colleagues at the University of Utah, and then released

[40] These remarks are peppered throughout Huizenga's text. The examples I have used here are taken from (1992) pp. 236, 234, 222, 216, 39, 35 and vii, respectively.

their results to the press instead of submitting them to a scholarly journal, they evaded the points at which obvious flaws in their work would have been caught. Fleischmann and Pons are portrayed by turns as incompetent (using a "high-school type" apparatus and misinterpreting the Nernst equation, "taught in college freshman chemistry courses"), grasping ("the craving for fame, notoriety and patent rights took precedence over following the normal scientific procedures"), stupid ("Fleischmann and Pons rushed into print announcing something with far reaching implications in nuclear physics that they didn't even understand"), and the naive victims of a shady university administration ("all evidence indicates that University of Utah administrators fostered the isolation of Fleischmann and Pons' cold fusion research from nuclear scientists both on and off campus").[41]

At many points in his cold fusion chronicle, Huizenga is acutely aware of the influence of extra-scientific factors on the scientific process. He is especially condemnatory of the University of Utah for permitting concern about patent rights to distort the normal scientific process, and for using the cold fusion results as an excuse to seek in excess of $125 million in so-called "pork-barrel"

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[41] Again, these are representative selections from Huizenga's (1992) litany, taken from ibid. pp. 1, 33, 39, 220 and 224.

funding.[42] Nor does he overlook the most obvious factor contributing to the tumult surrounding cold fusion. The promise of a limitless supply of cheap, safe energy was and is at least as important to both the scientific community and the general public as the discovery of new and unexpected facts in nuclear physics. It is this potential, according to Huizenga, that caused some scientists to allow themselves to be deluded by suspicious data and outlandish theoretical claims, while others took the outrageousness of the assertions as an urgent call to attempt to replicate the cold fusion results with as much speed and vigilance as possible.[43] The general point is that *good* scientists remained detached from the sociopolitical pandemonium of cold fusion, and set out to verify or disprove the facts. It was only the *bad* scientists who allowed their judgement to be clouded by hopes of fame and fortune.

Huizenga's tactic is to assess the workings and worth

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[42] "Pork-barrelling" refers to the practice of seeking federal funds for academic and research facilities by lobbying instead of by peer review, thereby "substituting politics for expert judgement" (Daryl E. Chubin, "Scientific Malpractice and the Contemporary Politics of Knowledge", p. 154, in Cozzens and Gieryn (eds), *Theories of Science in Society*, 1990). Chubin claims that funds allotted in the pork-barrel manner have increased in the US from $3 million in 1980 to $145 million by 1987. The University of Utah hired a powerful lobbying firm to represent their interests to the US Congress.

[43] Huizenga claims that the more startling the result, the more quickly it will be reexamined by other scientists. If true, this "norm" would certainly explain why many scientific claims about women have been underinvestigated: they fit conventional, androcentric expectations.

of a research program by looking at the rules that govern such projects. Harding holds that this is the only tactic available to critics of bad science. But one does not assess scientific research in this way any more than one would examine the laws governing a society in order to learn about how that society worked and what it was like to live in it.[44] There are social norms, plus institutional and many other motives for actions, just as there are scientific norms, in addition to institutional and other motivations. But science in its legalistic characterization is not the same thing as science in practice. Science has a set of "labour practices" which include detecting errors, encouraging some theories and practitioners, discouraging others, etc.; but just as there is lousy labour practice that falls short of breaking the law, there is bad science that does not violate any particular norms.[45]

It may be objected that the case of cold fusion is a poor one for illustrating my central claim, that deception in science reveals a great deal about the operation of norms

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[44] My thanks to Mohan Matthen for suggesting this analogy; Imre Lakatos makes a similar point in "History of Science and its Rational Reconstruction" (1971) that *a priori* methodology is like statute law, and the methodology abstracted from scientific practice is like case law. He argues that just as jurisprudential compromises between statute and case law are sometimes required, so are scientific compromises between *a priori* and practical methodology. Harding's emphasis on the former obscures the existence of the latter.

[45] Thanks to Mohan Matthen for suggesting this line of argument to me.

and rules in science. Fleischmann and Pons were not frauds, after all. They were just misguided, overeager, and out of their depth. Whether or not they set out to intentionally deceive the public and the scientific community, however, there is no question that segments of both groups reacted as though they had been duped, and felt even more strongly that the most central dictates of scientific practice had been violated by the cold fusion researchers. The fact that the cold fusion fiasco appears first and foremost a case of self-deception does not rule it out as a resource.

Nonetheless, it is when we turn to external critics of science, that is, to those outside of the scientific community, that an examination of bad science is particularly useful in divulging the innermost operations of the scientific process. Moreover, it is significant that such studies often disclose an incontrovertible gap between the traditional empiricist rhetoric of scientists like John Huizenga, and the actual workings of science. The most compelling example of this gap lies in the demand for peer review of manuscripts prior to publication and of project proposals in order to receive public funds. The evasion of the peer review process in favour of a press conference is repeatedly held up by Huizenga as one of Fleischmann and Pons' most deplorable sins. Yet recent studies of the efficacy of peer review suggest that it is not the reliable gatekeeper that Huizenga touts.

Marcel LaFollette's work confirms that many of the assumptions upon which peer review relies are ill-founded.[46] She writes that "the emphasis on peer review reinforces a myth that says all scientific journals use rigorous expert review in selecting all content and that the peer review process operates according to certain universal, objective, and infallible procedures, standards and goals. Quite the opposite is true, however."[47] LaFollette points out what should perhaps be far more obvious, especially to the Huizengas of the world, that the procedures hailed as ensuring the authenticity, accountability and authority of science "are simply arbitrary creations and, like other human creations, they are fallible".[48] LaFollette's use of the word "arbitrary" here, if taken to mean random or capricious, is probably too strong. I suggest that we read her as holding that peer review procedures (and their applications) have arisen through a series of discretionary choices that could have been otherwise.[49] Harding, with her legalistic characterization of the norms of science and the value of examining departures from them, must take heed of this. The extra-scientific influences that are supposed

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[46] LaFollette, *Stealing Into Print: Fraud, Deception and Plagiarism in Scientific Publishing* (1992)

[47] ibid. p. 119

[48] ibid. pp. 119-120

[49] I owe this observation to Kathleen Okruhlik.

to be filtered out by peer review are right there in the procedure: availability of staff and financial resources, the values, experiences and opinions of editors and reviewers, competition between journals, and chance.[50] Anonymity of review, which is a recent innovation and not used uniformly,[51] can at best work to diminish the prejudices of editors and reviewers, and can have little effect on factors like resource availability and competition.[52] Furthermore, with respect to a sufficiently narrow subdiscipline or finely delimited issue, it is sometimes the case that there are very few, if any, peers who can be relied upon to give an informed assessment of an experiment.[53] It is obvious that Huizenga is aware of the

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[50] Many scientists were horrified at the results of a 1981 study commissioned by the US National Science Foundation that suggested that luck played a significant role in the peer-reviewed process of awarding research funds. See Allan Clark, "Luck, Merit and Peer Review" (1982)

[51] LaFollette (1992) p. 128

[52] Huizenga demonstrates his comprehension of at least one of these issues when he observes that "faster publication can sometimes be insured by telling the editor that a competing paper is due to be published in another journal". (*Cold Fusion*, 1992, p. 218) However, he remains true to his assumption that such manipulations are the mark of bad science.

[53] ibid. p. 122. As LaFollette emphasizes, this is especially true in new fields and with regard to "cutting edge" research. Some journal editors strive to avoid using an author's scientific competitors as reviewers, making the available pool even smaller. Contrast this with Huizenga's claim that "most scientific advances are the result of collaborative efforts of scientists working at the frontiers of their discipline over a period of time" (p. 52).

problems created when scientists stray from research within their area of specialization, because he continually berates chemists Fleischmann and Pons for probing into physicists' territory. Yet he apparently does not see the ramifications that the maintenance of rigid disciplinary boundaries holds for the possibility of objective peer review. It would be very simple-minded to suggest that physicists can review the work of other physicists and chemists of other chemists, without taking into account the often very fine divisions found in these broad concentrations. This is especially true given that "each subgroup of science sets its own standards for research procedures, policies, communication and evaluation".[54] Therefore, one of the lessons of the study of scientific malpractice is that the sciences are sometimes not methodologically unified.

Even when a peer group is readily identifiable, claims LaFollette, scientific self-monitoring breaks down. Rival laboratories are found to have deep-seated disagreements over the most elementary points of scientific procedure, and even where there is agreement, self-correction can fail. In the most blatant cases where experimental results and scientific artifacts are fabricated out of whole cloth, peer review has not been able to uncover such transgressions

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[54] LaFollette (1992) p. 16

because it was never designed to do so.[55] The power of peer review is restricted to more mundane tasks, such as checking figures, assessing overall plausibility, and watching for obvious logical or practical howlers.[56]

Forged data, such as that used by the infamous progenitor of modern intelligence-testing, Cyril Burt, is unlikely to be discovered through peer review. LaFollette points out that even the most sceptical reviewer may fail to question data because of a subconscious inclination toward belief in the truth of the conclusions based upon them. These conclusions "may fit common cultural biases and the 'facts' may be cleverly constructed to those expectations".[57] This was almost certainly the case with Burt's data, which reinforced Eurocentric expectations about the intelligence of non-whites. But even the most severe scrutiny of Burt's individual papers would have been unlikely to reveal his forgery. Often, it is only the accumulation of data, and of conclusions, publications,

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[55] W.J. Broad and Nicholas Wade, *Betrayers of the Truth: Fraud and Deceit in the Halls of Science*, (1982)

[56] Even then it does not work. Frederick Grinnell describes the case of some unusually paranoid researchers who, afraid their important results would be plagiarized by journal referees, deliberately reported a slightly incorrect gene sequence in a paper, making the correction only when the article had been accepted for publication and was at the page proof stage, so that the referees would not learn the correct sequence until the paper appeared in print. The referees did not catch the error. Grinnell (1992) p. 124.

[57] LaFollette (1992) p. 125

grants, promotions and prestige based upon them, that gives rise to suspicion. Such was the case more recently for John Darsee, a Harvard Medical School researcher whose propensity for fabricating data was discovered when his progress to the top of his field at a tender age was deemed a trifle too meteoric to be authentic. Note, however, that Burt's work, although widely criticized and discredited, still appears in some standard textbooks of psychology,[58] and some of Darsee's publications are still cited as valid.[59] This leads us to the thorny issue of the embeddedness of science in publishing and the law.

Huizenga's model of proper scientific conduct takes the correction of error to be central. "When errors are discovered", he writes, "they should be acknowledged immediately, preferably in the same journal".[60] LaFollette's work illustrates the naivete of this assertion. The proliferation of multiple-author manuscripts,[61] and the

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[58] See the discussion of Burt and his legacy in Stephen Jay Gould, *The Mismeasure of Man* (1981), especially chapter 6. Burt is also discussed by Peter Medawar (1959) and by Lewontin, Rose and Kamin (1984).

[59] Walter Stewart and Ned Feder, "The Integrity of the Scientific Literature" (1987) pp. 207-214

[60] *Cold Fusion* (1992) p. 222

[61] John Hardwig offers the example of a *Journal of Physics* article with *ninety-nine* different authors. No single author can be said to know or be responsible for the content of the article in the way that Huizenga demands. Indeed many of them would be unqualified to perform some of the experiments on which their interpretations and conclusions depend. Hardwig,

reliance of researchers on institutional support, means that in practice, the retraction of error is seldom the routine matter Huizenga makes of it. Co-authors may disagree on the interpretation of a result, so that one may see an error where another does not. Or one author may even have concocted evidence without the knowledge or approval of another.[62] This might not be so worrying if it were not for the fact that scientists and their sustaining institutions have become litigiously protective of their reputations of late. Many scientific journals are finding it impossible to print retractions of falsified data, or even notices of ongoing disagreements or investigations, because of the threat of lawsuits.[63] Although LaFollette does not say whether lawsuits have been threatened between co-authors over disagreements in interpretation of evidence, there seems nothing to stop matters from coming to such a turn. Journals themselves are at risk here as well, because each will want to maintain a reputation for quality and reliability. The printing of too many retractions, for whatever reason, may diminish their standing with their

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"Epistemic Dependence" (1988)

[62] This was one of John Darsee's more abominable crimes. The practice of including "honourary" co-authors, usually the head or senior member of a lab or project, has come under fire as a consequence of Darsee's fall from grace. Cf. Stewart and Feder, op cit, and Eugene Brunwald, "On Analysing Scientific Fraud" (1987) pp. 215-216. Brunwald was Darsee's supervisor at Harvard.

[63] LaFollette (1992) pp. 185-200

intended audience.[64]

To complicate matters further, retractions in science are not always made on the strength of "ordinary error". In 1990, Stanford University physicist Blas Cabrera retracted his 1982 finding of the first and only unambiguous observation of a magnetic monopole.[65] (Magnetic monopoles are allegedly called for in order to make Maxwell's unified theory of electricity and magnetism perfectly symmetrical, and are given precise mathematical description by his equations.) Proponents of grand unified theories, or GUTs, had long predicted the existence of monopoles, and were delighted by Cabrera's discovery. What they were not delighted by was his claim that, after eight years of trying unsuccessfully to replicate the result, he was forced to conclude that the monopole evidence was statistically improbable and should be discarded. However disappointing it may have been, Cabrera was operating under principles that Huizenga would admire: no independent confirmation, no data. Interestingly, however, there is a suggestion in the particle physics community that Cabrera's retraction ought to be rejecte , because it is based on mere statistical likelihood. "No one, not even Cabrera himself, has found any error in the original experiment; nor have any new theoretical arguments surfaced to cast doubt on it. The

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[64] ibid. p. 185

[65] Hans Christian von Baeyer, "Dead Ringer" (1990)

retraction was a judgement call, based on the overwhelming negative evidence of the past eight years."[66] Those physicists who disagree with Cabrera's judgement will not likely be persuaded by his retraction. What this example illustrates is that the criteria for judging science fluctuate, even among the most traditional empiricists. The fact that these criteria are disputed suggests that feminist philosophers of science may enter the dispute and contribute to it, without being limited by unreasonable assumptions about the nature of the criteria themselves.

Retraction may have limited effectiveness for other reasons, regardless of whether it is necessitated by the discovery of fraud, an honest mistake, or a statistical or other anomaly. No scientific publication can ever be truly retracted, because the practicalities of publication intervene. To what ends ought journals go to ensure that every reader is made aware of the withdrawal of erroneous or fraudulent data? Journals with already limited resources will be reluctant to bear the expense of investigating complaints, and of correcting the record. The standard response is to print a notice in the earliest available issue outlining the relevant mistakes or suspicions and advising readers to disregard previously published claims. Naturally, not every reader will see the retraction or correctly judge its importance to their own work, and many

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[66] ibid. p. 3

researchers may continue to rely on problematic data. It would be unrealistic to expect any more strenuous response ("everyone cut out pages 301 through 308 from the April 10, 1989 issue of *Journal of Electroanalytical Chemistry*"), for obvious reasons. So what is a conscientious researcher to do? Adopt a new rule that says, "whenever primary literature is used as the basis for further work, always check *n* number of issues after the initial appearance of data for notices of retraction or modification"? And even where retractions manage to reach a large proportion of those likely to be interested and influenced, the popular press tends not to be so vigilant in informing the public of caveats, disclaimers and recantations as they are in publicizing new scientific "discoveries". Therefore, feminist recognition of the need to view science as a socially embedded process must take account of as broad a range of social variables as possible. When the issues at stake are part of the very fabric of patriarchal science, these considerations are complicated by the fact that cultural expectations may make investigators less likely to see bad science as such.

As I argued in chapter two, the character of the individual scientist is important to the scientific enterprise because all of its (communal) mechanisms are founded on the presumption of honesty and trustworthiness in its (individual) practitioners. Internal and external critics of science accept this. Huizenga emphasizes the need for all parties to the scientific process (i.e., researchers, editors and referees) to regard their responsibilities as solemn ones.[67] LaFollette focuses on a simple practical issue: scientific publishing cannot function without the presumption of individual honesty.[68] Many more commentators add that science itself cannot function without such a presumption.[69] This point is reinforced in the reaction of scientists to their colleagues' deception. The prevailing mentality is neither to circle the wagons in defense of Science, nor to sacrifice transgressors as an example to the public and to other scientists. Both situations are found in practice.

What emerges in virtually all cases, however, is the fact that scientists are ill-equipped to deal with duplicity. They claim that they are "not trained to think in terms of dishonesty in science".[70] Junior researchers in particular often do not even know what the appropriate channels are for reporting suspected fraud.[71] Faculty members have been found to hold that in principle, they believe they ought to be responsible for the ethical

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[67] *Cold Fusion* (1992) p. 215

[68] LaFollette (1992) p. 88

[69] See especially the quotations in Broad and Wade (1982)

[70] LaFollette (1992) p. 103

[71] ibid. p. 143

"mentoring" of their students, but report striking differences between their espoused values and actual practice.[72] Faculty and students alike have been found to fear retaliation if they report suspected misconduct,[73] and with good reason. LaFollette documents numerous cases of careers lost and reputations ruined, even when whistle-blower's suspicions have proved correct, or when the impact on science of some particular deception was negligible.[74]

Interestingly, the study of fraud in science demonstrates that the standards of individual honesty and reliability that scientific practice demands are seen by both internal and external critics as ethical, not epistemological, requirements. This is a juncture at which the dialogue between critics of bad science and science-as-usual may take hold. The former argue that cheating is rare, and occurs only among the "bad apples". Bad science is explained by reference to the moral (and often psychological) pathology of bad scientists. The latter contend that dishonesty is widespread, and that all of the apples in the barrel of science are rotten to the core.[75] But neither claim is quite right, and neither is

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72 Swazey, Anderson and Lewis, "Ethical Problems in Academic Research" (1993) p. 549

73 ibid. p. 547

74 LaFollette (1992) pp. 148-151, 153, 176

75 This metaphor is used especially by Broad and Wade (1982)

particularly helpful in improving science. The double-aspect view of knowers, for which I argued in chapter two, comes into play here as a device for unifying what I have identified as disparate ethical and epistemological concerns.

Questions of individual character are a prominent part of the communally shared and negotiated background of science, and there are ethical standards with which every science, and each scientist, must comply. Moral virtues like honesty, trustworthiness, prudence, and caution become (or are already) epistemological because they have long been so central a part of science. If their impact has begun to diminish, that too needs to be placed on the table for negotiation in science. Many contemporary historians of science are sensitive to the role of individual virtue and character in the construction of modern western science, but feminist critics are less so.[76] Adherence to these standards has definite epistemological consequences. It is counterproductive to maintain an artificial distinction between ethical and epistemological norms, between good scientists and good science.

Part of what is up for grabs in the communal mediation

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[76] Evelyn Fox Keller is aware of the importance of character in early science, but she does not tie it to any explicitly feminist considerations. (Keller *SLSD*) Feminist retellings of the history of science have focused on the way women have been excluded by scientific rationality defined as masculine, but not on the way women have been excluded by scientific morality defined as masculine.

of scientific rules is a code of personal conduct for individual scientists. Although the standards of that code appear admirably high, the presence of plagiarism and other forms of fraud in science indicates that the rhetoric of science has once again outstripped its practice. This is a good example of submerging the individual in the group, against which I argued earlier. Just as we saw that we cannot safely assume that the intellectual authority of a particular individual is being respected because the community has a rule for that, we cannot assume that a particular individual meets the moral standards of the community. Nor can we assume that the community actively enforces those standards. Does this mean that feminists ought to scrutinize the honesty and trustworthiness of every scientist? Certainly someone ought to, although I am not sure who, how, or how often. There may be, in the end, more subtle and effective (and probably safer) ways to uncover the epistemological and moral commitments of scientists. One such way is to look at bad science, and the debate surrounding it, as part of the articulation of the governing norms of science. I will discuss other ways in chapter five.

CHAPTER FOUR

THEORETICAL STRATEGIES AND FEMINIST CRITIQUES OF SCIENCE

One of the strongest claims made by Sandra Harding on behalf of feminist critics of science-as-usual is that "one needs an adequate theory about science in order to begin to eliminate the ways in which science and its technologies victimize women".[1] Harding is one of a number of philosophers who think it possible, or even required, to combine strategies for ending the oppression of women with methods for finding out about the world.[2]

Although feminist theorizing about science is important, it is not obvious that a theory of science alone has or ought to have the power that Harding wishes to ascribe to it, the power to begin to reduce or eradicate harm to women. In this chapter I shall explore Harding's demand for theory. I argue that this demand requires some fleshing out on both practical and logical grounds. I will

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[1] Harding, *WS?WK?*, p. 73. I do not address the difficulties arising from Harding's hasty conflation of "science *and* its technologies" in this essay. The reader should be aware that questions about the relationship between technology and science are explored in a more sophisticated manner by other writers, including other feminists. See Don Ihde's *Instrumental Realism* (1991) and Joan Rothschild (ed), *Machina Ex Dea: Feminist Perspectives on Technology* (1983)

[2] Alison Jaggar is another; many of the views that Harding expresses, and in particular her objections to feminist empiricism, are also found in Jaggar's *Feminist Politics and Human Nature* (1983)

affirm a feminist empiricist philosophy of science in this chapter, and in the next I will propose that the feminist critique of science will also advance by using the disruptive voice of feminism more self-consciously and more subversively. We do need a theory of science in order to understand and explain the nature, impact, successes and failures of science. The best such theory will be akin to the best theories in science; it will have the widest possible range, the greatest explanatory power, and the most flexibility. Feminist empiricism will be shown to be such a theory.

Feminist empiricists take science to be a socially embedded process, governed by communally enforced norms. The most important of these norms is that knowledge claims must be adequately supported by evidence, and that the source of such evidence is experience. This opens the door to an explicitly feminist perspective on the experience of women, which must be considered a resource for science. This represents a departure from traditional empiricism, which regards experience as "unvarnished", and hence not subject to the influence of political or any other perspectives. Feminist empiricism retains some of the conservatism of traditional empiricism with respect to well-established scientific method and well-confirmed results. But so long as the norms of science are generated within male-dominated science communities, and without sufficient attention to the emergent practical and theoretical inadequacies of some norms, this conservative approach is too restrictive, and feminist empiricists challenge it.

The veracity of evidence is crucial to the feminist empiricist in two ways. First, scientific claims about sexual differences, human origins, and the nature of women have been shown to rest on suspicious evidence. Feminist empiricist critics demand that the contamination of evidence by gender and other biases be recognized and condemned, and thus insist that the rigorous standards of mainstream science be consistently applied. Secondly, most contemporary empiricists argue that what counts as evidence is itself a subject for ongoing debate governed by empiricist standards. No *a priori* pronouncements about evidence and its interpretation will be tolerated, and feminist empiricists embrace this ideal. The net result of a feminist empiricist approach to science goes beyond simple inclusion of women's experience to an expanded role for women in the construction, critique, and revision of scientific standards and claims.[3]

I. Initial Problems with Theory

How ought Harding's statement, quoted above, be read? It may be tempting to interpret her claim as a logical one, asserting that a theory about science is a necessary

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[3] Bruce Hunter and Mohan Matthen were especially helpful in developing the preceding discussion.

condition for the elimination of women's victimization by science. It is difficult to see what conception of the relationship between theory and oppression could possibly ground so strong a claim, and so I can see no reason to attribute such a view to Harding. To do so would suggest that she requires a second, deeper theory to make sense of her requirement for a theory of science. Is she then making the simple practical claim that in order to get scientists and philosophers of science to attend to and engage with feminist concerns about science, feminists must present their own theory about science? Harding is certainly aware of the instrumental importance to feminists of being able to engage one's intended audience, as we saw in chapter three.[4] Yet Harding's demand for theory goes beyond the merely practical.

This is fortunate in many ways, because as the fulfillment of some pragmatic condition for the liberation of women, providing a feminist theory (or theories) about science poses some difficulties. The effort to involve one's target audience is threatened by a double-edged sword.

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[4] Harding is surely correct about the need to engage, but this need goes beyond the mere ability to capture and hold the attention of scientists and philosophers of science. Lynn Hankinson Nelson and Richmond Campbell observe that feminist empiricism has the advantage of building from common ground with those already engaged in the practice of science and theorizing about science, placing feminist empiricism rather more firmly in the cooperative enterprise of communal negotiation. See Campbell, "The Virtues of Feminist Empiricism", p. 108 and Nelson, *WK* Introduction.

Feminist theories sometimes capture the attention and even respect of philosophers and scientists, but only to the extent that they focus on theory. But to the extent that they are theoretical, they seem to exclude praxis. (Most feminists are familiar enough by now with the view that "ivory tower" feminism, that is, the creation and refinement of feminist theories by often privileged academic women, offers little aid and comfort to the majority of women, and may do little to advance the women's movement. Most academic feminists respond, with justification, that making theory *is* (or can be) a political act.) This problem is always with us: focus on theory and alienate activists, but speak of politics and lose the respect of students of science.

Academic science and especially philosophy are already secluded within the institutional environment, so that criticizing science on theoretical grounds has an appropriate "feel" to it. One problem is that science is also segregated, even within the academy, from the humanities, toward which scientists frequently maintain a stunning indifference, or even open hostility.[5] If established philosophies of science do not attract the concern of scientists, then there is little reason to hope that any feminist theory will be successful (although

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[5] The classic statement of the animosity of the sciences toward the arts and humanities can be found in C.P. Snow's *The Two Cultures* (1959)

feminist theory may succeed in a negative sense, in attracting antagonistic criticism). Even if a feminist theory can overcome indifference and/or enmity, the level at which it will occupy many scientists will be the theoretical one. Cooperative discussion and refinement of feminist philosophy of science may obstruct the achievement of the stated goal, the elimination of the victimization of women by science.

We should recognize that no theoretical approach from within the humanities has yet yielded a comprehensive, workable theory about science, so why be sanguine that feminist theories will fare any better?[6] We might also ask what reason we have to think that feminist theories of science are immune to some of the criticisms of theory that feminists themselves have raised. Ruth Hubbard, for example, states quite baldly that "every theory is a self-fulfilling prophecy that orders experience into the framework it provides."[7] This claim is either not intended to apply to feminist theories, in which case an explanation for their exclusion is required, or it does apply but Hubbard thinks that there are reasons why it is not

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[6] My thanks to Mohan Matthen for reminding me of this point.

[7] Hubbard, "Have Only Men Evolved?" in Harding and Hintikka (eds) *Discovering Reality*, 1983 p. 46

particularly troubling to feminist theories.[8]

Philosophers of science of every conventional contemporary stripe benefit from the integration of their work with history of science and with the rest of philosophy.[9] Feminist philosophers can profit from these new syntheses as well as from their own political insights. But whether these advantages will be enough to establish a robust theory of science cannot be known in advance. Neither, efforts to develop feminist theory, about science as well as other aspects of society, have sometimes caused divisions between women and feminists. As Susan Sherwin has pointed out, these differences are often exaggerated by feminists and their critics, but are nonetheless raised to the level of substantive political conflicts.[10] Some feminist theorists have responded to these potentially damaging conflicts by insisting on greater freedom for

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[8] Satisfactory explanations are notoriously difficult to generate here. We cannot, for example, argue that feminist theories are excluded from this general condemnation because their biases are "progressive" or "self-conscious"; the theories of Bacon and Descartes were originally intended to be inclusive. Nor can we claim that the prophecies of feminist theories are "safer", given the by now widespread criticism that many such theories ignore racial, class and other important distinctions.

[9] See Richard Boyd's introductory essay in *The Philosophy of Science* (1991)

[10] Sherwin, *No Longer Patient* (1992) p. 27. Sherwin also points out (p. 13) that the term "feminism" refers to both the theories that help to reveal the oppression of women, and to the political movements that seek to eliminate such oppression. This ambiguity obviously bears on Harding's demand for feminist theory about science.

feminists to disagree, and to be able to express their ambivalence toward the very structures of reason in which this disagreement is embedded. This move may preserve the solidarity of the women's movement, but from the perspective of critics of feminism, permission to disagree may appear as a feminist embrace of contradiction and incoherence rather than an admission of the exploratory nature of our project. At bottom, Harding's prescription for theory shares some features of the very positivistic conception of science of which she is so critical: belief in the efficacy of certain patterns of reason, and the instrumental value of a well-grounded theory of science in changing (or even *controlling*) the behaviour of scientists and public attitudes toward science. As I shall argue in the next section, sharing some common ground with logical positivism need not be cause for horror.

The emphasis on theory also carries certain risks not directly linked to the specifically feminist goal of terminating harm. In spite of her strong criticisms of Bacon, Harding recognizes that his project was motivated by an inclusive impulse, by an urge to draw more people, including plodders and drudges, into the burgeoning world of science.[11] The same can be said of Descartes, whose rationalism was intended not to exclude but to broaden the

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[11] Harding, *WS?WK?* p. 64, p. 72

opportunities of reason for all.[12] Many have argued that Bacon and Descartes' original gestures toward inclusiveness were doomed to failure because their views were based on too abstract and detached an ideal of reason, arising from their experiences as men. But if we find feminist arguments about the social embeddedness of knowledge compelling, then we must recognize that our current perception of the inadequacies of Baconianism and Cartesianism may be conditioned in part by the tremendous differences between the social contexts in which Bacon and Descartes dwelled and the social contexts from which we assess them. Our theory of science, and our social and political reactions to it are as temporally context-bound as our practice of science.

One stumbling block for any theory, no matter how well intentioned, will be its inability to predict and adapt in advance to those social, linguistic and cultural mutations that may render even a very sensitive theory oppressive or otherwise inappropriate. Richard Bernstein reminds us of Isaiah Berlin's observation that history reveals "a changing pattern of great liberating ideas which inevitably turn into suffocating straightjackets".[13] Harding's qualified defense of feminist standpoint theory,

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[12] See Genevieve Lloyd, *The Man of Reason* (1984) pp. 44-50, on Descartes' egalitarian aims and how and why they failed. Some critics are a little hasty in overlooking the Cartesian goal of making reason accessible to everyone.

[13] Bernstein, "The Rage Against Reason" (1988) p. 205

and her determination to explore and include the contributions of "other Others" in her theory of science is laudable and will enhance the flexibility of feminist philosophy of science, but no theorist is prescient. As I argued in chapter one, the meanings of many of the most central concepts of current scientific rhetoric have been repeatedly transformed over time and between different groups. A theory based on or responsive to the experiences of those who are marginalized today may be inadequate in the future, and may be ineffective in responding to the wily and subtle ways in which the abstract "letter of the law" can be circumvented.[14] But these are not reasons to abandon the quest for a theory about science altogether. They are reasons for being cautious regarding the scope and power of such theories. Harding allows that multiple theoretical strategies are preferable to any attempt to tell the one, eternally true and perfect story about the way the world is. (It is in this spirit that I will go on to defend a

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[14] Feminist critiques of law and legal theory offer a revealing parallel here. The adoption of gender-neutral jurisprudence has resulted in some ludicrous case law. In 1974 the US Supreme Court ruled that the exclusion of pregnant employees from disability insurance did not violate the equal protection clause of the Fourteenth Amendment because pregnancy, not gender, was at issue. Although recognizing that only women become pregnant, the court concluded that pregnancy is not a gender-based category because non-pregnant persons may be either men or women. Cf. Zillah Eisenstein, *The Female Body and the Law* (1988) p. 66. In Canada, challenges to the *Charter's* equal rights guarantee by men have outnumbered those by women by 35 to 9 in the first three years of litigation. Cf. Brodsky and Day, *Canadian Charter Equality Rights for Women: One Step Forward or Two Steps Back?* (1989)

narrative approach to science, as an adjunct to and even a parasite on existing theories of science.)

II. The Tainted Roots of Empiricism

The requirements for a feminist philosophy of science can now be discerned in outline. Such a theory ought to take human experience, broadly construed, into account, that is, it ought to be empirical. In addition, it ought to be responsive to and flexible enough to outlast the social context in which it is constructed, it should be humanistic at least in the sense that it recognizes the fundamentally human nature of science and its intimate connection to all other human activity. Finally, it should point to concrete ways in which science must change in order to become non-oppressive and harmless. Presumably, it should also address the issues and questions that have been established by mainstream philosophy of science, even if only to challenge their significance.

Can any theory fulfill all of these criteria? Some of the conditions are in tension: how is it possible to transcend our own social context so as to take everyone's experiences into account? How humanistic can our efforts be if, as Harding says, "I always see the world through my culture's eyes; I think within its assumptions"?[15] It is all very well to insist that our theory of science should accommodate different social (and historical) contexts as

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[15] Harding *WS?WK?* p. 59

well as different human experiences, but it is hard to see how this praiseworthy goal can be met in a workable, informative theory without transcending difference, at least to some extent. The attempt to transcend historical and methodological differences among the sciences seems inevitably to risk the cost at which positivism purchased humanism. Post-positivist philosophy of science recognizes the need for "local" assessments of histories, disciplinary practices and assumptions about method, evidence and confirmation, rather than a "global", abstract analysis meant to be applicable to the whole of science.[16] Similarly, the alternative for feminist science theory may be to develop multiple smaller, context-driven accounts of the ways in which science has affected and may continue to influence people's lives.

These difficulties notwithstanding, I think it must be emphasized that a distinctly feminist empiricism is a better philosophy of science than Harding or Jaggar, with whom Harding is largely in agreement, allow. As I mentioned at the beginning of chapter three, Harding's initial rather low regard for feminist empiricism has been tempered in her more recent work, and to be fair, she has never been *opposed* to feminist empiricism *tout court.* Even in *The Science*

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[16] Richard Miller (*Fact and Method: Explanation, Confirmation and Reality in the Natural and Social Sciences*, 1987) is the most thorough and eloquent spokesperson for this position.

Question in Feminism, in which she established and made definitive the distinction between feminist empiricist, feminist standpoint, and feminist postmodern epistemologies, she argued that the apparently unresolvable tensions between these divergent epistemologies could be a valuable resource for feminism. Heeding the details of these tensions also helps feminist theorists to avoid the pitfalls of universalism and absolutism (i.e., "humanism") that have been identified as key errors in positivist philosophies of science.[17] But I would argue that Harding sees the merits of feminist empiricism as being restricted to its role as a lure for non-feminist scientists, and for feminists, as a foil to the other two varieties of epistemology. This is why three recent defenders of feminist empiricism rightly identify themselves as responding specifically to Harding's objections.[18] Harding's position strongly implies that whatever epistemological strategy she believes she ought to defend, she is actually committed to a kind of pragmatism in which theories or positions will be chosen according to the needs and goals of the theoretician and/or her audience. It is because of this underlying pseudo-pragmatism that Harding's feminist epistemology has recently been described

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[17] Harding, *SQF*, chapter 10

[18] The three are Helen Longino (*SSK*, 1990), Lynn Hankinson Nelson (*WK*, 1990) and Richmond Campbell, "The Virtues of Feminist Empiricism" (1994). Nelson has the strongest focus on Harding's antipathy for feminist empiricism.

as *Feyerabendian*, a label that would probably horrify Harding.[19]

Harding's objections to feminist empiricism revolve around its alleged commitment to a merely corrective programme for feminist science. Curiously, Harding does not recognize the feminist positions of Longino and Nelson as being empiricist in the sense that she criticizes. She distinguishes between Longino and Nelson's "philosophical" feminist empiricisms, which are non-positivist, revisionist empiricisms, and "spontaneous" feminist empiricism.[20] Spontaneous feminist empiricists "think that insufficient care and rigor in following existing methods and norms is the cause of sexist and androcentric results of research" and refuse "fully to address the limitations of the dominant conceptions of method and explanation and the ways the conceptions constrain and distort results of research".[21] By isolating Nelson and Longino from their prefeminist empiricist antecedents, Harding creates problems. If their

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[19] Fetzer and Almeder, *Glossary of Epistemology/Philosophy of Science* (1993) p. 105; Harding cites, with disapproval, Feyerabend's approbation of those rational reconstructions of science which "change science from a stern and demanding mistress into an attractive and yielding courtesan who tries to anticipate every wish of her lover. Of course, it is up to us to choose either a dragon or a pussy cat for our company. I do not think I need to explain my own preferences". (Harding, *SQF* p. 120, and *WS?WK?* p. 43, quoting Feyerabend's "Consolations for the Specialist".)

[20] Harding, "Rethinking Standpoint Epistemology" in Alcoff and Potter (eds), *Feminist Epistemologies* (1993), pp. 51-56

[21] ibid. pp. 52-53

views are not "really" empiricist in the "traditional" sense, then neither are the views of many self-proclaimed empiricists, including Quine, van Fraassen and Giere. Harding introduces a double standard for assessing historical continuity between empiricisms. Spontaneous feminist empiricism is incapable of generating systematic self-criticism of science and re-evaluation of its own methodological norms, she says, so it is judged to be continuous with the empiricist tradition as associated with Locke, Berkeley and Hume, and their later positivist brothers. Philosophical feminist empiricism, in contrast, explicitly intends to examine and alter the norms of inquiry, so Harding deems it discontinuous with historical empiricism rather than [illegible] that her assessment of the weaknesses of empir[illegible]e been hasty or incomplete.[22] Ca[illegible]e heart of this double standard by in[illegible]nist empiricism be read not as a revisionis[illegible]eply committed to both the importance o[illegible] contextual values and to the norms of current scientific practice.[23]

Harding rejects feminist empiricism because she believes its roots lie in positivism. As an explicitly apolitical doctrine, she argues, positivism is unable to

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[22] Even Locke's empiricism was to some extent politically motivated, providing the groundwork for progressive criticism of the state.

[23] Campbell (1994) p. 110n

recognize social biases in science, because it does not have the resources to cleanse itself of such biases. Although positivists recognize that sociopolitical matters are significant to science at some level, that level is the context of discovery, and only the context of justification is of interest to positivists. Harding has it backward when she says that feminist empiricism is rooted in positivism. After all, empiricism as such has rather a longer and more varied history than does positivism. However, it seems that Harding does not mean *logical* positivism when she objects that feminist empiricism is rooted in positivism. She claims that "only in the late seventeenth century was it first said that the positive benefit of science could be restricted to its method, thus making it unnecessary for scientists and the institution of science to be *overtly* concerned with the social, political, and economic origins, consequences, or constituting values of science. (The term "positivism" is an even later invention; *it named an idea that was already well understood*.)"[24] But this way of construing positivism, as the view that what is distinctive about science is its method, is so broad that it is in no way committed to a distinction between the contexts of discovery and justification, nor to any other core norms.[25]

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[24] Harding, *WS?WK?*, p. 57, emphasis added

[25] Jaggar makes similar moves in attributing a "positivist conception of objectivity" to empiricists generally; Jaggar (1983) p. 356. Nelson and Campbell argue as I do, that the

Would Harding respond that the distinction between the contexts of discovery and justification merely names an idea already well understood?[26] I suggest that she could do so only at the cost of ignoring or minimizing substantial historical, geographical and disciplinary differences in the central methodological assumptions of scientific practice, of the sort discussed at length in chapter one.[27] If Harding's objections to positivism are to have any sense, they should be read as objections to logical positivism.

Since Harding bids us to look at the origins of feminist empiricism as a reason for strictly curtailing our reliance on it, we should examine the issue of theoretical roots a little more closely. Catharine MacKinnon makes a point similar to Harding's (although stronger and rather less sophisticated) in her work in feminist legal theory. MacKinnon is extremely critical of (among others) so-called

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positions Harding criticizes are objectionable in much the way she claims they are, but they are not necessary to empiricism. Cf. Nelson *WK* p. 264

[26] The distinction between the contexts of discovery and justification is a very late idea, alluded to by Carnap in the preface to *The Logical Construction of the World* (1928) and made explicit by Reichenbach in *Experience and Prediction* (1938).

[27] To focus specifically on Reichenbach's distinction, Harding would, for example, have to acknowledge that even Reichenbach (surely a die-hard positivist) did not think it possible to draw a sharp line between discovery and justification, nor did he think the context of justification could be ruled by logic alone, excluding all subjectivity. He insisted that finding all of the points at which "volitional decisions" determine the content of science is one of the most important tasks of epistemology. (Reichenbach 1938, pp. 4-16)

"cultural" feminists who celebrate a uniquely feminine character, and who propose an alternative morality based on it. MacKinnon sees cultural feminism as founded on an unwitting embrace of the very things for which patriarchy has long valued women, i.e., their caring labour and emotional connectedness. MacKinnon rejects this vision as utterly tainted by its roots in patriarchy, and complains that the very consciousnesses of women have been completely colonized.[28] If this stark conclusion were true, however, then we might feel justified in dismissing MacKinnon's own views on pornography and the social construction of female sexuality. We must at the very least expect some explanation for the fact that some women have escaped or thrown off their psychological subjugation. If Harding's objection to the source of feminist empiricism is of this sort, then we probably also have reason to reject feminist standpoint and feminist postmodern epistemologies as well. Hegel and Derrida are not exactly renowned for their progressive views about women. Feminism itself is rooted in liberal political theory, which most contemporary feminists reject. As my summaries of Harding's position in this and the previous chapter indicate, she is not so artless as MacKinnon. Still, I feel that we should take to heart Annette Baier's reminder that feminist emphasis on the

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[28] MacKinnon, *Feminism Unmodified: Discourses on Life and Law* (1987)

cooperative nature of belief acquisition "and our shared responsibility for successes and for failures, should incline us toward a willingness to get helpful support from any well-meaning fellow worker, alive or dead, woman or man".[29] The intentions of any "fellow worker", may never completely outweigh the impact of her or his views, but they matter. And the intentions of at least some empiricists\ logical positivists were *not* apolitical.

A detailed examination of the political and ideological commitments of the Vienna Circle lies outside the scope of this essay. However, there are three aspects of this issue that warrant highlighting here. First, in suggesting that there are features of logical positivism which, with some revision, could be turned to feminist purposes in the philosophy of science, I do not wish to give the impression that I believe that the logical positivists were just misunderstood and that their programme ought to be rescued and revived. Logical positivism really is, on the whole, "dead, or as dead as a philosophical movement ever becomes".[30] Fortunately, we do not need a signed organ donor card in order to pirate some of its constituent remains.

Second, it is an oversimplification to claim that *all*

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[29] Baier, "Hume: The Reflective Women's Epistemologist?" (1993) p. 35

[30] John Passmore, *Encyclopedia of Philosophy*, vol. 5, p. 56, "Logical Positivism"

positivists, especially all Vienna Circle positivists and their comrades, were (or must by definition be) indifferent to the connection between science and social and political questions.[31] Jerry Ravetz has argued that "many of the main protagonists in the development of twentieth-century philosophy of science have been *deeply* committed to causes *directly* involving humanity; and their doctrines of the philosophy of science were shaped with those broader ends *consciously* in view".[32] Ravetz naturally refers to Karl Popper here, since Popper's political commitments were made well known through such works as *The Open Society and its Enemies*. But Ravetz intends his remarks to apply quite broadly to virtually the whole of twentieth-century philosophy of science, including the logical positivists, Kuhn, Lakatos, and Feyerabend.

In some logical empiricist publications, including the 1929 Vienna Circle "manifesto",[33] Carnap's

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[31] Many philosophers of science divide the early empiricist tradition into its Viennese branch (the logical positivists), and the logical empiricists of Berlin, and emphasize that in addition to shared key assumptions, there were also many differences in the points of view of members of the two schools. Such fine distinctions tend to be blurred in the feminist critique of positivism. See Salmon et al, *Introduction to the Philosophy of Science* pp. 2-3, and Kitcher (1993) pp. 4-5 n4.

[32] Ravetz, "Ideological Commitments in the Philosophy of Science" (1992) pp. 5-12, emphasis added

[33] The Vienna Circle, *The Scientific Conception of the World*, reprinted in Neurath and Cohen (eds), *Otto Neurath: Empiricism and Sociology* (1973)

autobiographical essay,[34] and many of Neurath's essays,[35] a connection between political ideals and science is made quite explicit. And, just as had been the case centuries earlier for Bacon and Descartes, one of the guiding impulses of at least some logical positivists was the demystification of science in order to make the foundations of knowledge accessible to everyone.[36] However, the question of whether the Circle should engage in political polemic was a deeply divisive one. Carnap, Neurath and Hahn may have advocated the importance of political engagement, but they were adamantly opposed by Schlick, Kraft and Reichenbach.[37] I think that the most charitable interpretation that can be offered here is that there was simply no consensus about the relevance of politics to science among the members of the Vienna Circle. When we compare their divergent outlooks to the positivism of, say, Comte, with his revised calendar and positivist religious festivals, we can see that there are many strains within positivism, some of which are in

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[34] In Schilpp (ed) *The Philosophy of Rudolf Carnap* (1963)

[35] Especially "Anti-Spengler", plus some of the other essays collected in Neurath, op cit; Neurath was openly and thoroughly critical of the political neutrality of the conservative members of the Circle.

[36] My thanks to Bernie Linsky for directing me to Douglas Carr's 1979 MA thesis, "Tolerant Empiricism" (University of Alberta), and the references on this issue therein, especially Neurath, Carnap and Morris (eds), *Foundations of the Unity of Science* (1970)

[37] See, e.g., Proctor, *Value-Free Science?* (1991)

conflict.[38] This suggests both that the apoliticalness of positivism is not clear-cut, and that feminist philosophy of science may yet glean some valuable lessons from the ideological positions of the likes of Neurath and Carnap.

It is admittedly hard to believe that a group of politically engaged intellectuals writing in Germany and Austria in the 1930s could be so silent about the rise of fascism, and it is not very satisfying to attribute this silence to political naivete. The Vienna Circle was the target of Nazi recriminations, having its publications banned in Germany. This leads me to my third point. The decision to be or become neutral with respect to politics is itself a political move with many possible meanings. It may amount to a tacit endorsement of the status quo, or it may indicate indifference to social matters, or it may conceal an uneasy truce between science and state authority, or it may be founded upon the belief that values are of no relevance to science. All of these motives seem to be present to varying degrees among empiricists and logical positivists. I suggest that the social and historical context in which an apolitical stance is adopted informs what it means to be apolitical, and therefore cannot be overlooked. Feminists ought to question claims of a general

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[38] Comte's positivism was rejected as too materialistic and speculative by the "purer" Baconians, including Cuvier and his disciples. See Appel, *The Cuvier-Geoffroy Debate* (1987) p. 48 and p. 254 fn. 35

nature made in this context.

III. Empiricism, Norms, and Values

Feminist empiricism, even with its historical connection to positivism, is not restricted to criticizing bad science, nor is it politically inert. In its most recent "philosophical" versions, its champions offer detailed treatises of feminist empiricism, giving it some general advantages. For one, feminist empiricism maintains, sensibly, that there is something of value in the existing norms of scientific inquiry, especially when these are taken to be the rules that "scientists implicitly endorse in constructing, defending and criticizing various experimental tests of theoretical hypotheses",[39] and not the rhetoric with which they sometimes obscure their activity. Thus it coheres with the commonsense view that empirical inquiry provides the most credible source of knowledge of how things are.[40] It is also sensitive to Lynn Hankinson Nelson's observation that "scientific and political revolutions occur only if those with revolutionary visions are able to convey their insights to others in ways that make it possible for others to come, eventually, to share them".[41] This observation goes far beyond the merely instrumental appeal

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[39] Campbell, "The Virtues of Feminist Empiricism" (1994) p. 111 fn7

[40] John Dupre, *The Disorder of Things* (1993) p. 1

[41] Nelson, *WK* p. 19

of feminist empiricism that I attributed to Sandra Harding. It is a clarion call for change.

Some proponents of feminist empiricism have claimed that their position is the only real feminist alternative currently available to philosophers of science. This may overstate the case a little, but only a little. Feminist empiricisms, even if imperfect, are certainly among the most exhaustively detailed and elegantly argued of contemporary progressive theories. They exemplify growth in feminist theory by consciously stepping out of the critical and into the constructive mode, a task that feminist standpoint and feminist postmodern epistemologies seem far less able to achieve. And as Nancy Tuana has recently argued, feminist empiricism may provide a blueprint for resolving tensions between diverse feminist approaches to science and epistemology.[42] But perhaps the greatest advantage on which its adherents insist is that feminist empiricism be taken as a theory of evidence, not as a theory of justification.[43] This is important because, as will be shown in the next chapter, evidence is something that women

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[42] Tuana, "The Radical Future of Feminist Empiricism" (1992)

[43] Nelson, following Quine, and Campbell, hold empiricism as a theory of evidence; Longino's "contextual empiricism" is more concerned with why certain statements or states of affairs get taken *as* evidence.

can do something about.[44]

Helen Longino states what perhaps has long been obvious, that the possibility of doing value-free science is just nonsensical.[45] Science proceeds on the back of certain assumptions about the value of doing science, and about the best ways of doing it. This is an important crux from the feminist perspective. Consider Harding's claim that feminist empiricism "appears to challenge mainly the incomplete practice of scientific method, not the norms of science themselves".[46] It is clear that the plausibility of this claim depends on what is meant by "norms", which Harding unfortunately does not specify. In places, it appears that she intends "norms" to refer to the "discourses of objectivity and truth/falsity"[47], but as we saw in chapter one, these discourses have changed over time and in response to variations in widespread beliefs, including beliefs about how science should be practiced. It is clear that truth, at least in the sense of diminishing falsehood,

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[44] If scientists take themselves to be working, and correcting their work, according to empiricist ideals, and it is clear that many of them do, then that is a piece of evidence for feminist philosophy of science. Problems with this evidence are analogous to those raised by radical feminists with respect to false sexual consciousness and the naive voluntarism that characterizes radical feminist demands for realigning women's sexual behaviour. See Bartky, "Feminine Masochism and the Politics of Personal Transformation" (1990)

[45] Longino, *SSK* p. 4, 13

[46] Harding, *WS?WK?*, p. 113

[47] ibid.

is of value to Harding, as it should be. Therefore it cannot be the norm of truthfulness that she intends to call into question.

It could be that Harding is attacking what most theorists refer to as the epistemic virtues or values of science. Most contemporary writers identify values or virtues that play a necessary role in the development of scientific knowledge. These virtues include truth, explanatory power, accuracy, internal coherence, external consistency, scope, simplicity, unifying power, predictive power, generality, instrumental efficacy, and fecundity.[48] Harding is surely correct to imply that a progressive philosophy of science must go further and acknowledge the role of *non*-epistemic values, i.e., social and cultural ones, in science. However, a quick review of the current literature reveals that there is no agreement as to where and how to draw the line between epistemic and non-epistemic values for science, nor even whether it is helpful to draw such a line at all.[49] Giere, for example, insists that the

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[48] This list represents a digest of the *constitutive* values of science (Longino's term) found in Kuhn, *The Essential Tension*, Longino, *Science as Social Knowledge*, Laudan, *Science and Values*, van Fraassen, *The Scientific Image*, Giere, *Explaining Science*, McMullin, "Values in Science", Campbell, "The Virtues of Feminist Empiricism", and Putnam, *Realism with a Human Face*.

[49] Phyllis Rooney, in "On Values in Science: Is the Epistemic/Non-Epistemic Distinction Useful?" (1992) argues that feminist commitment to such a distinction may unduly limit feminist work. My thanks to Alex Rueger for bringing Rooney to my attention.

only epistemic value on which philosophers can agree is truth, while his view of non-epistemic values tends more toward personal characteristics like open-mindedness than toward culture-wide attitudes.[50] Longino, Laudan, Kuhn and McMullin think simplicity, coherence, and scope are epistemic values, but van Fraassen labels these same ideals "specifically human concerns, a function of our own interests and pleasures", and argues that they "cannot *rationally* guide our epistemic attitudes and decisions".[51]

van Fraassen also parts company with those who think that explanatory power is or should be a significant epistemic virtue for science. He thinks that explanations that meet the primary epistemic criteria of empirical adequacy, consistency and strength will already be powerful, whereas a merely powerful explanation will not be sufficient to meet the primary criteria.[52] Campbell argues in a related vein that the norm of explanatory power is really in conflict with the positivist claim that the norms operate only within the context of justification, because what makes an explanation powerful is in part its ruling out relevant alternative hypotheses. "The comparative nature of this norm is such that it cannot be applied without noting

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[50] Giere (1988) pp. 161-162

[51] van Fraassen (1980) p. 87. van Fraassen would not even assent to the idea that truth is a central virtue, holding as he does that theories need not be true to be good.

[52] ibid. p. 94

whether there are other relevant theories, and these are part of the context of discovery".[53] The very lack of consensus over issues so central as these suggests an ambivalence among philosophers of science upon which feminists may be able to capitalize in their efforts to get other values on the table for consideration, or to rethink the ways in which existing virtues are realized in practice. It is plain that there is no pre-established set of norms to which science must conform.

When we attend to some specific discussions of constitutive values in science, we also discover that the relationship between epistemic values and scientific practice is quite open-ended. This is the level at which Harding's observation about norms is least likely to apply. As Larry Laudan has pointed out, the ideals embodied in any list of epistemic virtues can be realized according to any number of rules for the practice of science.[54] This suggests very strongly that there is room, even within empiricism, to effect substantial normative and methodological changes in science, including the introduction of rules for scrutinizing science. It is this notion, that the norms of science are themselves part of the empirically tested ground of science, that is, in my view, central to a workable feminist empiricism.

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[53] Campbell (1994) p. 97

[54] Laudan (1984) pp. 36-37

I have now written pages about the strengths and weaknesses, real and imagined, of feminist empiricism, without saying anything directly about what I take feminist empiricism to be. I have hinted that the works of Longino, Nelson and Campbell are especially worthy of philosophical attention. All three insist that it is essential to move description and especially criticism of science within the boundaries of scientific practice.[55] Nelson's particular commitment to Quinean holism, wherein the relationship between science and epistemology is one of "reciprocal containment"[56] indicates that this move is not beyond the pale for contemporary empiricists, as Harding would have it. There is no "violation" of the empiricist boundary between the contexts of justification and discovery in such a move because, as Campbell points out, empiricism is not necessarily committed to such a boundary, nor can it be if it wishes to be consistent with certain of its apparent norms. Often, perhaps characteristically, beliefs that are discovered and in need of justification at one time later become part of the background against which further discoveries are made.[57] This applies equally to empiricist

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[55] This position was first articulated most clearly by Kathryn Pyne Addelson in "The Man of Professional Wisdom" (1983) p. 165-186; Nelson makes good use of Addelson's insights in chapter 5 of *WK*.

[56] Quine, "Epistemology Naturalized", in *Ontological Relativity and Other Essays* (1969) p. 83

[57] Campbell (1994)

beliefs about the distinction between discovery and justification, the apolitical nature of the latter but not the former, the role of values in science, and the relevance of feminism to science. Thus feminist empiricism makes it possible (for Nelson and Campbell) or even mandatory (for Longino) for science to investigate and assess the adequacy of its own norms of practice, and to revise them in the face of new evidence.

While Nelson may be somewhat soft on Quine in places (specifically with respect to his behaviourism), I find her use of his views as a starting point from which to launch a feminist empiricism quite compelling. She is not soft on Quine on the central question of the need to critically assess the role of values in science: Quine says we cannot, while Nelson says we must. Quine thinks that we lack the right kind of evidence for such a task, while Nelson points out that Quine's willingness to give an evolutionary account of the origins of human values belies his evidential complaint.[58] Lest one still hold out against any ideas associated with the authors of the traditional canon, Campbell's and Longino's proposals are non-Quinean. In the confluence of the three positions we can find a very workable feminist empiricism. Campbell offers the important argument about the inescapable interconnectedness of the contexts of discovery and justification, already mentioned,

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[58] Nelson, *WK* pp. 132-133

and adds a further valuable refinement to our understanding of feminist empiricism.

The most common objection to feminist empiricism is that the very attempt to connect these two ideas, feminism and empiricism, is incoherent. Harding certainly makes this assumption, and Campbell points out that it is a fairly commonplace position within feminist circles.[59] On Harding's view, says Campbell, any scientific accomplishment of the political goals of feminism is at most a by-product of the application of norms which themselves lack any political content.[60] The attempt to introduce explicitly feminist values within empiricist science is supposedly ruled out because empiricist science permits no room for such political norms. Campbell addresses this worry very elegantly by distinguishing between what he calls internal and external feminist empiricism. The external variety, which is what Harding calls feminist empiricism, is inadequate, for the very reasons she cites. But an internal feminist empiricism which is both coherent and still properly empiricist remains possible. Campbell argues that if empiricists accept epistemic norms like the ones discussed above, then the idea that there is some "pure" form of empiricism, where hypotheses are tested against

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[59] Campbell (1994) p. 95. It is even more commonplace outside feminist circles; see Clifford Geertz, "A Lab of One's Own", *New York Review of Books* (1990)

[60] Campbell (1994) p. 92

evidence in isolation from all political influences, lacks all sense. As already emphasized, many empiricist epistemic norms are essentially comparative and thus require that reference to the context of discovery be made in the process of hypothesis testing. The observations made in order to confirm a hypothesis are themselves affected by the context of discovery, in the dual senses that observation is theory-laden, and is performed using instruments whose reliability depends on theory.[61] The theories in question lie in the context of discovery.[62] The only way a good empiricist could make these frequent and necessary forays into the context of discovery, while still maintaining that justification is unaffected by political, personal and other factors, is either by stipulation, or by further dividing the context of discovery into politicized and non-politicized sub-contexts. Neither option is satisfactory.

I have said that feminist empiricism enjoys a constructive advantage over other feminist epistemologies.

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[61] Of course the reliability of scientific instruments depends also on the skill of instrument-makers, who are firmly embedded in the context of discovery. See Addelson (1983) p. 183 fn. 6

[62] Kathleen Okruhlik makes a similar point in "Birth of New Physics or Death of Nature?", where she argues that when rational theory choice is seen as essentially comparative and when theories are generated in a deeply sexist culture, non-sexist alternatives will not even have a chance to emerge. The solution, similar to Campbell's, is to reclaim the epistemic significance of the social and political factors involved in generating theories. Cf. Harvey and Okruhlik, eds, *Women and Reason* (1992) pp. 73-74

It is in this regard that I find Helen Longino's approach, while not without significant obstacles, appealing. Nelson and Campbell both appear content to have established that a good feminist empiricism is one that gets the concerns of feminist science critics onto the agenda of science, and forces empiricist scientists, as a matter of consistency, to deal with those concerns. In adopting this position, Nelson really demands of Quine (and those who follow him) that he conform to the position he has articulated. If Quine or any other empiricist believes that the task of evaluating science is itself part of science, then feminist claims about science (including claims about its political nature and the role of values) are part of the evidence that scientists must now take into account. No claims are made in advance about the likely outcome of this investigation, avoiding the worry about "self-fulfilling prophecies" mentioned above.[63] Campbell does not state the case in Quinean terms, but he too argues for a feminist empiricism that is "able to view its own methodology as the product of social construction and hence subject to empirical inquiry

[63] In fact Nelson recently claimed (in discussion) that many of the results of science might emerge from the process virtually unchanged. L.H. Nelson, "On What We Say There Is and Why It Matters", paper read to the Canadian Philosophical Association, Ottawa 1993. Nelson also permits the possibility that some of the central claims of feminism may be modified or rejected in the process of examination and critique. It is partly for this reason that feminist empiricism may have the strength to which Nancy Tuana points, to resolve tensions between feminisms.

and revision."[64] Thus feminist empiricism places feminist claims on the table for consideration in a manner not readily available to other feminist approaches. Many feminist theorists have by now recognized that feminist standpoint and feminist postmodern epistemologies may be hamstrung by the daunting task of having to convince scientists of the plausibility of these epistemological stances before their specific criticisms of science can be broached. Feminist empiricism, it appears, faces no such obstacle. It preserves the power and potential of science, but asks it to face new challenges. It holds out the possibility of a science that is better by its own and by feminist standards.

Longino's strategy is somewhat different. She shares with Nelson and Campbell a reticence about making a priori predictions about the outcome of any reassessment of science.[65] But she differs from them in placing greater emphasis on two features of empiricism. The first of these is the question of evidence itself.[66] Longino points out that there are no obvious clues to be found in states of affairs to identify them as evidence, nor what they might be evidence for. The possible permutations of evidential

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[64] Campbell (1994) p. 90

[65] Longino, *SSK* p. 52

[66] Nelson addresses this question as well, but with an eye to debunking historical "Cartesian" assumptions about the nature of evidence. See *WK*, chapter 7.

relations are complex, because facts are taken as evidence in light of other beliefs and assumptions.[67] The same state of affairs may provide evidence for differing and even contradictory hypotheses, the same state of affairs may be taken as providing evidence under one description but not under another, and so on.

This is an important discussion from the point of view of the demand that feminist critiques of science be taken as evidence for science, because it suggests that earlier optimism about the power of feminist empiricism, based on the requirements of simple consistency, may have been rather hasty. If claims are taken as evidence in light of background assumptions, but the background assumptions of science are predominantly sexist, racist, and class-determined, then any claims that seek to challenge these biases are less likely ever to be seen as candidates for evidence. The only way out of this quandary seems to be to change the relevant background beliefs, and that task will be a very difficult one. The obstacle that confronted feminist standpoint and feminist postmodern epistemologies has been pushed back a step, but not resolved. I contend that feminist empiricism continues to enjoy the advantage here, because although the sexism (racism, classism) of science must still be confronted, the burden of proof shifts to the more traditional empiricist, who must give a

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[67] Longino *SSK*, pp. 40-50

satisfactory explanation of why feminist observations are irrelevant, and why methodological issues need not be addressed. It is a fairly slim advantage, though, and feminist empiricists should be aware of this.

The second feature of feminist empiricism that Longino approaches somewhat differently is the idea that criticism of science, including its norms, methodology and intersection with social context, must be made part of the practice of science. As we have already seen, this insight is not entirely new to feminist theorists. What is new in Longino is the proposal of a concrete set of criteria for ensuring the objectivity of such criticism. Longino promotes objectivity as a function of maximal participation and consensus in the scientific community. She argues that since the objectivity of scientific communities is dependent on the depth and scope of their transformative interrogations, such communities will be objective to the extent that they follow what she claims are the strongest possible rules for facilitating such interrogation.[68] Scientific communities, she asserts, must be compelled to provide recognized avenues for criticism, shared standards, community responsiveness, and equality of intellectual authority.[69]

I think that feminist empiricism needs something very

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[68] Longino, *SSK* pp. 76-82

[69] ibid. p. 76

like Longino's insight here. It is one thing to foster an epistemological position that permits the empirical investigation of science, as Nelson and Campbell do.[70] It is another to require such investigation, and to propose principles for its conduct, as Longino does. Nelson's and Campbell's positions are somewhat too weak in this regard. But is Longino's too strong? It would certainly be tempting to object that Longino, as a proponent of consensus-building in science, has no business dropping a set of criteria for the objectivity of scientific communities on the table and then demanding that they be adopted. Such an objection can be disposed of easily enough, by pointing out that she is engaged in prescriptive philosophy of science, and that the normative requirements she sets forth are to be negotiated. It is better to begin such negotiations with some substantive issues. But a more pressing problem lies with the implementation of the fourth criterion, the requirement of equality of intellectual authority.

Longino calls this a "Habermasian criterion" intended to "disqualify a community in which a set of assumptions dominates by virtue of the political power of its adherents."[71] She offers the obvious example of the dominance of the Lamarckian doctrine of evolution in 1930s Soviet science, which is obvious precisely because this

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[70] See Nelson, *WK* p. 173, 308, and Campbell (1994) p. 90

[71] Longino, *SSK* p. 78

doctrine was not merely assumed but politically enforced. The same sort of dominance functions at a less obvious level when unquestioned assumptions about sex and gender infiltrate the construction of knowledge in biology and the social sciences. However, even assuming that such a criterion would gain acceptance at the negotiation stage, it remains unclear how Longino envisions its fulfillment.

Kathryn Pyne Addelson points out that there are already knowledge hierarchies in place within science, such that the "authority to define the nature of the living and non-living world around us", usually attributed to science in general, is in fact held by only a sub-group of scientific elites.[72] Those at the top of a "prestige hierarchy" will earn more, employ greater numbers of underlings, publish more frequently, obtain more research funding and greater access to facilities for research, author the definitive textbooks in their field, win prizes, and serve as experts when consulted by those outside their field. She argues that science will be made more, not less rational, by taking the social aspects of prestige hierarchies into account. One cannot simply legislate equality of cognitive authority in the face of such differences. As we saw in chapter three, junior researchers who are critical of the work of senior colleagues are not

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[72] Addelson, "The Man of Professional Wisdom" (1983) p. 167

merely ignored because of their lower position in the prestige hierarchy. Their livelihood is threatened. Longino must be aware that the requirement for equality of cognitive authority cannot be implemented without sweeping changes in the way science is practiced and the ways it relates to other, less obvious social factors.[73] The phrase "political power" in Longino's rejection of communities wherein dominant assumptions are enforced via the political power of their adherents must certainly be understood very broadly.

Since feminist empiricism uses evidence as the thin end of its wedge, it behooves feminist science critics to amass evidence, as much and as well as possible. (We have already seen that this task will be a time- and resource-consuming and frustrating one; nonetheless feminist empiricism demands it.) The feminist educational program to involve more girls and women in science and mathematics retains and even increases its value. The corrective agenda in feminist science criticism remains as crucial as ever. I think feminists ought also to study the presentation of certain scientific explanations. Some such explanations seem constructed for the purpose of eliciting evidence of a

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[73] In addition to economic factors, there may be psychological and other barriers to the presumption of equal cognitive authority in some scientists, hence the need to retain some role for the influence of the individual. See Barber, "Resistance by Scientists to Scientific Discovery" (1961). The narrative proposal of chapter five is intended in part to address such resistance.

particular kind, and in a particular way, while other explanations are constructed to obscure the nature or even the lack of evidence. This study will occupy us in the next chapter.

IV. Realism

Empiricism as a metaphysical commitment has been held in opposition to scientific realism. Here, however, we are dealing with empiricism as a theory of evidence, enabling us to consider the viability of some other form of realism. As I pointed out in chapter two, Evelyn Fox Keller is noteworthy among feminist science critics for her insistence that, despite efforts to impose a masculine will on the construction of reality, the world places limits on those efforts.[74] Keller is a realist who recognizes that the constraints of realism are really very weak, and that language, especially metaphoric language, further weakens those constraints. She writes,

> "Metaphors work to focus our attention in particular ways, conceptually magnifying one set of similarities and differences while dwarfing or blurring others, guiding the construction of instruments that bring certain kinds of objects into view, and eclipsing others."[75]

Language is thus a very powerful force in the scientific construction of reality for Keller. Nevertheless, she insists that "it would be *foolhardy* to lose sight of the force of the material, nonlinguistic substrata of ... that which we loosely call 'nature'".[76] Keller's choice of the

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[74] Keller, *SLSD*, pp. 33-36

[75] Ibid. p. 33

[76] ibid. p. 33, emphasis added

term "foolhardy" in this passage implies that her commitment to realism is motivated by something far more important than practical considerations. She argues that there is a world of "prelinguistic and pretheoretical phenomena, constraints, and opportunities in which we must reside, and with which we, *as part of that world*, must negotiate our survival."[77]

Ian Hacking, among others, embraces a more practical solution. He points out that philosophy has focused too much on whether science is a body of logically related knowledge propositions which represent the world, and insists that paying attention to the practice of science means paying attention to experimentation. That attention, in turn, yields a very elementary sort of realism, as evinced in his infamous pronouncement on using a spray of positrons or electrons to alter the charge on a niobium ball: "if you can spray them then they are real".[78] In other words, "we shall count as real what we can use to intervene in the world to affect something else, *or* what the world can use to affect us."[79] This latter criterion is of particular interest, as the discussion of Scheman and Campbell, below, will indicate.

John Dupre extends Hacking's pragmatic point to the medium-sized furniture of the universe, and his realism has

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[77] ibid. p. 4

[78] Hacking, *Representing and Intervening* (1983) pp. 22-23

[79] ibid. p. 146

much in common with Keller's. While realism is sometimes taken as equivalent to an aggressively reductive form of physicalism, Dupre rejects such reduction in favour of a realism that permits many different descriptions of the things in the world without insisting on the ultimate or final reality of any single level of description. He calls his position "promiscuous realism", and argues that the extension of the label "real" to all of the many possible characterizations of objects appropriate for their investigation under different scientific headings (and under non-scientific headings) is no threat to their ontological robustness.[80] Consequently reductionism is untenable, and science will remain disunified. The fact that there are many ways of dividing up the world is not a threat to its reality.[81]

As a Quinean, Lynn Nelson adheres to the view that "our very notion of things is just a conceptual apparatus that helps us to foresee and control the triggering of our

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[80] Dupre, *The Disorder of Things* (1993) p. 262

[81] It is not clear from Dupre's discussion of science and values, which includes comments on feminist philosophy of science, whether he promotes the disunity of science as an ideological as well as a philosophical position. It strikes me that some feminist critics treat science as though it were unified, and that it would advance feminist political strategy to insist that it is not. This would provide a means for dealing with the question of the transferability of expertise across fields, as discussed in chapter three. See Dupre (1993) especially chapters 10 and 11.

sensory receptors".[82] Our everyday ontology may embody a commitment to physical objects existing independently of ourselves, but this belief, like all beliefs, is not immune from revision. Nonetheless, it appears that Nelson's position is in places compatible with the view that the world places constraints on what we believe.[83] Helen Longino is prepared to concede a minimalist form of realism, claiming that "there is a world independent of our senses with which those senses interact to produce our sensations and the regularities of our experience. There is 'something out there' that imposes limits on what we can say about it."[84] These positions are much like Keller's, Hacking's, and Dupre's.

Harding does not address the question of realism per se. We can gather from her many remarks about the impossibility of establishing one true picture of the world, about the failure of science to provide a mirror reflection of nature, and about the need to replace false theories

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[82] Quine, "Things and Their Place in Theories" p. 1, in W.V.O. Quine, *Theories and Things* (1981)

[83] In a very densely written passage, Nelson points out that we do not experience the firings of our sensory receptors, we experience the world. By "the world" she does not mean nature, however. She means something more like our theories of nature. She argues, then, that our experience of theories constitutes the major constraint on what it is reasonable to believe. However, it is not clear that we can be said to experience theories any more than we can be said to experience sensory firings. See Nelson, *WK* p. 276

[84] Longino, *SSK* p. 222

about the world not with true ones but with less false ones, that she is at least ambivalent about the metaphysical status of the world. Such ambivalence must answer to Jean Grimshaw's observation that "it is difficult to make sense of much feminist criticism of male-biased theories without supposing that the latter have in some way *misdescribed* reality, *misrepresented* how things are."[85]

Richmond Campbell takes up this theme in the context of his feminist empiricism. On the one hand, he points out that there would be little point to the imposition of comparative norms like explanatory power and predictive success in science if we did not believe that the differences between the predictive and explanatory potentials of theories were a consequence of independently existing features of the world.[86] On the other hand, he adds that "our evolving views of the world, including feminist views about the nature of science, imply that there are important features of the world (*for example, androcentrism in science*), that exist whether or not we notice them."[87] Thus, in order to make sense of the entire feminist project for science, and not just its corrective agenda, realism is necessary to account for the successes

[85] Grimshaw, *Feminist Philosophers: Women's Perspectives on Philosophical Traditions* (1986) p. 94

[86] Campbell (1994) p. 102

[87] ibid. p. 104, emphasis added

and failures of both science and feminism. Realism is needed to make feminist correctives to science compelling, but also to explain how the androcentrism of science was concealed from women, the erosion of that concealment, and the ongoing difficulties that feminists experience in getting others to notice androcentrism as an independently existing feature of the world.

Naomi Scheman makes observations that share in the elementary commitment to some constraining reality, but her further insights are also important. Her realism bears on knowers and their interdependency with one another, with language, and with objects, and thus on highly complex states of affairs. At this level of complexity, the notion of an independently existing reality cannot provide the kind of simplistic constraints that a physical object ontology can, but that does not mean that it provides no constraints at all. One reason that we know these complex situations to be real, Scheman says, is that they look different to people who are placed differently in relation to them. "The only way to take diversity of perspectives seriously is to be robustly realistic", she argues.[88] Thus she points out that the kind of epistemology that has historically denied the influence of different perspectives and has held out against the relativism that such perspectives threaten is in

[88] Scheman, "Though this be Method, Yet There is Madness In It" in Antony and Witt (eds) *A Mind of One's Own* (1993)

fact incompatible with realism. I think that feminist philosophers of science ought to take this "basic" form of realism seriously, for the reasons Campbell and Scheman cite.

Realism is resisted, however, because it is often taken to be a foundationalist notion, providing the ultimate metaphysical ground for all knowledge claims. This connection is particularly pronounced in Keller's work, which has been comprehensively criticized on this count. The support of realism for foundationalism impinges on broader philosophical debates lying outside the confines of this essay. It should be mentioned that the potential philosophical resources for breaking this support are many and diverse. One may, for example, appeal to Putnam's internal realism,[89] Fodor's distinction between epistemological and psychological foundationalism,[90] a Sellarsian explanatory coherence[91], or a Davidsonian externalism in order to defend a feminist philosophy of science that is both realist and anti-foundationalist.[92] (Note that one could not appeal to all of these at the same

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[89] cf. Hilary Putnam, *Reason, Truth and History* (1981) and *Representation and Reality* (1988) especially chapter 7

[90] Fodor, *The Modularity of Mind* (1983)

[91] Sellars, "Empiricism and the Philosophy of Mind" in *Science, Perception and Reality* (1963) and "More on Givenness and Explanatory Coherence" (1979)

[92] Davidson, "Epistemology Externalized" (1991)

time.) In any case, since the feminist need of realism is compelling without being unduly restrictive, some such defense ought to be undertaken.

The sort of realism at stake in much of the preceding discussion is of a fairly conventional, even dull metaphysical sort. Many philosophers affix the label "naive" realism here, (whether or not it is appropriate) as a gesture of dismissal, preferring to look instead at the myriad representations of what we (unthinkingly) call reality. "No Reality Without Representation" is the rallying cry that launches the project of interrogating the cultural construction of reality and the concomitant effacement of such construction from scientific discourse.[93] This project is plagued by a "complacent and dogmatic antirealism",[94] especially in its literary mode. Feminist science criticism is deeply committed to the value of making science itself yield up its secrets, but as the views of Scheman and Campbell make clear, this commitment must include some metaphysical position robust enough to sustain the emergence, continuance, and occasional failure of feminist scholarship and politics.

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[93] George Levine, "Looking for the Real: Epistemology in Science and Culture" (1993), p. 18; Levine mentions that at a 1989 conference on realism and representation held at Rutgers University, t-shirts bearing the slogan "No Reality without Representation" sold out at once. (Shirts with the counter-slogan "Get Real" are still available.)

[94] ibid. p. 9

As our understanding of the nature and potential of feminist empiricism becomes more sophisticated, Harding's initial objections to it begin to wither away. Harding's distinction between "spontaneous" and "philosophical" feminist empiricisms indicates her acknowledgement of this fact. Nelson makes a major contribution to that withering with her insistence that a consciously political perspective is required in order even to raise questions about the social and political contexts within which science is practiced as factors affecting the content of science. Campbell presses even further with his refinement that such a perspective is not a violation of the traditional empiricist boundary between science and values, but is actually made possible by empiricist scientific practices. Both therefore clear a space for the consideration of feminist claims about science as evidence which challenges conventional androcentric accounts of science. Such efforts fail to meet Harding's demand for a theory about science in order to eliminate scientific victimization of women, but turn the demand on its head. Nelson and Campbell use feminist claims about the scientific victimization of women in order to reconfigure empiricist theories about science. Longino goes further still with very concrete prescriptions for how this reconfiguration ought to proceed. I have argued that the combination of these three stances provides feminist science with a very powerful and potentially very fertile resource. I have also claimed that realism, far from contravening this potential, in fact enhances it. I will now focus on a possible feminist use of this resource.

CHAPTER FIVE

WHY DOES NOTHING HAPPEN WHEN ANYTHING GOES?

Herbert Burhenn has observed that "the philosophical yield of introducing the concept 'narrative' into discussions of explanation has frequently appeared to be very small".[1] This remark is made against the background of a debate in philosophy of science in which various individuals have questioned the permissibility and efficacy of narrative (or "historical") forms of explanation. Narrative seems *prima facie* to fulfill some human need rather than the stringent requirements of logical justification. Carl Hempel has cautioned that the mere attainment of psychological understanding is not adequate for the purposes of science because it does not lend itself to objective test.[2] Thus disciples of Hempelian ideals are (or were) suspicious of narrative. Some even tried to show that narrative could (or must) be retooled so that it would fit the deductive-nomological model of scientific explanation.[3] Erosion of support for the D-N model suggests that the time is ripe to consider narrative anew. In this

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[1] Burhenn, "Narrative Explanation and Redescription" (1974)

[2] Hempel, *Philosophy of Natural Science* (1966) pp. 47-48

[3] See, for example, M. Ruse, "Narrative Explanation and the Theory of Evolution" (1971)

chapter I will argue that an understanding of narrative in science combines a significant philosophical yield with feminist transformative potential.

I. Notes on Idiom, and a Narrative Bridge

There is an abundance of narrative and literary approaches to most intellectual affairs. Telling and hearing tales are everywhere identified as activities to which seekers of insight must attend in their ongoing efforts to understand human nature and the things we hold dear. Although this view dominates contemporary literary theory,[4] it is also found in other disciplines, few of which are outwardly concerned with narrative as such, including philosophy. Richard Bernstein notes that recent philosophical emphasis on the importance of narrative should be regarded as a reminder rather than as a new insight because, he says, "narrative discourse has always been important for philosophy".[5] He observes that "typically, every significant philosopher situates his or her own work by telling a story about what happened before he or she came along - a story that has its own heroes and villains".[6]

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[4] See, for example, Roland Barthes, "An Introduction to the Structural Analysis of Narrative" (1975), in which he remarks that narrative "is present at all times, in all places, in all societies; indeed narrative starts with the very history of mankind; there is not, there has never been anywhere, any people without narrative".

[5] Bernstein, "The Rage against Reason" (1988) p. 186

[6] ibid.

Although Bernstein does not wish to make reliance on narrative a condition for philosophical significance, it is a common stylistic feature.

I am interested in narratives as they are used by scientists, especially by sociobiologists. Scientific narratives are instruments for the organization and understanding of data, and they are no less significant to the process of science than its more formal tools. For the purposes of my discussion, I wish narrative to be understood rather simply, as the presentation of conjectural histories. I will use the terms "narrative" and "tales", "fables", "stories" or "story-telling" interchangeably. I am aware that these terms carry baggage of their own, and that the reader's familiarity with other ways of thinking about narrative will influence her or his assessment of my position. But recognition of the function(s) and value of narrative takes many forms, and not all of these will be relevant in what follows.[7] The diversity of perceptions and techniques surrounding narrative means that the very terms in which I will present my position are not just loaded, but overloaded.

I have already said that many philosophers now

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[7] For example, work in the lineages of Cassirer, Vico, Lyotard, Gadamer and Ricoeur will not be explored. My argument is not related to the "interpretive turn", i.e., to hermeneutic and structural approaches to science, and I am not at all interested in what has recently been called "Philosophy as/and/of Literature" (Danto, 1985).

acknowledge the ubiquity and power of narrative. Alasdair MacIntyre, for example, has shown the need to conceive of our lives as historically or structurally unified in order to make sense of our motives and actions.[8] For MacIntyre, narrative provides the contextual cohesion required to assess the moral worth of our behaviour. Similarly, Carole Pateman has written that "telling stories of all kinds is the major way that human beings have endeavoured to make sense of themselves and their social world",[9] and she reads the contract theories of social and political philosophy as archetypal versions of such stories. Pateman argues that the sexual dimension of the social contract has been repressed, and she sets out to retell those stories.

In feminist epistemology, Lorraine Code, Hilary Rose, and Donna Haraway (among others) advocate the use of literature as a tool for enhancing insight. This is rather different from either assuming or fostering an interest in narrative in the way that I recommend, but I share in some of the underlying motives. Rose and Haraway are especially interested in the political value of the transformed futures offered in feminist science fiction. Haraway's position, especially as expressed in her 1989 book *Primate Visions*, sees feminist retelling of the narratives of primate anthropology as vital to new understandings of the field.

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[8] MacIntyre, *After Virtue* (1981) p. 191

[9] Pateman, *The Sexual Contract* (1988) p. 1

She interrogates the privileged place western science grants to monkeys and apes in the "border zone" between nature and culture,[10] and the ways in which women's explorations of this zone have challenged both our understandings of primate social dynamics and the role of women in science. Rose adds that "the literary genre *describes* in order to mobilise us to *identify* and or to *rebel*, something which a scholarly genre is *inherently less capable* of doing".[11] The tension that exists in this passage, between identifying "and or" rebelling, will figure vitally in what follows. I want to point to the presence of narrative *in* scientific theorizing, however, and not just construct a new narrative *about* science.

Telling new stories about our primate cousins highlights the ways in which older androcentric narratives have reinforced patriarchal ideology in human society, and made it appear "natural". Haraway is careful to insist that this transformation is not the result of any essential "feminine" differences that women bring to their work in the field, but is "an *historical* product of their positioning in particular cognitive and political structures of science, race, and gender".[12] Haraway focuses on the possibility of

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[10] Haraway (1989) p. 1

[11] Rose, "Reflections on the Debate Within Feminist Epistemology" (1988) p. 134, emphasis in original

[12] Harway (1989) p. 303, emphasis in text

challenging widely accepted data-gathering and interpretive practices by offering alternative narratives informed by feminist sensibilities. I would add that, in the case of sociobiology, it is not merely androcentric bias but the structure of the narrative itself which functions to conceal the cognitive and political factors in question. Readings of conventional narratives must therefore be attuned not only to masculine bias but to structure. The narrative path worn smooth by frequent traffic is often the one that should arouse the most suspicion.

Code's argument for the importance of literature is more general. "From good literature", she writes,0 "one can come to understand hitherto unarticulated aspects of human experience and hence to know oneself better both as an individual and as a creature of a certain kind".[13] Thus, among other things, Code's commitment to taking narrative seriously includes the need to treat persons as both individuals and as community members. I therefore explore narrative in part because it is closely attuned to the double-aspect view of knowledge as constructed and held by both individuals and communities, for which I argued in chapter two.

A few commentators on evolutionary theories draw an explicit connection between story-telling as a central feature of such theories and story-telling as a factor in

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[13] Code, *ER* p. 223

human comprehension. Many contemporary philosophers of science insist that science needs to be described from a more holistic perspective, as a process that is thoroughly interwoven with other threads of social, cultural and historical life. How odd, then, that so few have noted the further affinity between studying narrative and science as uniquely human, and *related*, activities.[14]

The philosopher's recognition of and reliance on narrative discourse is not particularly shared (at least not explicitly) by those working in the sciences[15], with one notable exception. It is virtually *mirrored* in the field of evolutionary biology and its subfields, and so provides an important bridge between philosophy and evolutionary theory. Sociobiologists, and evolutionary theorists in general, are faced with evidential limitations seldom found in other sciences.[16] Thomas Goudge points out the obvious when he

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[14] Alasdair MacIntyre comes closest to making such a connection in "Epistemological Crises, Dramatic Narrative and the Philosophy of Science" (1977)

[15] D.P. Verene has argued that science actually *resists* narration, that narration as a rhetorical form is not generated by science, but comes from "outside" of science and can be used by it. I see no reason to accept such a constraint on the natures of science and narrative. See Verene, "Metaphysical Narration, Science and Symbolic Form" (1993)

[16] Although cosmology shares with evolution its status as an historical science, it has far greater theoretical and evidential resources available to it, including the whole of nuclear physics, and the presence of trace elements, particles, and temperature variations on earth and throughout the universe. The precision with which predictions about the origins of the universe, down to "10^{-43} seconds ABT" (After the Beginning of Time) have been made and confirmed is really

reminds us that the histories of all the populations that have ever existed are not directly accessible. "They can only be got at through the records or traces of them which have survived and are utilizable by those conducting the inquiry", he writes. "Moreover, hypotheses and assumptions have to be introduced; and the more fragmentary the records or traces, the more elaborate must the scheme of hypotheses and assumptions be."[17] The fossil record is exceptionally fragmentary. Even if it were abundant, however, fossilization preserves neither soft tissue nor genetic material, so many of the hypotheses posed by evolutionists and sociobiologists are in principle unverifiable by direct means. It is not merely that the fossil evidence is sketchy and incomplete, however. Even with perfect knowledge in this area, we cannot see adaptation and natural selection. How, for example, does the adaptation for speed in a horse's leg balance off the internal weakness of its structure?[18] Any answer must be conjectural, and it is here that narrative explanations gain a foothold.

Sociobiologists have, of course, been attacked for the indirect means they use to verify their stories, including anthropomorphic observations of animals, especially

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quite breathtaking. See Timothy Ferris, *Coming of Age in the Milky Way* (1988) for a popular account.

[17] Goudge, *The Ascent of Life* (1961) p. 34

[18] Thanks to Mohan Matthen for emphasizing this point, and for suggesting the example.

primates, and evidence based on extant hunter/gatherer societies.[19] More vehement, however, is the attack on fiction itself as used to flesh out scant evidence. Helen Longino and Ruth Doell sum up the situation as follows: "The distance between evidence and hypothesis cannot be closed by anatomical and physiological knowledge, by principles from the theory of evolution, or by commonsensical assumptions. It remains an invitation to further theorizing or, as some would have it, storytelling."[20]

Narrative is in many ways the most important part of the explanatory apparatus that sociobiology borrows from its more "respectable" kin in evolutionary biology. Narrative is a (if not the) critical shared element between sociobiology and other evolutionary theories, and is shared with many other types of scientific explanation. Absent the ethnocentrism and the unprincipled use of hypotheses and results from other fields, sociobiology still shares with other evolutionary theories a central and inescapable reliance on narrative presentation of its claims. In fact, the utilization of the narrative approach to explanation in evolution pre-dates the "new" synthesis which marks the

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[19] It is in particular pointed out that however interesting primates and extant hunter/gatherers may be, they are not our ancestors. Cf. Nelson, *WK* p. 352 fn. 119

[20] Longino and Doell, "Body, Bias and Behaviour: A Comparative Analysis of Reasoning in Two Areas of Biological Science" (1987) p. 175

emergence of sociobiology by a century.[21] Even the philosophical consideration of the admissibility of historical explanation pre-dates the new synthesis. (It is not always clear where evolutionary theorizing leaves off and sociobiology begins. Sociobiologists tend to exploit this ambiguity.)

The weak interpretation of the function of narrative in sociobiological explanations is that it makes the evidence "hang together" more neatly and convincingly. It is to be hoped that "more data and increasingly rigorous hypotheses will eliminate for good the subjective element" found in evolutionary theorizing.[22] This is essentially the position that Goudge defends. The stronger version, however, argues that the evidence for sociobiology is so sketchy, and the support it lends so uncertain, that it is the stories that end up doing the explaining. The stories of sociobiology cross the line between being representational devices which enhance explanation, and explanation full stop. What is more, the presence of stories serves to obscure the inadequacies of the available evidence, by making it look as though they are functioning only to impart historical structure to that evidence.

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[21] E.O. Wilson's 1975 book on sociobiology was subtitled *The New Synthesis*, thereby making allusion to Julian Huxley's 1942 *Evolution: The Modern Synthesis*, and placing Wilson's work, perhaps inappropriately, in the tradition of Huxley, Ernst Mayr, J.B.S. Haldane, and Sewall Wright.

[22] Landau (1984) p. 262

The debate over sociobiology tends either to focus on its misbegotten use of legitimate scientific concepts, or to dismiss narrative explanations from science. The feature of sociobiological explanations that is of interest to me here is their narrative character, but I would be remiss if I did not at least comment upon this other class of objections. Rose, Lewontin and Kamin have argued that sociobiological explanations have three features in common.[23] First, such explanations appeal to ethnocentric experience as evidence for their universality. The white, middle-class American family of the 1950s, and the culture in which it was embedded, is found throughout the natural world. Second, sociobiological explanations assume that genes may arise with any arbitrarily complicated action the theory requires. It is for this reason that geneticists are particularly disdainful of sociobiology. They object to the way in which sociobiologists use the rhetoric of genetics, "without acknowledging the conceptual and experimental constraints that are assumed by geneticists".[24] The illegitimacy of this theoretical scavenging is created by the misuse of the foundational terms of genetics, in order to invoke the authority of genetics as the grounds for sociobiological

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[23] Lewontin, Rose and Kamin, *Not in Our Genes* (1984)

[24] Howe and Lyne, "Gene Talk in Sociobiology" (1992); although Lyne is not a biologist, Howe is, and moreover their objections are endorsed by other scientists, including Lewontin.

analyses of human behaviour, as discussed in chapter three. Finally, Rose, Lewontin and Kamin claim, sociobiological explanations contain adaptive stories, with no quantitative check on whether those stories are really supported by variation in reproductive success.

It is clear that two claims have been conjoined here. For one thing, sociobiologists invent stories. As I have argued above, the nature of their field seems to require it. But those stories often fail to come in contact with evidence in certain required ways. In much of the debate that has arisen around the telling of stories by sociobiologists, some commentators give undue emphasis to the mere telling of stories, not realizing that this much is inevitable in evolutionary biology in general. Recall Nelson's argument, cited in chapter three, that we cannot fault sociobiology for borrowing concepts, hypotheses and results from other fields of science, because all scientists borrow from outside their field. If we were to outlaw the practice, science would grind to a halt. However, as I remarked earlier, it is not borrowing *per se* that is problematic in sociobiology, it is what is being borrowed that needs to be scrutinized. Similarly, it is not the mere telling of stories that ought to arouse our critical suspicions, but the actual stories told, and for similar reasons. If we were to forbid story-telling as an acceptable form of scientific explanation, or at least as an acceptable adjunct to such explanation, we would find ourselves ruling out much of what currently passes for respectable science.

When story-telling is considered in current science criticism, it frequently meets with hostility. The explanation for this rancour is not merely a worry about treating science in too "literary" a mode.[25] Many science critics argue that the stories of sociobiology (and, sometimes, of other evolutionary theories) are blatant attempts to disguise ideologically laden, excessively deterministic accounts of human nature in the language of reasonable scientific hypotheses. These narratives are not explanations in any sense, the critics assert. They are "merely" stories. But the real problem is the kinds of stories told, and the way they operate in conjunction with assumptions about evidence, and with social values, that causes trouble. What is more, there is an opportunity worth exploiting in the fictionalized gaps of sociobiology.

Response to the recognition of the centrality of narrative in sociobiology and other evolutionary theories takes many forms. Some critics reject the admissibility of narrative outright.[26] Unfortunately, this is a risky move,

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[25] This worry is articulated by George Levine, "Why Science Isn't Literature", in Alan Megill (ed.) *Rethinking Objectivity* (1994)

[26] Longino and Doell appear in places to do so, as does Evelyn Reed. In *Sexism and Science* (1978), Reed compares Elaine Morgan's "aquatic ape" hypothesis with E.O. Wilson's

as narrative is an accepted scientific practice. In addition to the striking example of evolution, science relies on narrative in myriad vital ways. The relevance of bodies of evidence to particular questions is established narratively, the interpretation of both theory and evidence takes narrative form, the task of saving the phenomenon relies on narrative, and the adjustment of background hypotheses in order to preserve a theory is a job for narrative. One may wish to object that none of these is an instance where narrative is a necessary element, but that will not do. To take just one example, it is by now fairly well established that evidence is not "self-announcing", but is identified by us with reference to background theories and assumptions. If it is not the case that narrative plays an essential role in connecting what is observed to the observer's background, then for practical purposes, evidence *is* self-announcing, and there is no need to worry about the theory-ladenness of observation or the underdetermination of theory by evidence. Finally, as I indicated at the outset, reliance on narrative is not just accepted scientific practice, it is a widespread phenomenon that many have

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sociobiology, conceding that at least Morgan's book "has the merit of a refreshing new type of biologism compared to the stale myths of eternal male supremacy", but she rejects all invention of "fanciful hypotheses" just the same. (p. 48) argued is indispensable to human understanding.[27]

Some commentators have tried to remove the sting of story-telling by redescribing it in respectable, scientific terms.[28] The observations about narrative in the paragraph above are seen as overzealous descriptions of the nature of interpretation in science. Evolutionary explanations in particular are inherently historical, they argue, but that does not mean that in interpreting them we should slide with undue haste into a wishy-washy literary mode. Some constructions, it is conceded, may be understood as narrative devices, in order to give our accounts a certain historical structure. But "narratives", at bottom, fit the usual pattern of our best explanations. We gain nothing, and lose in terms of scientific rigour, by falling into the narrative trap. This approach is inadequate, for reasons already given. It is not just that sociobiological stories make our evidence hang together in a way that is both pleasing and revealing. In many, or even most cases, there is almost no evidence, so that the explanatory burden is borne disproportionately by the stories themselves. But one cannot explain things just by arranging them in a sequence.

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[27] L.O. Mink argues that narrative is a primary and irreducible form of human comprehension in "Narrative Form as a Cognitive Instrument" (1978)

[28] This is the line taken by Michael Ruse, "Narrative Explanation and the Theory of Evolution" (1971), and Brian Baigrie (in correspondence). David Hull argues that such emphasis is misplaced in "Historical Entities and Historical Narratives" (1984).

One could easily dismiss sociobiology as poor science, as ideology masquerading as science, or as "lunatic fringe" with few serious adherents.[29] It does seem a rather easy target. Any positive feminist project that uses so addled a "science" as sociobiology as its starting point for a general critique of science risks being taken less seriously. I think this line of reasoning should be resisted. First, as I argued in chapter three, the remedial project for the correction of bad science has been underrated. Women (and others) will benefit when scientific authority is used to unmask false and distorted claims that have been made about them by scientists including sociobiologists. As Donna Haraway remarks, "we are obliged to comment on the received texts. One does not start from scratch when E.O. Wilson has the professorship at the Museum of Comparative Biology (*sic*)".[30] Secondly, again relying on my argument in chapter three, it is often by interrogating bad science that we bring the most deeply embedded norms of scientific practice to the surface, and discover that these norms are sometimes very different from the ideals that scientists articulate in public. Those discoveries are worth making. Thirdly, the assertion that sociobiology can be ignored as bad science assumes the

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[29] I thank Sharyn Clough for pressing me to think more deeply about this issue.

[30] Haraway (1991) p. 77; the correct title is the Museum of Comparative Zoology.

validity of the very criteria of scientific certainty and authoritativeness which feminists are trying to challenge.

Finally, no matter how ill-regarded sociobiology may be within some scientific communities (and I am not convinced that it is so in all), it maintains a tremendous hold on popular consciousness. The central commitment of sociobiology, that there is a genetic basis for human social behaviours, is taught in college lectures, promoted in best-selling books, and assumed in media discussions of related issues. The fact that sociobiological ideas do not always travel under the term "sociobiology" is grounds for including discovery of its pseudonyms on our agenda. This is especially the case since the redescription of sociobiological ideas often wins them a new lease on the attention of a public unaware that they have been discredited under another name.[31]

Many feminist critics of sociobiology have favoured the replacement of its androcentric narratives with gynocentric ones.[32] In some instances this proposal is found in conjunction with a rejection of the admissibility of story-

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[31] For example, *Time* magazine recently offered a cover story, complete with the lurid headline, "Infidelity: Is it in Our Genes?", on discoveries made in the "new" field of "evolutionary psychology". Even a superficial reading of the material presented in this "new" guise is enough to convince the wary reader that evolutionary psychology just *is* sociobiology. See *Time*, "Our Cheating Hearts", August 15, 1994

[32] See especially Sarah Blaffer Hrdy, *The Woman that Never Evolved* (1981), and Evelyn Reed, *Woman's Evolution* (1975)

telling, which leads to some uncomfortable tensions. Longino and Doell, for example, sometimes refer to Man the Hunter as a "story", but Woman the Gatherer as a "framework", suggesting that the latter form of explanation is somehow more scientific and less narrative than the former.[33] Certainly they are correct to argue that generalizations about the use of chipped stone tools differ according to whether one takes male hunting or female gathering as a crucial adaptation in the emergence of social behaviour and organization. They are also correct to point out that Woman the Gatherer stories both highlight the androcentrism of Man the Hunter stories and provide more comprehensive and coherent explanations. But the choice of a female-centred framework of interpretation is still a narrative ploy. Yet the replacement of androcentric with gynocentric narratives is an important move. Such stories, by their very presence, challenge the hegemony of "conventional wisdom", and hint at its impoverishment. They make it possible for us to go further.

An early objection to adding Woman the Gatherer to the story of human evolution was that it reinforced traditional, middle-class patriarchal assumptions about the gendered

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[33] op cit p. 175; Longino and Doell also place strong emphasis on the fact that there is often, perhaps characteristically, no possibility of decisive evidence for the interpretation of fossils and stone artifacts, suggesting that we ought to be less reticent about acknowledging our limitations.

division of labour. Even though female labour (including reproductive labour) was identified as relevant to the emergence of our peculiarly human traits, many assumed that the central tasks of females were still oriented around home and hearth, that their dependence on males for food and protection went unchallenged, and that the inevitability of contemporary domestic arrangements remained rooted in our evolutionary past. This objection lacks sagacity, as it fails to take seriously the nature and extent of the doubts raised by the gynocentric framework. Male hunting had been so intimately linked to the emergence of human intelligence, including use of language and tools, that females appeared as evolutionary millstones around the necks of protohominid men. By recognizing that the activities associated with females required just as much rational capacity, and provided just as many opportunities for the development of tools and language, the dignity of women was assuaged. More important, though, was that the artificiality of the separation of male and female realms was stressed, and a space was cleared in which to recognize the importance of group interaction.

Woman the Gatherer remains firmly entrenched in the evolutionary tradition that regards terrestriality, bipedalism and upright posture, tool and language use, intelligence, and social organization as the features in need of explanation and the linear chronicle of evolution as the structure in terms of which they should be explained. There does not seem to be much room in this tradition for departure from these patterns, no matter how successful we may be in reconstructing the role of females in such explanations. Literary theorists have explored the idea that there are basic stories, or deep structures, to which our narratives conform. There may be different versions of the same structures, tailored to time, place and mode, but the basic form can be found in all our stories.[34] One way to proceed from the recognition of the centrality of evolutionary narrative, then, is to identify its deep structure.

Using as her model certain structural studies of literature, Misia Landau identifies the evolutionary stages through which early hominids are thought to have progressed, and what each stage means.[35] She then examines the views of six early evolutionists, and compares how their theories compare with one another and with the overarching structural pattern to which all must, with some variations, conform. Landau insists that the point of this exercise is not to demonstrate that the views of different theorists can be made to fit a common pattern. It is the variations in the

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[34] Herrnstein-Smith, "Narrative Versions, Narrative Theories" (1981); Misia Landau points out that this position is contentious.

[35] The stages are arboreality terrestriality, encephalization and civilization; their "meanings" are not relevant for the purposes of this discussion.

way each fits that is informative. She argues that "it is by examining in what way each theory deviates from the common model that a structural analysis may be most fruitful."[36]

When judged from the perspective of an awareness of the need for alternative hypotheses, Landau's approach is too procrustean. Once deep structure is identified, elements of an explanation that do not fit the pattern are cut out of consideration. A disadvantage of the structuralist approach, then, is that it allows little room for innovative explanatory structures to gain a foothold. Nevertheless, Landau's project, and others like it, are indispensible to feminist narrators. It is only by recognizing the existence and power of prevailing narrative structures that we can find out which views need to be challenged, and what would constitute a genuine alternative to those views.

Some writers have already set out to make room for views that challenge traditional patterns. Robert O'Hara, for example, identifies the narratives of evolutionary theory as factors which promote its linearity.[37] He objects to narrative because it imposes limits on creativity, and because it does not reflect the "true nature" of our evolutionary past, which is not linear but branched. One might well think it odd to claim that

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[36] Landau (1984) p. 266

[37] O'Hara, "Telling the Tree" (1992)

narrative limits creativity. It seems more likely that one would find in story-telling too great a role for unconstrained imaginativeness of a sort that threatens the precision of scientific explanation. This fear seems to motivate the need to de-emphasize the importance of stories, discussed above. I think O'Hara is correct to observe that originality has been stifled in evolutionary explanation, though he is wrong to blame it on the presence of narrative, or even of linear narrative. His insistence that we can only understand the true nature of our evolutionary past by thinking and telling our stories in terms of "trees" (i.e., multiply branching histories) instead of linear narratives, is not compelling. But read in conjunction with Landau's observations about the deep structure of evolutionary narratives, we can see that O'Hara's worry about creativity makes some sense. If compatibility with prevailing patterns of explanation is one of the criteria for acceptability for sociobiology and other evolutionary theories, then the role of creativity in narrative will be restricted to mundane variables like the order in which crucial events occurred.[38] However, from the fact that a common deep structure can be uncovered for the stories in question, it

[38] Perhaps it is unfair to use the word "mundane" to refer to these variables, since order and timing of events are frequently the subject of controversy among evolutionists. My point is that widespread acceptance of the importance of such parameters limits the potential for considering, or even imagining, new categories.

does not follow that correspondence to such structure is (or is in part) what makes those stories acceptable. That argument must be made independently.

One way to launch such an argument is to examine whether there are evolutionary stories that do not adhere to a common structure, and if so, to look at how they are received. Elaine Morgan's response to the shortcomings of androcentric evolutionary narratives combines elements of the gynocentric strategy with the idea that genuine alternatives will be those that depart from the usual structures in significant ways.[39] Her story differs from those commonly told in one central respect. Morgan's proto-humans were not arboreal creatures who moved to the savannahs in search of new sources of food, where many significant changes in physiology and social organization were then facilitated by natural selection. Her early hominids lived in lakes and oceans.[40]

The interesting thing about Morgan's work is that she does not stray very far from the prevailing patterns of evolutionary story-telling. Most of the things that she seeks to explain about modern humans are things that most

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[39] Morgan, *The Descent of Woman* (1972), *The Aquatic Ape* (1982), and *The Scars of Evolution* (1990). I do not wish to suggest that Morgan's departure from accepted deep structures was intentional, although it is clear that she wishes to offer fundamental challenges to our understanding of human evolution.

[40] Morgan's inspiration is found in Sir Alister Hardy's work, first published in about 1960.

evolutionists and many sociobiologists try to explain: speech, diet, lack of natural defensive weapons, relative hairlessness, tool use, fat distribution, bipedalism, and reproductive factors such as sexual positions, concealed ovulation, d female receptivity and orgasm. Thus we might expect the tions her narrative exhibits to be taken pretty seriously by those who share her explanatory goals.

Morgan asks us to consider the idea that our ancestors evolved their many unique features in response to environmental pressures they faced as shallows-dwellers, living primarily in water at least chest-deep, and in wet caves along the shore. I do not wish to indulge in a detailed assessment of Morgan's theory, but I will give a cross-section of her motives and results. She argues that the existing explanations for the features listed above are inadequate. For example, the upright posture that is supposed to have been selected as better adapted to the rapid locomotion required for hunting is in fact a disadvantage. Bipedalism is inefficient.[41] Similarly, body hair is supposed to have diminished in our evolutionary precursors because the males of the tribe would have overheated running around hunting on the hot African plains.[42] But this does not explain why females would have grown less hairy, and the assumption of hot weather may not

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[41] ibid. (1972) p. 7

[42] ibid. pp. 9-10

match up to the actual climactic conditions. Morgan's work contains challenge after challenge to the flimsy efforts of conventional evolutionists, some of which have been brought up by other theorists as well.

Morgan does not just tear down the existing body of evolutionary explanation. Her hypothesis that our predecessors spent a substantial span of time in the water was not chosen just so that she could be iconoclastic. She argues that it explains many facts of human physiology better than the going alternatives, and that it also explains other features currently regarded as mysterious or not attended to at all by other theories. Climactic and botanical changes, together with an increase in predators and a decrease in readily available food, is alleged to have sent some protohominids scurrying into the water, where mammalian predators supposedly would not follow. Once there, they stayed. Food was abundant, slow moving, and easy to catch. Bipedalism enabled such creatures to stand with their heads above water deep enough to provide food and dissuade predators. Our hairlessness may seem odd in comparison to the furry coats of other primates, but not when compared to aquatic mammals, which have some hair but not much, and which grows in the same distribution and direction as the coats of human newborns. Excessive hair slows down movement in water. Ask any competitive swimmer.[43] Where most primate babies are scrawny, human babies are chubby. Morgan says that this too is a result of our aquatic evolution. A baby with plentiful subcutaneous fat would have added buoyancy, which would enhance its ability to survive in the water. Extra fat is also good for keeping aquatic animals warm. The story of our aquatic origins can also be used to explain male pattern baldness, the shape of the female breast, tears, copulatory preferences, and so on.[44]

Has Morgan's proposal been admitted to the pantheon of evolutionary and sociobiological explanations worth exploring? No. Morgan herself calls it "heresy",[45] recognizing the strength of the resistance of the scientific community to new and unconventional ideas. Perhaps the scientific community thinks views like Morgan's are "crack-pot", and that is why they are resisted.[46] This defense begs the question. As I have already said, her story ranges over much the same ground as mainstream theories. It is told in a reasoned, ordinary manner, using existing

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[43] Morgan (1982) pp. 148-149

[44] I am not claiming that such explanations are always superior. In many instances, they are at least questionable, but no more so than competing explanations offered by sociobiologists.

[45] Morgan (1990) p. 1

[46] One colleague of mine who professes an amateur interest in "crack-pot" theories puts Morgan's views in the same category as sasquatch sightings.

terminology in familiar ways. The accounts she offers of specific physiological features are no more ridiculous than some of those proposed by other writers.[47] There seems little ground on which to base the accusation of absurdity, unless the accuser is willing to acknowledge that much of accepted theory is equally absurd.

One could complain that Morgan's story is unacceptable to mainstream scientists because it is unsupported by evidence. Her views are largely overlooked in the scientific mainstream, but when they are attended to, this is the sort of criticism they typically face.[48] Morgan's theory "has received no support from fossil finds, and it remains a theory based on gaps in knowledge", writes one critic.[49] Again, however, this objection begs the question. As discussed above, much of what passes for evolutionary explanation is unsupported by fossil or other evidence, especially in sociobiology.[50] As Morgan herself

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[47] Desmond Morris' explanation for the shape of the female breast is a particularly striking example here, as discussed in Morgan (1982)

[48] *The Descent of Woman* was a best-seller, but this is probably attributable more to its aggressively pro-woman tone at a time when such sentiments were extremely popular than to its scientific appeal.

[49] Andrew Hill (1984), review essay of Morgan (1982), Gribbin and Cherfas (1982), Makepeace Tanner (1981) and Eldredge and Tattersall (1982), p. 189

[50] To his credit, Hill does not single Morgan out in this way in the review essay cited above (fn.48). He recognizes that "hypotheses and stories of human evolution frequently arise unprompted by data", and "the data which do exist are

points out, the Pliocene era, in which all of the interesting changes leading to the emergence of our ancestors were supposed to have taken place, left behind a dearth of fossil remains.[51] This five-million year gap in the fossil record "means that until more fossils come to light *all* hypotheses must remain speculative".[52] Yet many hypotheses are not so regarded. At least Morgan can offer a plausible explanation of the l k of fossil evidence for her story: the remains of our aquatic forebears, including any tools they may have fashioned for catching and eating fish and shellfish, washed out to sea. Critics of the aquatic ape hypothesis have argued that if our earliest ancestors were shallows-dwellers, we ought to be able to find shell middens, refuse heaps of discarded shells and bones, in coastal caves. Morgan's response is that she sees no reason to accept that shell middens would be relevant to her case, because other aquatic mammals do not leave them, and other primates do not collect food and return with it to a base or lair.[53] They eat where they find food. However, she

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often insufficient to falsify or even substantiate them". (p. 249) He actually says that Morgan's book is the most balanced and objective of the four he reviews, but finds it unconvincing all the same. Similarly, Stephen Jay Gould calls Morgan's speculative reconstruction "as farcical as more famous tall tales by and for men", but he does not comment on why the men's tales are more famous.

[51] Morgan (1982) p. 6

[52] ibid. p. 121

[53] ibid. p. 117

points out, when shell middens were discovered in coastal caves, those critics who had insisted on their importance suddenly changed their minds.

There are two things going on in the reception of Morgan's renegade narrative. On the one hand, she challenges the existing deep structure of evolutionary story-telling, although apparently it is a fairly mild challenge. The fact that her efforts are greeted with indifference or rejection suggests that there are very strong pressures to conform to those structures. On the other hand, her story is deliberately gender-sensitive. She states quite explicitly throughout much of her work that she rejects the androcentrism of mainstream evolutionary explanations, and intends to dislodge this bias by retelling the story from the female point of view. This suggests that her views are being rejected because they are gynocentric. What I think we should conclude from this is not that it is therefore androcentric gender ideology and not deep structure that makes narratives acceptable or unacceptable. I think that Morgan's narrative shows that androcentrism is *part* of the deep structure of the stories in question. Thus alternative points of view in science need not diverge substantially when taken from the point of view of scientific orthodoxy. In fact, as the example of Morgan suggests, very small departures may give rise to large, almost revolutionary, changes in awareness. As we have seen, the feminist project in science sometimes focuses only on the gender bias which some critics argue lies at the foundation of much scientific theorizing. Awareness of narrative bids us to look at the way in which gendered interests and experiences, taken as part of the deep structure of scientific narrative, play a role in explanation.

II. Finding the Gaps

There is another aspect of the role of narrative in evolutionary and especially sociobiological explanations which must be considered. In the previous section, I described Landau's structuralist approach to human evolution, and accepted it as an appropriate way to uncover interesting things about the stories that evolutionists tell. I also emphasized the relative absence of evidence for evolutionists as a factor that seems to encourage, and perhaps even necessitate, their reliance on story-telling. In the present section, I will place these two situations in tension, and will argue that in light of the lack of strong evidential constraints, the presence of common narrative patterns is grounds for suspicion. Feminists can use this tension to challenge those patterns. Recent work on the role of metaphor in science is of particular interest in this regard.

Part of the feminist critique of science has consisted of objections to the sexism of the metaphors in which science and scientific method is couched. Some defenders of science have been inclined toward the naive response that the solution is to cleanse science of its offensive metaphors. This response tries to minimize the problem by making it merely one of language, and easily rectifiable. However, many contemporary philosophers recognize that metaphoric language is indispensable to the scientific enterprise, even those who are otherwise unconvinced by the feminist critique. Such theorists hold that metaphors and analogies "are not just psychological aids to scientific discovery, or heuristic devices, but constituent elements of scientific theory."[54] As Keller and others insist, every metaphor opens up new avenues of consideration at the expense of closing others. Therefore, it is important for feminist critiques of science to pay attention not just to the limitations imposed by metaphor, as in the case of rape and domination, but also to the new explanatory possibilities that metaphoric languag can (and should) disclose. This awareness is nascent in the work of Nancy Leys Stepan.

A common feminist response to metaphorical language is to object to the way that choices of vocabulary by Bacon and others determined a masculinist path for modern science.

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[54] Stepan, "Race and Gender: The Role of Analogy in Science" (1986) p. 262. Stepan gives a brief overview of current thinking on metaphor and analogy in science, with bibliographical references.

Although this view is not without problems (addressed in the first chapter of this essay) it has a core of plausibility. What lends it this credibility is the tacit, largely unexplored premise that metaphoric language often has great power to channel our thinking in new ways, or to cement it in place. If this were not the case, it is as unlikely that Bacon or Newton would have bothered with such language as it is that their audience would have found their methodological proposals compelling. Stepan points to the complexity of understanding metaphoric usage in science. The variables are many, and it will often be difficult to assess where the influence of any variable predominates. Choice of metaphors for scientific application is constrained by the nature of the object studied, the social structure of the scientific community in which the study takes place, and the history of the discipline.[55]

Sometimes, Stepan argues, metaphors extend existing ways of thinking in new directions, or into new areas. Metaphor can also act to "neglect or even suppress information about human experience of the world that does not fit the similarity implied by the metaphor".[56] Both elements can be identified in the extension of metaphors of masculinity into the new area of the discourse of scientific

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[55] ibid. p. 265; Stepan relies on Stephen Toulmin's views here.

[56] ibid. p. 272

knowledge. But metaphors also open opportunities to think in different ways, to see things not seen before. They can literally create new knowledge.[57] Stepan considers that what might make analogies and metaphors most suitable for use in science is that they suggest new systems of implication, new hypotheses, and therefore new observations.[58] Of course we cannot predict in advance all of the similarities that will be brought into play by our choice of a particular analogy. It may take a long time to discover all of the information that can be yielded through the use of a new metaphor. Even with the best intentions, it will always be possible that some of our language choices will have undesired consequences. I see no way to avoid this problem. But I think that Stepan's insight, and her emphasis on the role of language in uncovering or provoking an awareness of unexplored forms of knowledge, is crucial. Although the language of sociobiology is not necessarily metaphorical, Stepan's point about metaphor applies equally well to the deep structure of sociobiology's stories as Landau conceives them. It is precisely the failure of sociobiologists to say anything new, in a context where evidential constraints are so loose as to permit them to say almost anything they want, that is so very unsettling.

Donna Haraway shares a similar awareness of the

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[57] ibid. p. 271

[58] ibid. p. 268

revolutionary potential of extending common patterns of thought in new ways, for both the improvement of science and the empowerment of women. She argues that "rhetorical strategies, the contest to set the terms of speech, are at the centre of feminist struggles in natural science".[59] Women's narrative practice can thus be part of the struggle both to play a role in the production of facts about women, and to seize a scientifically authoritative voice for women. Story-telling is embedded in social systems of beliefs and practices, and there are rules for narrative, including scientific narrative. By participating in the telling of the stories of science, exhorts Haraway, women participate in the ongoing operation of making and remaking those rules, and assist in demystifying the process of science. She specifically targets those critics of evolutionary biological story-telling who reject the anthropomorphic inclinations of such narratives. Her argument is similar in spirit to mine when she insists that "forbidding comparative stories about people and animals would impoverish public discourse", and would be an unenforceable rule in any case.[60] Women's efforts to tell self-conscious but still anthropomorphizing tales in primate anthropology have served to alter our understanding of some primate behaviours. As a result of such efforts, some women now have the social

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[59] Haraway (1991) p. 72

[60] ibid. p. 106

authority to tell new scientific stories.[61] The potential of narrative must not be quelled by those who find certain stories unsettling, but as Helen Longino insists, there must be mechanisms present in the social structure of science that permit those who are unsettled to respond to those stories and to have their responses taken seriously by the scientific community.

What is objectionable about the narratives of sociobiology (and other evolutionary theories), seen from perspectives like Stepan's and Haraway's, is not that they are narratives, or that they use excessively metaphoric language, or even that they exhibit certain structure. The grounds for feminist disapproval is the lack of imagination that those superficially florid narratives conceal.[62] The stories of sociobiology are dominated by an incredibly limited set of narrative patterns or icons. The paucity of variation in sociobiological tales is breathtaking given the creative prospects they could enjoy. As the example of Morgan's narrative challenge illustrates, the masculinist icon is central in limiting those prospects. The prevailing tone of sociobiological stories is that of validation of the status quo. "There, there", the sociobiologist seems to

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[61] ibid. p. 106

[62] I do not wish to suggest that the problem is aesthetic, that the stories are just boring. On the contrary, the narrative style of many sociobiologists, especially E.O. Wilson, is quite captivating.

murmur. "All of those unsettling social and political movements designed to alter the warm, safe, lives we (privileged white males) lead will come to nothing. It is not in our nature." Note that the discourse of domination to which some feminists have drawn our attention is absent in this solace. Sociobiologists seem instead to be conceding that nature dominates us and that we are powerless against it. It is as if sociobiology is using the trendy, prestigious language of evolution, genetics, and other fields to reinforce the decidedly unfashionable Leibnizian saw that all is for the best in the best of all possible worlds. The opportunity is ripe for a feminist *Candide*.

It is my proposal that a disruptive feminist strategy would be to embrace both the importance and the function of narrative explanation. That is, feminists might wish to accept the fact that it is narrative that does the explaining in many contexts in order to offer new narrative explanations that are more consonant with the experience of women. As we have seen in earlier chapters, the need to get alternative points of view, and the theoretical positions they may generate, on the scientific agenda has been identified as a primary goal for the feminist project in science. This goal is shared by theorists whose positions are otherwise quite different. As Harding puts it, "collective delusions can be undone by introducing fresh perspectives".[63] Using narrative to accomplish this goal has several advantages.

Strongest among these, in my view, is the power of narrative to inform and incite. Recall Hilary Rose's remarks, cited above, that the literary genre is more capable of generating descriptions with which people can identify, or against which they may want to rebel. This capability arises from the nature of narrative itself, from what, historically, stories (and poems, epics, etc.) have been for: the rehearsal of memories, the forming of expectations, and the summoning of the tribe to action.[64] The reassuring tone of sociobiological narratives elicits the identification of a particular sort of listener, while the approach for which I am arguing has as its goal the provocation of new listeners and new story-tellers with perspectives and talents as yet untapped. Although many women (and some men) find sociobiological stories irritating enough to incite rebellion, their disagreement often undervalues this aspect of narrative's power. Taking narrative seriously must entail more than merely embracing story-telling as a new and possibly fruitful way of presenting alternative evidence and hypotheses. Narrative

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[63] Harding, *WS?WK?* p. 62, quoting Millman and Kanter (1975)

[64] On poetry as "bardic practice", i.e., as a social and political force in which the values of the community are negotiated and promulgated, see Terrence Des Pres, *Praises and Dispraises: Poetry and Politics, the 20th Century* (1988)

is not just a means to expand the discursive toolbox of science. Once we recognize both science's reliance on narrative and the hidden capacities of stories and storytellers, narrative can become a potent instrument for confronting the content of androcentric evolutionary explanations, and for disputing the forms and protocols of such explanations.

Literary theorists acknowledge that the role of narrator is one which enjoys interpretive authority.[65] As Haraway's remarks, above, indicate, this authority can be captured just by entering the fray of sociobiological and evolutionary narrative. Old fears and stereotypes, as well as tacit and explicit barriers that keep women from science, do not magically vanish on this approach, but they can be diminished, especially when one becomes aware of the ways in which women's stories are already contesting and changing accepted scientific explanations.[66]

Haraway stresses that emphasis on scientific storytelling serves to maximize inclusivity. She is somewhat troubled by feminist arguments that seek to establish the historical construction of science as masculine, and as excluding women. She worries that the net effect of such arguments may be to provide women with excuses to remain

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[65] Bruno Bettelheim, e.g., in *The Uses of Enchantment* (1976)

[66] Haraway gives a detailed description of the impact of such stories in "Daughters of Man the Hunter" (1991)

outside of and irrelevant to science.[67] But narrative imposes few limitations on participation. It is "potentially open to ordinary women 'in' and 'out' of science", writes Haraway.[68] One could even assert that story-telling enjoys a connection to certain feminine traditions and practices, such as oral histories and bed-time stories for children. My point here is not, however, that there is anything uniquely feminine about narrative, but that women may bring unique and valuable perspectives to the narrative dialogue, and what is more, that they will often require no special scientific expertise to do so.

Some individuals may find narrative a more comprehensible and hence more persuasive form of discourse than dry, abstract, often mathematized scientific explanations. Such explanations seem particularly out of place in the human sciences, which may be one reason why narrative explanations flourish there. One of the advantages of the story-telling approach is that it includes the notion of *self*-persuasion, of a narrator articulating her tentative ideas, along with worries, aspirations, and values, in such a way that we can observe the unfolding of

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[67] Haraway, "Situated Knowledges" (1991)

[68] Haraway, "Daughters of Man the Hunter" (1991) pp. 106-107. Excessive emphasis on textuality, of the sort found in deconstructive approaches, is exclusionary for those who cannot read or write. Focus on narrative need not imply such exclusion, as it can (and must) accommodate oral traditions, folk psychologies, and so on.

her beliefs, while learning about her goals and ideals. Narrative, especially when spoken, also lends itself more immediately and more obviously to dialogic response, so that the interruptions and questionings of others become an integral part of that unfolding. The individual and community proceed together in this form of knowledge-seeking. Another advantage is that narrative can be more flexible, more open to interpretation and response than more scholarly accounts, making it somewhat easier to accommodate changes in social context and belief systems. Narrative can lack finality, making it continually possible to modify and renew stories, and to maintain the sort of vigilance that feminist criticism requires.

I think it is important at this stage to quash a very obvious objection. It may appear as though I am merely giving feminist critics of science, and women in general, an elaborate permission to make up stories. But I am not claiming that anything goes. I am saying that given the preponderance of already existing androcentric narratives in sociobiology, and the valuable instrument of structural study of such narratives, feminist story-telling can be tailored to exploit the shortcomings that a gender-sensitive reading of existing stories reveals. Anything *with a motive* goes. But I may then be faced with a further problem, namely that stories told with an eye to revealing and redressing the inadequacies of male-biased narratives may not be true. I do not find this objection persuasive. Other philosophers and historians have on occasion suggested that "it is legitimate to use *fictionalized* history of science to illustrate one's pronouncements on scientific method".[69] One might think that an analogous case can be made for using fictionalized natural history to illustrate one's pronouncements on the experiences of women in contemporary gender-stratified society. The obvious challenge to be made here is that the two situations are too disanalogous for the case to be made. It does not matter if the history of science did not really unfold in that way, the argument would go. It could have happened like that, and our views about scientific method would have come out much the same. But one might say that the same is true for evolutionary explanations, especially since the evidence that might establish the "true" course of natural history is in principle unavailable. The *tu quoque* justification is ultimately unsatisfying, however, and may license the construction of specious histories that are damaging to women, or to our best possible understanding of science.

A more convincing defense of motivated narrative may be found in its potential for inclusivity. If part of the problem with androcentric stories is that they fail to ring

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[69] Stephen G. Brush, "Should the History of Science Be Rated X?" (1974) p. 183 (emphasis in original); Brush cites J.J.C. Smart and Herbert Feigl as two philosophers who find such license acceptable.

true to those who are socially situated differently from those who tell the stories, then the best stories will be those that ring true to the greatest number of alternatively situated individuals. This does not make the best stories *true*. But it does not make them false, either. Since the evidence imposes such weak constraints on stories in this context, a more explicitly social goal, namely input from multiple perspectives, is appropriate.

This assertion implies other problems. The most rigorous of scientists is very likely to complain that widespread social acceptability is not an objective criterion for scientific knowledge. We do not ask everyone how they feel about gravity as part of the task of setting out to explain it. But this complaint misconstrues my position. I am not arguing that we need to ask everyone how they "feel" about the problems and solutions of sociobiology and evolutionary theory. I am saying that insight from diverse sources can both uncover bias in existing explanations, and inform new theoretical choices. Furthermore, we can and should respond to the precision-minded scientist that social acceptability at some level is already part of the objective criteria for scientific knowledge. That is what the argument for the double-aspect approach to knowledge is about. We may not see the influence of group standards in many cases because of the homogeneity of the group, and because of artificial barriers between scientific and non-scientific communities. Narrative is an attempt to bridge this gap.

Gravity is an obvious but hackneyed example in this context, yet it pops up in a great many discussions of this and related issues. Those inclined to use such examples as trump cards in their rejection of the need for inclusive science must acknowledge that much scientific work deals with objects and phenomena very unlike gravity. In many cases, part of the ongoing work of refining a hypothesis includes negotiating what the hypothesis is really about. As Richard Burian has argued, when scientists are immersed in the heat of a live controversy, especially in biology, they often cannot draw sharp boundaries between ideological, political, and more narrowly scientific issues, as they perhaps could about more straightforward and well-established concepts.[70] Often, the greater scientific merit of any one interpretation will not be available as a basis on which to draw one's conclusions. In the midst of the fray, different theoretical options may be equally meritorious, or equally lacking in merit. The rigorous scientist may want to convince us that in such cases one bides one's time until the evidence is in, but we have already seen that this does not reflect actual scientific practice. One gathers and interprets evidence with certain assumptions already in mind. And in the case of

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[70] Burian (1986) p. 24

evolutionary histories, again, no amount of patience will be rewarded.

Burian's proposal is that we should therefore allow "political" considerations to influence our judgement in science. Among such considerations, he includes the competence and reliability of the theorists whose positions are in competition, the strength and suitability of the disciplines on which they draw, the appropriateness of the techniques they employ, and the fit of their theories with orthodoxy.[71] Critics of sociobiology have commonly questioned the competence and reliability of those theorists and the strength and suitability of the disciplines on which they draw. Feminist subversives will have to answer similar challenges. I have already defended the appropriateness of the narrative technique sociobiologists employ, because I want feminists to be able to use it as well. A key difference between my proposal and Burian's, then, revolves around the question of orthodoxy.

On the face of it, Burian's political considerations do not appear all that political, unless by "political" he means "conservative". But my challenge to his view is not about whether or not fit with scientific orthodoxy is important, but about how it is important. There will probably always be some narratives of human physiological and social origins that are just too outlandish. Stories

[71] ibid. pp. 37-38

that contradict well-grounded beliefs about the age of the universe, the physical structure of matter, the biochemical composition of life, and so on, are not motivated by significant gaps in narrative understanding, as I have claimed such stories should be. Along one dimension, the stories that I a advocating do cohere with many more accepted tales. They try to explain the same sorts of things, they follow the same general rules of logic and sequence, and so on. But along another dimension, feminist stories use fit with orthodoxy in a negative sense, to disrupt conventional wisdom. It is this destabilizing function to which feminist narrative must continually return.

A further worry remains, however. Scientists may still object that I am asking narrative to play a role that scientific explanations are not supposed to play. This talk of disruption and rebellion is alien to the ideal of pure, objective science, untainted by any political or social agenda. I contend that the work of feminist and other science critics shows that this ideal is and has been a rhetorical smokescreen for the value-ladenness of science, particularly when applied to science in the midst of live controversy. And as feminist empiricists argue, the merit of acknowledging that social values play a role in science is that we can at least start to look at them from within science itself, and criticize, refine, and alter them according to what such examination reveals about them.

III. What Kind of Stories?

In light of the argument I have made for the relevance and even revolutionary function of feminist story-telling, I think it is important to say something about what kind of stories adhere most closely to the strategy I have outlined. I will offer an example of my own, and discuss another, more detailed example of what I would call disruptive narrative. It must be acknowledged that participation in narrative is not simply a matter of standing up and speaking out. Although narrative has the potential to be more inclusive and empowering for women, story-telling also requires a certain degree of articulateness, some self-confidence, and above all, an audience. I take it that the first two of these requirements are fairly minimal, and can be taught. The need for an attentive audience is more problematic, and I will address it in the final section of this chapter, and in the conclusion of this essay.

One need not have a structural analysis as formal as Landau's in order to firm up one's intuitions about where the obvious gaps lie in established sociobiological and evolutionary narratives, although it certainly helps. Part of the argument for the need of fresh perspectives in science is the claim that readers who come to these narratives with alternative points of view will be struck by certain aspects that may seem quite ordinary to those immersed in the field. This is not a question of "seeing" or "knowing" differently, as some would have it, but of attending to different properties of proposed explanations, and drawing them into question. When one sees the same idea appearing over and over again, and especially when it strikes one as particularly implausible, then that is the place from which a new narrative may be launched. For example, bright plumage or other decorative bodily accoutrements in animals have been explained as aids to sexual attractiveness. The lovelier or more noticeable the body, the more likely it will be to attract a mate, hence ensuring the endurance of the animal's genes. Whether or not this explanation is a plausible one is not at issue. It is its extension into the human realm that I wish to query. Some sociobiologists have argued that the physiological traits of women have been selected for under similar pressures which faced our protohominid ancestors.[72]

Sociobiologists claim that the female breast has a certain shape because that shape was attractive to early men, who thus became more willing mates. The hips have a certain structure, giving rise to a pleasing sashay, because that way of walking was attractive to men, who followed it into the primitive boudoir and enabled the pleasingly shaped woman to maximize her genetic potential. Sociobiologists do

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[72] See Morgan's (1972) discussions of Desmond Moore, Lionel Tiger, Robert Ardrey, and others.

not, as far as I know, offer similarly detailed explanations for the male physique. This is especially ironic since sexual selection is almost always applied to male traits, such as the peacock's showy tail.[73] Sociobiologists do explain obvious general traits of the male form, such as its greater average size and strength, by reference to the reproductive appeal of such characteristics. A bigger, stronger mate is a better protector and provider, so women will be likely to seek such traits in their sexual partners. But the size, shape, and attractiveness of specific male body parts is not addressed. This silence cries out for comment, both in order to explain why it exists, and as a starting point from which to tell new stories that challenge the adequacy of the old ones.

Sociobiologists are committed to explaining everything in terms of natural selection, and they favour the evolutionary concept of sexual attractiveness when it comes to explaining the physiological minutiae of women, so why does sexual attractiveness not appear in their explanation of the particular physical form of men? If they favour an explanation that does not draw on sexual attraction, what motivates this inconsistency? And what other sorts of stories might we tell about human physiology, male and female, that relies on other concepts altogether? Women's perspectives on such questions may well make very different

[73] Mohan Matthen pointed this irony out to me.

assumptions about what is to be explained. As Elaine Morgan points out, the evolution of hip and breast shape is more likely to be explained by reference to ease of childbirth and nursing than sexual attractiveness.[74] And sexual attractiveness may strike many women as a ridiculous sort of "explanation" in any case, for it assumes that early females had this challenge placed before them - get early man in the sack - and only those who were shaped a certain way succeeded. One might well suggest that a story more consonant with the experience of women would be one that put aside the idea of sexual attraction, and looked to other forms of animal behaviour as adaptations which freed females from the constant sexual demands of males. I suggest that stories which start from the assumption of the evolutionary importance of sexual attraction might give way to stories which assume that sexual contact always take place, and which draw on categories including (but not limited to) pleasure-seeking behaviour instead of genetic investment strategies.

The kind of new narrative that I have in mind may be found in either the scientific or the literary context. Both will share the power to inform new ways of thinking. Scientific narratives may appeal to different audiences than literary ones, and there is already a large body of scholarship demonstrating the interconnections between the

[74] Morgan (1982)

two audiences. This is hardly surprising, once we recognize that the social embeddedness of science includes its influence by and on literature and more commonplace narratives. I wish to look at an example of a new fictional narrative which I think fits the strategy I am defending.

The fictional narrative that I have chosen as exemplary is Sherri S. Tepper's 1988 science fiction novel, *The Gate To Women's Country*. The novel is set in a post-nuclear future. A number of walled cities have been built away from radioactive burns, and these are occupied by women and children under the age of five, plus a small number of males over the age of fifteen, who form a class known as Servitors. Each city in Women's Country is protected by a garrison, which lies outside the city walls, and which houses male children between the ages of five and fifteen, and the Warrior class. The process by which garrisons are populated and through which some men become Warriors while others become Servitors involves various ceremonies. Twice a year, the men of the garrisons come into Women's Country in order to drink, carouse, and enjoy sexual assignations with the young women of the cities. Women are subject to medical exams before and after carnival, in order to combat the spread of sexually transmitted disease.

All women who give birth to sons may keep their boys at home until the boys are five years old. Sons are then taken to a special passageway between the city wall and the garrison and presented formally to the men. Mothers do not see their sons for ten years, while they are trained in the ways of the garrison, learning about military strategy, arms, gamesmanship, sex, and above all, honour. Although they are free to choose whether they will become masculine, respected, indispensable Warriors, or soft, feminine, useless Servitors, the training they receive in the garrisons tends to favour the Warrior's point of view.

At age fifteen, some young men elect to return to Women's Country through the gate built especially for that purpose. It is regarded as a journey of shame and cowardice by the Warriors, who spit at the young men who choose to return, throw rocks at them, strip them of their clothing, and often beat them. Many garrison men voice their disgust that the number of young men returning to Women's Country seems to be on the increase. The young men who choose to stay in the garrison go through a ritual of insulting and disowning their mothers. Finally, at age twenty-five, they become true Warriors, sworn to defend their city and "their" women and children in battle. There are delicate trade balances and other treaties and agreements between the cities of Women's Country, as well as occasional insurrections, which periodically require the Warriors to take up arms. Wars are fought with spear and knife, and are very bloody with a very high rate of fatality. The manly Warriors must bury their own dead and care for their own wounded, with no help from the medically more skilled women, in order that they can bear witness to the horror of war.

The women's cities are governed by councils which, on the surface, seem to occupy themselves with mundane tasks. But there are also secrets in the highest echelons of Women's Country. This theme pervades the novel. Garrison men frequently voice their belief that the women of the cities know things, mysterious, powerful things which are deliberately withheld from the men. Their notions of these secrets are very vague, but it is believed that the women may have some concealed knowledge of weapons and even magic.

The strategy favoured by one garrison for discovering the women's secrets is to befriend and romance Councilwomen and their daughters. One such daughter, Stavia, is the assignment of a young Warrior's son, Chernon. The two arrange clandestine meetings near the city wall where Chernon begs Stavia to bring him books. The garrison libraries contain only works of romance, battle tales, and practical knowledge for soldiers. In Women's Country, books are used for women's studies: medicine, engineering, management, metallurgy, animal husbandry. It is forbidden to give Women's Country books to a Warrior, but Stavia does so.

Over time, Stavia brings Chernon a number of books, but the secrets Chernon seeks are not in them. Chernon demands more books, frequently complaining that Stavia is not bringing the "right" ones. Eventually, he gives up:

> "For a time he had thought he might find secrets in the books Stavia had given him, but there were no mysteries there. Just numbers and names for things and stories about how people had lived long ago - not even powerful people, just ordinary shepherds and weavers and people who grew crops. They might have had reindeer instead of sheep or cotton instead of wool, but there was nothing useful in that. No mysterious knowledge. Nothing about the wonderful weapons. Nothing of the stuff he knew had to be there, somewhere. Stavia hadn't given him the right books. Probably those books, the powerful books, were secret. Perhaps Stavia herself hadn't even seen the secret books yet."[75]

In the end, more direct and more violent intervention is planned by the men. However, as we learn at the conclusion of the novel, the secrets the men sought were in those ordinary books all along.

Stavia's mother, a councilwoman, ultimately admits to Stavia that the secrets in question are not all that subterranean. The Council has placed clues here and there, for those with the wits to see them, she confesses. And indeed one old man, an itinerant magician, figures the secret out for us. "Remarkable how many books in Women's Country refer to selection", comments this fellow.[76] Stavia's mother berates her that she had the proper information all along, but failed to understand it. She had learned in school how the Laplanders selected the docile reindeer bulls for breeding, because they were easier to herd. After the planetary devastation wrought by men,

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[75] Tepper (1988) p. 148

[76] ibid. p. 288

Women's Country determined to alter the evolutionary pattern of humanity by establishing a similar breeding program for men.

All at once, the meaning of the rituals of Women's Country becomes obvious. The medical attention that women receive prior to carnival is partly to prevent transmission of disease, but it also includes contraceptive implants to prevent impregnation by Warrior men. The drunkenness and spirit of sexual freedom that prevails at carnival is designed to keep each Warrior unsure about exactly which woman he has slept with. But no Warrior ever fathers sons, nor any children at all. All of the babies born in Women's Country are conceived (mostly through artificial insemination) with Servitors: men who have rejected the violent, misogynist, dangerous world of the garrisons. And the breeding program is working. The perception that more young men are returning to Women's Country through the gate of shame is correct. The Councils of Women's Country have found a way to increase the numbers of the kind of men with whom they wish to live and work. Even the periodic wars turn out to be manipulated by the Councils in order to keep the numbers of garrison men manageable, and to ensure that the women always outnumber the men by a substantial margin.

Tepper's use of the theme of secrets works in tandem with her repeated emphasis on the claim that powerful knowledge is readily available to those who take the time and have the sense to look, to comprehend what they learn, and to act on it. "It's all in books!", the young Stavia exclaims, although she is as yet unaware of the immense truth of her observation.[77] Tepper's didactic technique is so subtle that one may miss the impact of her narrative if, like the young Stavia, one does not recognize its significance to the present situation of ordinary women. The knowledge women need to change their world *is* all in books, and in other narrative forms, including books like Tepper's, and including the stories of sociobiologists.

Tepper's novel could be taken as a blueprint, a clever advocacy of feminist natural selection. I do not read her that way. I think Women's Country is important because it evokes new ways of thinking about the old stories, and suggests new answers to our questions about how things might have been. The narrative is by no means a flawless one, sliding at times into an uncritical biological determinism. But it also recognizes that the forces of evolution have been at work on both men and women, and that the intimate relationships of women with their sons, brothers and lovers does not erase or prevent women's oppression by men.

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[77] ibid. p. 78

IV. Narrative as a General Strategy for Science

Alasdair MacIntyre has argued that "dramatic narrative is the crucial form for the understanding of human action".[78] It should be obvious that it is not the only form suited to such understanding, but as I have argued above, it enjoys many advantages. MacIntyre extends the point in a fairly obvious way. If we recognize, as we must, that science is a human activity, then we must accept that it is at least possible to understand science itself using a narrative approach.[79] We may try to understand previous philosophical and even feminist accounts of science as variations on certain common structures, as I recommended we do for evolutionary stories.

Philip Kitcher complains of current criticism of science, including feminist criticism, that it is disparaging not of science as it "really" is, but of an inaccurate myth about science.[80] He dismisses much criticism of science as myth-bashing, a variety of straw person argumentation. Kitcher is committed to the view that there is something that would count as getting it right

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[78] MacIntyre, "Epistemological Crises, Dramatic Narrative and the Philosophy of Science" (1977) p. 464

[79] In fact, MacIntyre makes a much stronger claim, that science can only be understood as rational in so far as a narrative exposition of it is viewed as a rational activity.

[80] Kitcher, *The Advancement of Science* (1993), especially the introduction and envoi.

about science, and that if we wish to be critical of science, we should be very careful to get it right. But this position shares in some of the evidential problems that plague evolutionary theorizing. Although it is not the case that the evidence for a general philosophy of science is unavailable in principle, it is surely true that we currently find ourselves lacking an effective, non-question begging means of determining where we should be looking for evidence and when we have enough of it.

At levels both abstract and mundane, our studies of science do not yield uniform general pronouncements. As we saw in the first chapter, an historical perspective on some alleged scientific ideals shows that claims about, for example, the objectivity of science are meaningless in the absence of a temporally, socially situated understanding of that concept. Nor can we appeal (as Kitcher seems to think we can) to practice, to understanding the process of science by studying what it is that scientists do. Such studies reveal that there is often widespread disagreement among scientists about the nature of their work, and there are probably considerable discrepancies between what scientists do and what they believe they are doing.

Yet philosophers, sociologists and historians of science do make general claims about the nature of science, claims which sometimes fail to take the diversity and complexity of the evidence into account in much the same way that sociobiological stories fail to take note of the absence of evidence. An awareness of the deep structure of sociobiological narratives can prompt us to respond with new stories, motivated by the ideas and experiences about which old tales are silent. The structure of philosophical narratives about science, on the other hand, is very close to the surface. These stories are about explanation, confirmation, reality and progress. It is the details of the scientific process that are silenced. Feminist philosophers of science are already exploring those silences and telling new stories, seizing authoritative voices and entering the dialogue in which science itself is being made.

It is of the utmost importance that we acknowledge potential risks that may be posed by a feminist embrace of scientific stories. The narrative approach may hold dangers for women. It is, after all, the unoriginal, reassuring narratives of sociobiology (and many other fields) which have figured centrally in the oppression of women. More worrisome, however, is the fact that the disruptive potential of story-telling for which I have argued is open to all comers. As Haraway points out, stories are hard to restrict.[81] As women find their voices and begin using them to tell previously inadmissible stories, the loose, unrestrained inclusiveness of narrative has also provided a forum for those who would discredit those stories. And the

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[81] Haraway (1991) pp. 105-106

sanction of science (or the academy) can be given to misogynist counter-narratives as easily as it can be granted to the feminist narratives that inspired them. The narrative approach to science may raise anti-feminist and misogynist sentiments to a new level. On the other hand, stories that are presented in the guise of acceptable scientific or philosophical hypothesis may permit women to express ideas, and even emotions, in a way that has seldom been available to them.

The work of many feminist theorists is dismissed as shrill, angry, and filled with hatred, as offensive to men and embarrassing to other women.[82] Such charges are largely unjustified, but any attempt to respond to them throws the debate off course, forcing feminists to defend their work as calm and reasonable, and their anger as appropriate, instead of focusing on the nature of gender oppression and proposals for its elimination. Narrative presentation of ideas can serve to dampen their impact, to make them seem less harmful and insidious. It is this feature of sociobiology which causes its critics such alarm, but again, I think it is a feature which may be used to the advantage of women.

Whether we opt for a narrative or a more formal approach to disrupting science and our philosophical

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[82] Catharine MacKinnon and Sandra Harding have been particularly victimized by such criticisms, especially by Christina Hoff Sommers, Katie Roiphe, and Michael Ruse.

understanding of it, feminists will always be faced with this element of risk.[83] I have argued that story-telling facilitates dialogue with non-specialists more openly and more readily than the exclusionary scholarly approach, while Nelson has argued that we need to meet scientists and philosophers on their own turf, and show them that according to standards that they already accept, they must take the feminist critique of science seriously. Either way, Longino argues, there must be shared common ground between feminists and those they would see convinced of the merit of their tales. But that common ground goes beyond the mere narrative and/or theoretical commitments to which Nelson and I refer.

Richard Bernstein insists that dialogic communication presupposes shared values, emotions, and moral virtues, among which he includes willingness to listen, to understand what is genuinely alien to oneself, and the courage to risk one's most cherished beliefs. Bernstein complains of Rorty's "conversational" approach to philosophy that it attempts to compensate for the absence of a genuine shared

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[83] One might object here that the move to narrative explanations is not always a free choice, but depends upon the nature of the science in question. Clearly those sciences which are already obviously historical (evolution, cosmology, sociobiology) will lend themselves most readily to the narrative approach. It remains possible that other sciences could accommodate narrative components, or that their natures could shift over time, making them more amenable to narrative description. I thank Kathleen Okruhlik for pressing me on this issue.

commonality, by violently imposing a spurious one and projecting a false "we".[84] Feminist efforts may be stymied by just such projection. My position and Nelson's (and Harding's and Longino's) are to some extent predicated on the assumption that feminists can enter into dialogic communication with scientists and other philosophers, that the good will to which Bernstein directs our attention is present in at least some members of our respective audiences. We may be wrong about this. If, as Bernstein claims, "it is a self-deceptive illusion to think that the 'other' can always be heard in a friendly dialogue",[85] then those who are regarded as "other" must be wary of such illusions. Bernstein proposes that what may be required for the mainstream to enter into authentic communication with the marginal is "rupture and break - a *refusal* to accept the common ground laid down by the 'other'".[86] However, from the point of view of women in a gender-stratified society, this, too, entails risks. It is my view that a narrative strategy may be able to maintain a delicate balance between these two sources of danger, because of its flexibility, inclusiveness, and, it is hoped, subtlety.

I have argued in this chapter for a motivated narrative approach to the correction of the misogynist, damaging tales

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[84] Bernstein, "The Rage Against Reason" (1988) p. 205

[85] ibid. p. 206

[86] ibid.

of sociobiology and other evolutionary theories. I have also indicated where the sources of feminist motivation lie. While evolutionary sciences are the easiest to disrupt in this fashion, because of their pre-existing reliance on narrative explanation, it is clear that the strategy can easily be applied in other fields. I think sociobiology is the best place to start simply because it purports to explain modern, gender-laden social arrangements in evolutionary terms, and it is our dissatisfaction with those arrangements that leads us to feminist consciousness in the first place. I have also indicated how this approach might later be extended narratives about science, or at least about some of its constituents, especially its history. How- - spite of my advocacy of the creative and even irit of feminist narrative, I have revealed a mism about the success of this, or any, sophy of science. My final task in this essay ront that pessimism directly.

CONCLUSION

AGAINST THE WAR, BUT SUPPORTING THE TROPES?

I have argued that feminist science criticism ought to include a self-conscious re-reading and re-telling of the central narratives of human nature. I have also provided some grounds for thinking that this approach can be extended to narratives in and about science more generally. In this final passage, I wish to do two things. First, I wish to stress that the first four chapters of this essay are in one sense a detailed object lesson in the use of the strategy advocated in the fifth chapter. In other words, my technique for presenting my argument might be described as a narrative one. I have identified significant ideas and commitments in feminist philosophy of science, and reconsidered them from the point of view of what I take to be the central tasks and goals of feminism. This amounts to the suggestion that these stories could be told another way, and that those other ways may be of considerable benefit to women. In some cases, however, I have left my interrogation of the structure of feminist narratives incomplete. In this concluding chapter I will offer a synthesis of the insights I have presented in order to make some suggestions which may perturb, but which I think feminist philosophers of science must begin to take seriously.

I. Old Metaphors, or New Structures?

Evelyn Fox Keller, Sandra Harding, and others demonstrate that at some level, science embraces the ideals of objectivity, neutrality, and detachment. They also show that science is, and has been, if not quite masculine, at the very least for men. I argued in chapter one that it is very difficult to draw a convincing connection between these two claims, because the content of the concepts in question has been so variable over time and across communities. There are further issues about the efficacy of this project that I wish to raise. Nonetheless, in light of the proposal outlined in chapter five, I wish to re-value the work of Keller and Harding as vital to the task of telling new feminist narratives about the nature and history of science.

The merit of feminist positions which seek to connect the ideals of science to the absence of women from science is that they provide the necessary structural/analytical basis from which to launch new narratives about science, analogous to the structural account of evolutionary narratives offered by Misia Landau. They show us that many of the categories upon which historians and philosophers of science have relied have produced theories of science that are simple-minded, blinkered, even dull. Some scientists may be encouraged to think and talk about their work as though it were objective and disinterested, but this is merely a posture. There is very little in the day to day behaviour of scientists, especially when considered over time and across social boundaries, that corresponds to the abstract philosophical ideals of objectivity and detachment. This suggests that such ideals do not serve the functions that both feminists and scientists have identified, but mediate and obscure relations between science and society at large.

Even if we can overlook the complications that I raised for any convincing account of the masculine nature of science, we must still ask whether such an account does the sort of work that some feminists want it to do. I submit that there is an explanatory gap between the assertion of masculinity in the norms and ideals of science, and the androcentric content of science. This gap exists because there is no account given of precisely how masculinist ideals of reason and objectivity function in the everyday work of science. We know that social, theoretical, and personal biases can limit the perspective and awareness of scientists, but we do not know what the relationship is between these biases and the overarching scientific values to which Harding and others direct our attention. I suggest that, given the rich texture that such biases have, and the complexity that overarching scientific values exhibit, a general, global account of such a relationship is not a productive goal for feminism. This means that a reconsideration of the values and prejudices of the individuals who do science is in order. Masculine objectivity as such is not the problem, or is at least not the whole problem.[1]

In one sense, Keller, Harding, and other feminist philosophers of science are not telling us anything new when they point to the objectivity and masculinity of science, so the value of their project does not lie in the fact that it makes us aware of facets of science which were previously hidden from us. (I am here taking "masculine" in the weaker sense identified above, i.e., that science is identified as something for men.) We need not look far in order to verify the claim that the positions they outline are not novel. The urge to describe and defend science by reference to its objectivity and neutrality has not abated, in spite of challenges brought by philosophers, historians, sociologists and even journalists and politicians. On the contrary, it seems that such ideals for science are currently defended more ardently than ever.[2] But if the connection between these ideals and scientific practice is a shaky one for feminists, it is no less precarious for scientists and their champions. Thus structural similarities between the feminist critique of science and mainstream philosophy of

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[1] Mary Hawkesworth makes a similar point with respect to the objectification of women through the application of allegedly masculine intellectual rules, "From Objectivity to Objectification: Feminist Objections" in Megill (1994).

[2] See, for example, Gross and Levitt (1994), and Ruse's review of it, *The Sciences* (November 1994)

science reveal a natural point of departure from which to ask new questions.

Similarly, the idea that science is masculine at least in the minimal sense of being for men is not a surprising one, especially if we take an historical view. Women have long done science in the absence of any widespread acceptance of their right, or even their ability, to do it. Formal barriers to women's participation in science have only recently fallen away. Systemic barriers still exist. And even in the absence of impediments formal and informal, continued scientific obsession with gender differences tacitly reinforces the idea that women are naturally unsuited to science. Once again, general structural commonalities between the androcentric agenda of scientists and the feminist critique of science highlights a significant absence. If the claim that women cannot or ought not do science is unwarranted, so is the claim that men can and ought, at least according to the principles for the doing of science that philosophy has so far identified.

Nevertheless, feminists recognize in objectivity the kernel of an idea that ought to be preserved in science. This, too, is hardly surprising. It is unlikely that feminist objections to the content of science would be taken seriously if feminists were perceived to have given up the ideas of truth, rationality and objectivity altogether. This points to one of the central strengths of the feminist project in philosophy of science, namely its ability to demonstrate that the very meaning of our current scientific norms and ideals could have been different, and may therefore be changed. This point is reinforced by the historical perspective I offered in the first chapter. It is tied to the arguments of the second and third chapters, as well, in the following ways.

Feminist philosophers of widely diverse positions tend to resist the idea that there is anything interesting to be learned by focusing attention on the biases and preconceptions of individual practitioners of science. The assumption here is that since individual behaviour is governed by the norms and standards of the community, there is nothing revealing about it. Furthermore, any behaviour that departs from those norms in certain rather flagrant ways will merely be labelled "bad science", and will likewise fail to be revealing. Once again, however, I would suggest that the fact that these positions are shared by mainstream scientists and philosophers presents feminist critics with an invaluable opportunity to raise new doubts and ask new questions. It is troubling that the net effect of the emphasis on the self-effacing objectivity of the scientific method and the shift toward communally sustained epistemic standards is the same.

Just as there is a social-intellectual hierarchy in which science occupies a prestigious position, there is a hierarchy within science. And just as the values esteemed by science in general may be thought to differ from those favoured by the rest of society, there may be disparities in values among scientists depending on where in the scientific hierarchy they find themselves. Philosophers of science do not typically consider that the best accounts of the work of the great men of science may be utterly inadequate to explain the work of its plodders and drudges, and vice-versa. It may be the case that the compensatory function which scientific method was originally designed to fulfill, to level men's wits, has become pathological in the context of contemporary institutionalized science. The discourses of scientific objectivity and rationality may be serving as camouflage for the absence of real scientific acumen in some quarters.

II. Rational Persuasion

Although I am convinced that feminist empiricism represents the most fruitful theoretical stance for feminist philosophy of science, it is faced with some problems that cannot be diminished just by extending our notions of evidence and experience to include the narrative strategy that I outlined in chapter five. Nor can these problems be resolved by recognizing feminist empiricism, standpoint theory, postmodernism and narrative as conjoint strategies designed to meet the different needs and goals of different parts of the feminist agenda.

Nelson gives a compelling argument that careful consideration of the feminist critique of science is warranted by standards that empiricists already accept, or should accept, if they wish to be consistent with other elements of the position they profess. Addelson, Longino and Harding add their voices to Nelson's with their insistence that the inclusion of new perspectives in science, rather than threatening the objectivity and rationality of science, will make it better, stronger. At the very least, the relevance of these issues cannot be ruled out *a priori*, without betraying existing principles of science, and if they are ignored, science may be held back instead of improved. But as Max Planck warned us in a similar context, "a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it".[3]

If we take the observations of Kuhn and Feyerabend, concerning the non-rational and sometimes irrational nature of science, seriously, then we may have to concede that it might not matter whether feminist critics of science can marshal an excellent case, and it might not matter that we can articulate the theoretical underpinnings of the feminist critique in a persuasive way. The option of refusing to

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[3] Barber, "Resistance by Scientists to Scientific Discovery" (1961) p. 597 (quoting Planck's *Scientific Autobiography*).

hear, or hearing but refusing to respond, even in the face of the best possible evidence, will remain at the discretion of powerful scientists and philosophers. Thus feminist philosophers of science are placed in a very strange and paradoxical position *vis-a-vis* rational persuasion. While we recognize the ills to which it is heir and may in the end be defeated by those very shortcomings, we must nonetheless rely on it in order to get our point across at all.

Disruptive feminist narratives are valuable in this regard, because they can help ensure that the "new generation" to which Planck refers is exposed to and, it is hoped, participates in telling the new truths that may be more convincing once their opponents die. But the weight of resistance to such narratives, and even to more straightforwardly scientific or philosophical accounts, is powerful and, unfortunately, threatening. Thus rational persuasion presents a second and even more serious obstacle to feminist theorizing about science, and to feminist goals more generally.

As feminists we must consider that ultimately no strategy, whether couched in terms of evidence, rationality and objectivity, or narrative, disruption and subversion, may be able to overcome androcentric bias in science or elsewhere. Both of these strategies, as different as they are, share fundamentally in the belief that the root cause of women's oppression by science is lack of information. Science is seen as harmful to women because its mostly male practitioners have paid insufficient attention to the influence of their masculinist and pro-scientific biases, and are often unable to perceive those biases even when feminists try to help them to do so, precisely because those biases are so strong and so widespread. Those in power are unlikely to be eager to join in a project that demands the curtailing of their authority, no matter how compellingly we argue that their own standards of objectivity and rationality require it of them But it may be the case that science and its associated ideals, authoritativeness, and prestige has excluded, confined, and even harmed women because men have wanted it that way.

We must consider the possibility that male power and privilege, including scientific power and privilege, rest not on ignorance but on evil. A new narrative may be one that takes as its starting point the assertion that it is men, not science, who oppress women, and considers that science may have its current and historical structure and consequences because men hate women. This may be the most disruptive and dangerous story of all.

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```
xdelta=(coi2-coi1)/numco;
% calculate reactor N2O by solving 5th order equation
for i=1:numco
count=count+1;
coi=coi2-xdelta*(i-1);
m1=coi-n2oi;
q1=k1*(1.+k32)-k2*k38*(1+k8);
t1=k1*m1*(1.+k32);
q2=mco*k1*(1.+k32)+k2*(1.-k8*k38);
t2=(mco*k1*m1+1.)*(1.+k32);
a=klh*k2*k38;
a1=a*(q1*(q2-q1));
b1=a*(q1*(t2-t1)+t1*(q2-q1));
c1=a*t1*(t2-t1);
a2=q1*q2*k2+k2*k38*q2*q2;
b2=q1*(t2*k2+q2*(1.+k32))+t1*q2*k2+2.*k2*k38*q2*t2;
c2=q1*t2*(1.+k32)+t1*(t2*k2+q2*(1.+k32))+k2*k38*t2*t2;
d2=t1*t2*(1.+k32);
bb=a1-a2*n2oi+b2;
cc=b1-b2*n2oi+c2;
dd=c1-c2*n2oi+d2;
ee=-d2*n2oi;
zz=[a2 bb cc dd ee];
zroot=roots(zz);
[m,l]=size(zroot);
for ir=1:4
xroot(ir)=0.0;
end
n=0;
%eliminate imaginary and negative real roots
for ir=1:m
if imag(zroot(ir,1))==0.0
if real(zroot(ir,1))>0.
if real(zroot(ir,1))<=n2oi
n=n+1;
xroot(n)=zroot(ir,1);
end
end
end,end
%matrix xxx stores the positive real roots(N2O)
xxx(i,1)=n;
xxx(i,2)=coi;
xxx(i,3)=xroot(1);
if n>1
xxx(i,4)=xroot(2);
end
if n>2
xxx(i,5)=xroot(3);
end
if n>3
xxx(i,6)=xroot(4);
end
```

```
for jj=1:4
tco(jj)=0;
to(jj)=0;
rate(jj)=0;
if xroot(jj)>0.
tco(jj)=(q1*xroot(jj)+t1)/(q2*xroot(jj)+t2);
to(jj)=((1.-tco(jj))*k38*k2*xroot(jj))/ .....
(k38*k2*xroot(jj)+tco(jj)*(1.+k32+k2*xroot(jj)));
rate(jj)=(klh*tco(jj)*to(jj)*100.)/coi;
end,end
nroot=0;
for jj=1:4
if tco(jj)>=0.
if to(jj)>=0.
if tco(jj)<1.
if to(jj)<1.
if rate(jj)>0.
if rate(jj)<100.
if (to(jj)+tco(jj)) <1.
nroot=nroot+1;
res1(i,2+nroot)=rate(jj);
end,end,end,end,end,end,end,end
nroot;
res1(i,1)=nroot;
res1(i,2)=coi;
end
for i=1:numco
if res1(i,1)==3
if nrtsav==1
num1=i;
xhbpt=res1(i,2)+xdelta/2.;
nrtsav=3;
end,end
if res1(i,1)==1
if nrtsav==3
num2=i;
xlbpt=res1(i,2)+xdelta/2.;
nrtsav=1;
end,end,end
%create the vectors a and b for plotting
for i=1:(num2-1)
a1(i)=res1(i,2);
b1(i)=res1(i,3);
end
for i=num1:(num2-1)
b3(i+1-num1)=res1(i,5);
a3(i+1-num1)=res1(i,2);
end
num3=num2-num1;
for i=1:num3
a2(i)=res1((num2-i),2);
b2(i)=res1((num2-i),4);
```

```
end
for i=num2:numco
a4(i+1-num2)=res1(i,2);
b4(i+1-num2)=res1(i,3);
end
a=[a1 a2 a3 a4];
b=[b1 b2 b3 b4];
plot(a,b)
title('CO conversion prediction from Mechanism 3')
xlabel('CO in feed, %'),ylabel('CO Conversion, %')
sumerr=sumerr+((((xbpll+xbplh)/2.)-xlbpt)^2)+((((xbphl+xbphh)/2.)-xhbpt)^2);
xlb(itime)=xlbpt;
xhb(itime)=xhbpt;
if itime==1
save resa res1 /ascii
end
if itime==2
save resb res1 /ascii
end
if itime==3
save resc res1 /ascii
end
clear a,clear a1,clear a2, clear a3, clear a4
clear b, clear b1, clear b2, clear b3, clear b4
end
shg
```

```
%PROGRAM: MECH4.M
%THIS PROGRAM CALCULATES CO CONVERSION BY MECHANISM 4
% REACTIONS STEPS SIMILAR TO MECHANISM #1
% CO SELF-EXCLUSION EFFECT IS INCLUDED
% A SIMILAR PROGRAM "MECH4B.M" IS USED FOR CALULATIONS AT 520 K WITH FIXED
%FEED %CO AND VARIABLE FEED %N2O
a230, b230, c230
%a210,b210,c210
%a190, b190, c190
v=[0,2.,0,104];axis(v)
%plot the experimental CO conversion Vs feed CO
plot(y(:,1),y(:,2),'o'),hold
plot(x(:,1),x(:,2),'*')
%temp=input('enter temperature, K')
temp=499.0;
mult=(103000.)/(831.4*temp);
plot(z(:,1),z(:,2),'x')
%Enter the value of KLH after multiplying by 10e7 .
k=input('K1 K4 K48 Klh Nco')
%THE VALUES OF K AT 499K ARE K1=1200 K21=0.0173 K23=8 KLH=13.076 %Nco=1.025
%convert the parameters for use with mol% data
k1=k(1)*mult;
k3=k(2)*mult;
k37=k(3)*mult;
k12=k(4)*1.4529731
rnco=k(5);
%read data file d230.m
d230, %d210, %d190
sumerr=0.0;
for itime=1:3
nrtsav=1;
coi1=data(itime,2);
coi2=data(itime,3);
n2oi=data(itime,1);
xbpll=data(itime,4);
xbplh=data(itime,5);
xbphl=data(itime,6);
xbphh=data(itime,7);
numco=data(itime,8);
for j=1:numco, for l=1:6
xxx(j,l)=0.0 ;
end,end
for j=1:numco, for l=1:5
resa(j,l)=0;res1(j,l)=0;
end,end
count=0;
xdelta=(coi2-coi1)/numco;
% calculate reactor N2O by solving 4th order equation
for i=1:numco
count=count+1;
coi=coi2-xdelta*(i-1);
k7=k3/k37;
```

```
del=k1*(coi-n2oi);
k10=k1-(1.+1./k7)*k3;
k11=k1-k3;
k37=k3/k7;
gamma=1.+rnco*del;
k14=rnco*k1-k3;
k15=(rnco-1.)*k1+k3/k7;
k16=k1-k3+rnco*del*k3/k7;
aa=-k14*k14*k37;
bb=k14*k14*k37*n2oi-k14*(gamma*k37+k16)-k12*k10*k15;
cc=(gamma*k37+k16)*k14*n2oi-gamma*k16-del*k14-k12*(del*k15+(gamma-del)*k10);
dd=(gamma*k16+del*k14)*n2oi-gamma*del-k12*del*(gamma-del);
ee=gamma*del*n2oi;
z=[aa,bb,cc,dd,ee];
zroot=roots(z);
[m,l]=size(zroot);
for ir=1:4
xroot(ir)=0.0;
end
n=0;
%eliminate imaginary and negative real roots
for ir=1:m
if imag(zroot(ir,1))==0.0
if real(zroot(ir,1))>0.
n=n+1;xroot(n)=zroot(ir,1);
end,end,end
%matrix xxx stores the positive real roots(N2O)
xxx(i,1)=count; xxx(i,2)=n; xxx(i,3)=xroot(1);
if n>1, xxx(i,4)=xroot(2); end
if n>2, xxx(i,5)=xroot(3);end
if n>3, xxx(i,6)=xroot(4),end
end
%res1 stores the CO conversions, res2 stores CO coverages
%res4 stores the value of the polynomial (should be zero)
for i=1:numco
for j=1:4
res1(i,j)=0.0; res2(i,j)=0.0
res3(i,j)=0.0;%res4(i,j)=0.0
resa(i,j)=0.0;
res5(i,j)=0.0;
end, end
for i=1:numco
xdelta=(coi2-coi1)/numco;
coi=coi2-xdelta*(i-1);
nroot=0;
for ic=3:5
n2o=xxx(i,ic);
if n2o>0.
co=coi-(n2oi-n2o);
ic=ic-2;
coconv(ic)=(1-co/coi)*100.;
%rate(ic)=(coi-co)*68.824404;
```

```
tco(ic)=(k1*co-k3*(1.+1./k7)*n2o)/(1.+rnco*k1*co-k3*n2o);
to(ic)=k3*n2o*(1.+rnco*k1*co-k1*co+k3*n2o/k7)/ ....
(k7*(k1*co-k3*n2o)+k3*n2o*(rnco*k1*co-k3*n2o));
rate(ic)=k12*to(ic)*tco(ic);
res2(i,ic)=co;
res3(i,ic)=to(ic);
res5(i,ic)=rate(ic);
res1(i,ic)=coconv(ic);
if tco(ic)>=0.0
if tco(ic)<1.
if to(ic)>=0.0
if to(ic)<1.
if (tco(ic)+to(ic))<1.
if coconv(ic)>=0.
if coconv(ic)<100.
nroot=nroot+1;
end, end, end, end, end, end, end, end, end
% determine the lower and upper bifurcation points
res1(i,4)=coi;
res1(i,5)=nroot;
res2(i,4)=nroot;
if nroot==3
if nrtsav==1
xhbpt=coi+xdelta/2;
nrtsav=3; num1=i;
end,end
if nroot==1
if nrtsav==3
num2=i; nrtsav=0;
xlbpt=coi+xdelta/2;
end,end
end
%create the vectors a and b for plotting
for i=1:(num2-1)
a1(i)=res1(i,4);
b1(i)=res1(i,1);
end
for i=num1:(num2-1)
b3(i+1-num1)=res1(i,3);
a3(i+1-num1)=res1(i,4);
end
num3=num2-num1;
for i=1:num3-1
a2(i)=res1((num2-i),4);
b2(i)=res1((num2-i),2);
end
for i=num2:numco
a4(i+1-num2)=res1(i,4);
b4(i+1-num2)=res1(i,1);
end
a=[a1 a2 a3 a4];
b=[b1 b2 b3 b4];
```

```
p... (a b)
xlabel('CO % in feed'),ylabel('% CO Conversion')
sumerr=sumerr+((((xbpll+xbplh)/2.)-xlbpt)^2)+((((xbphl+xbphh)/2.)-xhbpt)^2);
xl(itime)=xlbpt;
xh(itime)=xhbpt;
clear a a1 a2 a3 a4 b b1 b2 b3 b4
for i=1:numcc
resa(i,1)=res1(i,5);
resa(i,2)=res1(i,4);
resa(i,3)=res1(i,1);
resa(i,4)=res1(i,2);
resa(i,5)=res1(i,3);
end
%SAVE THE RESULTS IN ASCII FILE FOR PLOTTING
if itime==1, save resa resa /ascii, end
if itime==2, save reso resa /ascii, end
if itime==3, save resc resa /ascii, end
end
shg
```

```
%PROGRAM BIFRMEC4.M
% This program generates data for the lower
% and upper bifurcation curves for Mechanism 4
temp=input('enter temperature, K')
mult=(103000.)/(831.4*temp);
k=input('K1 K21 K23 KLH Nco')
% convert the parameters for use with mol% data
k1=k(1)*mult;
k3=k(2)*mult;
k37=k(3)*mult;
k12=k(4)*1.4529731;
rnco=k(5);
for jtime=1:100, for i=1:3
res2(jtime,i)=0;
end,end
for jtime=1:100
n2oi=0.3+(1.3/99)*(jtime-1);
nrtsav=1;
%SELECT THE MIN.(coi1) AND MAX.(coi2) CO% IN FEED
coi1=0.05; coi2=1.0;
numco=100; %numco = NO. OF STEPS BETWEEN coi1 AND coi2.
for j=1:numco, for l=1:6
xxx(j,l)=0.0 ;
end, end
for j=1:numco, for l=1:5
res1(j,l)=0;
end,end
count=0;
xdelta=(coi2-coi1)/numco;
% calculate reactor N2O by solving 4th order equation
for i=1:numco
count=count+1;
coi=coi2-xdelta*(i-1);
k7=k3/k37;
del=k1*(coi-n2oi);
k10=k1-(1.+1./k7)*k3;
k11=k1-k3;
k37=k3/k7;
gamma=1.+rnco*del;
k14=rnco*k1-k3;
k15=(rnco-1.)*k1+k3/k7;
k16=k1-k3+rnco*del*k3/k7;
aa=-k14*k14*k37;
bb=k14*k14*k37*n2oi-k14*(gamma*k37+k16)-k12*k10*k15;
cc=(gamma*k37+k16)*k14*n2oi-gamma*k16-del*k14-k12*(del*k15+(gamma-del)*k10);
dd=(gamma*k16+del*k14)*n2oi-gamma*del-k12*del*(gamma-del);
ee=gamma*del*n2oi;
z=[aa,bb,cc,dd,ee];
zroot=roots(z);
[m,l]=size(zroot);
for ir=1:4
xroot(ir)=0.0;
```

```
end
n=0;
%eliminate imaginary and negative real roots
for ir=1:m
if imag(zroot(ir,1))==0.0
if real(zroot(ir,1))>0.
n=n+1;
xroot(n)=zroot(ir,1);
end ,end, end
%matrix xxx stores the positive real roots(N2O)
xxx(i,1)=count;
xxx(i,2)=n;
xxx(i,3)=xroot(1);
if n>1, xxx(i,4)=xroot(2);end
if n>2, xxx(i,5)=xroot(3);end
if n>3, xxx(i,6)=xroot(4);end
end
%res1 stores the CO conversions, res2 stores CO coverages
for i=1:numco
xdelta=(coi2-coi1)/numco;
coi=coi2-xdelta*(i-1);
nroot=0;
for ic=3:5
n2o=xxx(i,ic);
if n2o>0.
co=coi-(n2oi-n2o);
ic=ic-2;
coconv(ic)=(1-co/coi)*100.;
tco(ic)=(k1*co-k3*(1.+1./k7)*n2o)/(1.+rnco*k1*co-k3*n2o);
to(ic)=k3*n2o*(1.+rnco*k1*co-k1*co+k3*n2o/k7)/(k7*(k1*co-k3*n2o)+k3*n2o*(rnco*k1*co-k3*n2o));
rate(ic)=k12*to(ic)*tco(ic);
res1(i,ic)=coconv(ic);
if tco(ic)>=0.0
if tco(ic)<1.
if to(ic)>=0.0
if to(ic)<1.
if (tco(ic)+to(ic))<1.
if coconv(ic)>=0.
if coconv(ic)<100.
nroot=nroot+1;
end, end, end, end, end, end, end, end, end
% determine the lower and upper bifurcation points
res1(i,4)=coi;
res1(i,5)=nroot;
if nroot==3
if nrtsav==1
xhbpt=coi+xdelta/2;
nrtsav=3; num1=i;
end,end
if nroot==1
if nrtsav==3
num2=i;nrtsav=0;
```

```
xlbpt=coi+xdelta/2;
end,end
end
%create the vectors a and b for plotting
for i=1:(num2-1)
a1(i)=res1(i,4);
b1(i)=res1(i,1);
end
for i=num1:(num2-1)
b3(i+1-num1)=res1(i,3);
a3(i+1-num1)=res1(i,4);
end
num3=num2-num1;
for i=1:num3-1
a2(i)=res1((num2-i),4);
b2(i)=res1((num2-i),2);
end
for i=num2:numco
a4(i+1-num2)=res1(i,4);
b4(i+1-num2)=res1(i,1);
end
a=[a1 a2 a3 a4];
b=[b1 b2 b3 b4];
%res2 STORES THE CALCULATED BIFURCATION POINTS.
res2(jtime,1)=n2oi;
res2(jtime,2)=xlbpt;
res2(jtime,3)=xhbpt;
clear a a1 a2 a3 a4 b b1 b2 b3 b4 res1 num1 num2 num3
end, %SAVE THE RESULTS IN ASCII FILE FOR PLOTTING
save bifr1 res2 /ascii
```

```
%PROGRAM: LORATE.M
 echo off
%THIS PROGRAM CALULATES THE CO2 FORMATOION RATE BY MECHANISM 4.
%RATES FOR HIGH CONVERSION BRANCH CAN BE FOUND FROM PROGRAM   mech1.m
%THIS PROGRAM CALULATES THE RATE FOR LOW CONVERSION BRANCH ONLY
%BY USING EQUATION (28).
c230,  % EXPT. DATA AT 490 K WITH FEED N2O=1.2%
v=[0.6,2,0,40];
axis(v)
for ii=1:25
z(ii,3)=z(ii,1)*z(ii,2)*0.6875;
end
plot(z(:,1),z(:,3),'o')
hold
temp=499;
mult=103000./(831.4*temp);
k=input('k1,k23,klh')
%CONVERT THE INPUT K VALUES TO USE THE MOL% DATA INSTEAD
%INSTEAD OF mol/cu.m
k1=k(1)*mult;
k23=k(2)*mult;
%TO CONVERT KLH VALUE MUTIPLY BY (100.R.T/Q.P)
klh=k(3)*1.4529731/2;
%DATA FILE d230.m CONTAINS EXPT. BIFURCATION POINTS
d230
sumerr=0.0;
for itime=3:3
nrtsav=1;
coi1=data(itime,2);
coi2=data(itime,3);
n2oi=data(itime,1);
xbpll=data(itime,4);
xbplh=data(itime,5);
xbphl=data(itime,6);
xbphh=data(itime,7);
numco=data(itime,8);
for j=1:numco
for l=1:6
xxx(j,l)=0;
end
end
xdelta=(coi2-coi1)/numco;
for i=1:numco
%SOLVE THE CUBIC EQUATION FOR REACTOR N2O%
coi=coi2-xdelta*(i-1);
m1=coi-n2oi;
 nco=1.025;
aa=-nco*nco*k1*k23;
bb=nco*k1*(n2oi*k23*nco-(1+k23*nco*m1))-klh*k23*((nco-1)*k1+k23);
cc=nco*k1*(n2oi*(1+k23*nco*m1)-m1)-klh*k23*(1+k1*m1*(nco-1));
dd=nco*k1*n2oi*m1;
z=[aa bb cc dd];
```

```
zroot=roots(z);
n=0;
for ir=1:3
if imag(zroot(ir))==0.
if real(zroot(ir))>0.
n=n+1;
xroot(n)=zroot(ir);
end,end
end
xxx(i,1)=n;
xxx(i,2)=coi;
xxx(i,3)=xroot(1);
if n>1
xxx(i,4)=xroot(2);
end
if n>2
xxx(i,5)=xroot(3);
end
end
[m,l]=size(xxx);
numco=m;
for i=1:m
for j=1:4
res2(i,j)=0;
res3(i,j)=0;
res4(i,j)=0;
res5(i,j)=0;
end,end
for i=1:m
for j=1:5
res1(i,j)=0;
resa(i,j)=0;
end,end
for i=1:numco
xdelta=(coi2-coi1)/numco;
coi=coi2-xdelta*(i-1);
nroot=0;
res1(i,4)=coi;
for ic=2:4
n2o=xxx(i,ic);
if n2o>0.
co=coi-(n2oi-n2o);
ic=ic-1;
rate(ic)=(coi-co)*68.75;
res1(i,ic)=rate(ic);
end, end
end
end
plot(res1(:,4),res1(:,2))
xlabel('%CO in the feed')
ylabel('Rate of CO2 formation, nano mol/g.s.')
shg
```

```
%FILE a190.m
%EXPERIMENTAL DATA AT 461 K WITH 0.4% N2O IN THE FEED
x=[0.0503 0.1002 0.1501 0.2002 0.2356 0.2500 0.2667 0.2854 0.8860 0.7858  0.6862 0.5858 0.4877 .......
   0.3860 0.3362 0.2854 0.2360 0.2002 0.1802 0.1503  0.1402 0.1303 0.1202 0.1102 0.1002
   100 100 100 100 100 100 100 11.45 2.74 2.85 3.26 4.02 4.63 7.41 8.68 10.68 13.68 20.33 29.91 ........
   38.99 45.43 52.42 61.48 100 100].';

%FILE b190.m
%EXPERIMENTAL DATA AT 461 K WITH 0.7% N2O IN THE FEED
y=[0.1863 0.2862 0.3161 0.3363 0.3659 0.3861 0.7861 0.8861 0.7875 0.6861 0.5861 0.4858 0.4363 ......
   0.3857 0.3362 0.2861 0.2354 0.2012 0.1911 0.1856 0.1610 0.1401 0.1302
   100 100 100 100 100 12.71 5.26 4.47 4.61 5.16 6.02 7.72 10.03 12.07 15.28 19.35 25.57 32.06 ......
   33.12 36.34 52.00 100 100].';

%FILE c190.m
%EXPERIMENTAL DATA AT 461 K WITH 1.2% N2O IN THE FEED
z=[0.1860 0.2361 0.2860 0.3359 0.3859 0.4357 0.4859 0.5359 0.5366 0.5466 0.5564 0.5760 0.8862 ......
  0.8358 0.7855 0.7358 0.6858 0.6362 0.5861 0.5362  0.4863 0.4361 0.3861 0.3364 0.2864 0.2500 .....
  0.2362 0.2301 0.1901 0.1859
   100 100 100 100 100 100 100 100 100 100 15.46 12.05 8.43 8.07 8.19 8.49 8.95 9.65 11.43 12.35 ......
    13.64 15.65 18.14 21.65 26.94 47.60 48.77 55.84 100 100].';


%FILE a210.m
%EXPERIMENTAL DATA AT 480 K WITH 0.4% N2O IN THE FEED
x=[0.1001 0.1201 0.1401 0.1602 0.1665 0.1801 0.1863 0.2864 0.2967 0.3072 0.3159 0.3256 0.3371 .....
   0.3469 0.3569 0.3870 1.9297 1.7284 1.5263 1.3305 1.1305 0.9269 0.8307 0.7859 0.6864 0.5863 ......
   0.4855 0.3850 0.2859 0.2663 0.2458 0.2265 0.2201 0.2001
   100 100 100 100 100 100 100 100 100 100 100 100 100 100 24 18 2.91 3.05 3.4 4.04 4.93 6.32 ......
   7.13 8.61 9.15 11.03 14.19 20.15 31.37 40.7 45.08 55.06 100 100].';


%FILE b210.m
%EXPERIMENTAL DATA AT 480 K WITH 0.7% N2O IN THE FEED
y=[0.1869 0.2858 0.3863 0.4859 0.5855 0.5857 0.6064 0.6261 0.7862 0.8861 1.9301 1.7293 1.5268 ......
   1.3287 1.1297 0.9291 0.8862 0.7865 0.6860 0.5858 0.4860 0.4363 0.3857 0.3362 0.2855 0.2660 ......
   0.2358 0.1857
   100 100 100 100 100 100 97.57 20.46 14.16 11.81 4.84 5.15 5.76 6.88 8.36 10.79 12.36 13.08 ....
   15.03 18.29 24.08 26.99 32.55 40.62 58.54 100 100 100].';


%FILE c210.m
%EXPERIMENTAL DATA AT 480 K WITH 1.2% N2O IN THE FEED
z=[0.1851 0.2862 0.3865 0.4858 0.5859 0.6865 0.7365 0.7852 0.8386 0.8583  1.7333 1.5307 1.3289 .....
  1.1299 0.9289 0.7475 0.6860 0.5856 0.4861 0.3859 0.3658 0.3461
   100 100 100 100 100 100 100 100 100 15.03 7.61 8.43 9.72 11.59 14.71 19.83 27.49 33.71 44.02 ....
   66.03 100 100].';
```

```
% FILE a230.m
% EXPERIMENTAL DATA AT 499 K WITH FEED N2O=0.4%
x=[0.1658 0.1856 0.2358 0.2861 0.2864 0.3263 0.3370 0.3457 0.3662 0.3864 0.4052 0.4259 0.4859 .....
 0.5866 0.6864 1.9624 1.7341 1.5340 1.3341 1.1337 0.9319 0.7801 0.3849 0.3508 0.3309 0.3103 .....
 0.2903 0.2861 0.2706 0.1859
  100 100 100 100 100 100 100 100 100 41.45 38.82 33.73 24.87 18.18 13.70 4.63 4.60 5.14 6.17 ....
  7.49 9.69 11.98 42.09 44.67 50.87 56.45 62.79 100 100 100].';


% FILE b230.m
% EXPERIMENTAL DATA AT 499 K WITH FEED N2O=0.7%
y=[0.1853 0.2356 0.2856 0.3357 0.3362 0.3853 0.5862 0.6360 0.6568 1.9288 1.7300 ......
1.5279 1.3291 1.1284 0.7854 0.6859 0.5856 0.4864 0.4359 0.4259 0.4055 0.3859 0.3853
100 100 100 100 100 100 100 100 31.86 7.23 7.59 8.72 9.98 12.37 24.60 29.24 35.97 47.78 .......
61.21 100 100 100 100].';

% FILE c230.m
% EXPERIMENTAL DATA AT 499 K WITH FEED N2O=1.2%
z=[0.7475 0.76 0.8315 0.8337 0.9301 0.9330 1.0289 1.0305 1.0549 1.0815 1.2289 1.9598 1.7310 .....
 1.5315 1.3317 0.8857 0.7858 0.6858 0.5965 0.5861 0.5665 0.4854 0.3856 0.2859 0.1852
  100 100 100 100 100 100 100 100 100 20.32 18.49 9.97 11.08 13.82 16.64 27.92 33.28 39.77 ......
  55.78 100 100 100 100 100 100].';

%FILE a245.m
%EXPERIMENTAL DATA AT 520 K WITH 0.6759% CO IN THE FEED
x=[0.1995 0.2998 0.3997 0.5000 0.5501 0.6002 0.7000 0.7304 0.7504 0.7703 0.7905 0.8104 0.8306 .....
 0.9105 0.9004 0.8506 0.8004 0.7504 0.7303 0.7103 0.6902 0.6703 0.6503
  19.50 27.84 35.85 43.75 53.99 59.42 63.40 71.34 73.54 75.06 80.66 100 100 100 100 100 100 .....
  100 100 100 100 100 70].';


%FILE b245.m
%EXPERIMENTAL DATA AT 520 K WITH 0.915% CO IN THE FEED
y=[1.7992 1.5982 1.4989 1.3986 1.3003 1.2582 1.2286 1.2000 1.1686 1.1384 1.1076 1.0995 1.0986 ....
  1.0786 1.0486 1.0185 0.9988 0.9890 0.9570 0.9276 0.8986 0.8287 0.8006 0.7009 0.6007 0.5005 ....
  0.4005 0.3001 0.1997
  100 100 100 100 100 100 100 100 100 100 100 75.44 100 100 100 100 62.57 100 100 58.90 55.72 .....
  50.41 49.03 42.98 36.47 30.71 24.83 19.80 13.89];


%FILE c245.m
%EXPERIMENTAL DATA AT 520 K WITH 1.166% CO IN THE FEED
z=[1.8014 1.8004 1.6999 1.6290 1.5981 1.5682 1.5386 1.5000 1.4989 1.3994 1.3989 1.3486 1.3190 .....
  1.3008 1.2994 1.2895 1.2586 1.1989 1.0983 0.9984 0.8980 0.8009 0.7009 0.6009 0.5007 0.4003 ....
  0.3000 0.1998
  100 100 100 100 69.93 100 64.92 100 60.06 100 100 100 55.11 100 100 50.65 45.08 41.05 36.91 ....
  31.44 27.70 24.47 20.40 16.56 13.01 9.34].';
```

```
%The following data files contain Program inputs
%Each row contains %CO in feed, Low anf high limits
%of feed %CO, experimental lower and upper bifurcation
%points and the number of points to be used to draw
%a smmoth curve

%DATA FILE d190.m for experiments at 461 k
data=[.3999 .01  1. .1102 .1202 .2667 .2854 200
      .7000 .01  1. .1401 .1610 .3659  .3860 200
     1.1992 .010 1. .1901 .2301  .5466  .5564 200];

%DATA FILE d210.m for experiments at 480 K
data=[.3999 .080 2. .2201 .2265 .3469 .3569 200
      .7000 .080 2. .2660 .2855  .6064 .6261 200
     1.1992 .080 2. .3658 .3866  .8386  .8583 200];


%DATA file d230.m for experiments at 499 K
data=[0.40  .08 2. .2861 .2903 .3662 .3864 200
      0.7   .08 2. .4259 .4359  .6360 .6568 200
     1.19920  .08 2.0  .5861 .5965 1.0549 1.0815 200];


%DATA file d245.m for experiments at 245 K
%To be used for Program mech4b.m
data=[0.6759 0.2 2 0.6503 0.6703 0.7905 0.8104 200
      0.9154 0.2 2 0.9276 0.9570 1.0483 1.0786 200
      1.1661 0.2 2 1.2295 1.2586 1.5682 1.5980 200];
```

APPENDIX D

EXPERIMENTAL TIME-AVERAGE CO CONVERSIONS FOR THE N_2O+CO REACTION DURING FEED COMPOSITION CYCLING

The experimental values of time-average CO conversions during feed composition cycling experiments are listed in this Appendix. The instantaneous experimental data of time versus feed flows and reactor concentrations are available in the form of compressed ASCII files on a magnetic tape.

The following conditions were always maintained for all of the experiments.

Reactor temperature		499 K
Reactor pressure		103 kPa
Feed flow		185 sccm
Feed composition during cycling,		
	N_2O	0 - 2.4%
	CO	0 - 2.4%
	N_2	balance

Table D.1

Time-Average CO Conversions During Out-of-Phase Cycling

(with N_2O Phase-Lead of 180°)

Frequency, mHz	Time-Average CO Conversion, percent	Frequency, mHz	Time-Average CO Conversion, percent
0.7689	18.98	5.0231	77.10
0.8347	19.65	5.7444	79.62
0.9084	21.72	6.7349	79.49
1.1097	25.97	6.7349	78.94
1.1765	27.11	7.5120	76.55
1.3377	30.40	7.8125	76.99
1.5379	34.11	8.1380	76.40
1.5379	34.17	8.4984	75.22
1.6693	36.39	9.2798	73.26
1.8253	39.13	9.7656	71.38
1.9929	42.20	9.7656	70.94
1.9929	42.49	10.8507	68.45
2.2194	46.01	10.8507	67.92
2.5040	50.96	11.4889	66.00
2.5040	52.11	12.2070	30.83*
2.8722	56.92	13.0208	29.19
3.1001	60.13	13.0208	30.76
3.3103	62.91	13.9509	29.01
3.9859	70.29	16.2760	28.59
3.9859	72.42	17.7556	26.95
4.1555	73.71	19.5312	28.84

Table D.2

Time-Average CO Conversions for Cycling with 90° N_2O Phase-Lead

Frequency, mHz	Time-Average CO Conversion, percent	Frequency, mHz	Time-Average CO Conversion, percent
0.1000	21.82	4.2553	91.03
0.1250	25.39	4.5000	93.00
0.2500	35.73	4.6511	91.36
0.3333	44.50	5.0000	92.11
0.5000	52.15	5.5000	91.88
0.6667	54.75	6.0000	91.24
1.2500	64.19	6.5000	90.59
1.6667	69.38	7.0000	89.35
1.8182	71.49	7.5000	88.53
1.8182	70.98	8.0000	87.72
2.0000	73.91	8.0000	25.41
2.2222	77.19	9.0000	22.86
2.5000	80.19	10.000	22.42
2.8571	83.63	11.000	20.93
3.3333	87.50	12.000	20.76
3.5000	90.06	14.000	20.53
4.0000	91.44	16.000	20.92
4.0000	90.00	18.000	20.74

Table D.3

Time-Average CO Conversion for Cycling with 270° N_2O Phase-Lead

Frequency, mHz	Time-Average CO Conversion, percent	Frequency, mHz	Time-Average CO Conversion, percent
0.2000	12.39	3.3333	52.07
0.3333	16.18	4.0000	51.19
0.4000	17.20	5.0000	48.39
0.5000	18.64	5.7143	41.96
0.6667	21.40	6.2500	39.81
0.6667	22.01	7.1428	36.87
0.8333	24.16	7.6923	36.66
1.0000	26.80	8.3333	36.06
1.1111	28.68	9.0909	24.94
1.1111	29.25	10.000	23.31
1.2500	31.54	12.500	21.58
1.4285	33.65	14.286	21.43
1.6667	37.08	16.667	20.82
1.8182	39.65	20.000	20.44
2.0000	42.87		

Table D.4

Time-Average CO Conversion for Cycling Frequency of 2 mHz

N_2O Phase Lead, degree	Time-Average CO Conversion, percent	N_2O Phase Lead, degree	Time-Average CO Conversion, percent
10	46.84	190	42.79
20	76.23	200	41.76
30	92.48	210	41.23
40	93.44	220	41.10
50	92.61	230	41.47
60	89.64	240	41.92
70	85.80	250	42.31
80	81.49	260	42.77
90	77.25	270	43.33
100	72.72	280	43.80
110	68.71	290	44.40
120	64.45	300	41.82
130	60.18	310	37.08
140	56.40	320	31.70
150	52.76	330	27.22
160	48.72	340	22.60
170	46.07	350	19.48
180	44.25	360	18.90

Table D.5

Time-Average CO Conversions for Cycling Frequency of 5 mHz

N_2O Phase Lead, degrees	Time-Average CO Conversion, percent	N_2O Phase Lead, degrees	Time-Average CO Conversion, percent
10	21.95	190	77.[illegible]7
20	24.84	200	74.54
25	54.14	210	70.56
30	68.23	220	66.64
35	83.17	230	62.49
40	85.50	240	58.05
45	89.90	250	53.77
50	91.36	260	50.33
60	92.28	270	46.00
70	92.58	280	41.20
80	92.20	290	36.67
90	92.20	300	31.70
100	91.64	310	27.99
110	90.95	320	24.98
120	90.21	330	22.85
130	89.17	335	23.19
140	88.00	340	21.05
150	86.27	345	20.19
160	83.73	350	20.29
170	81.56	355	19.88
180	79.49	359	20.19

Table D.6

Time-Average CO Conversion for Cycling Frequency of 7.04 mHz

N_2O Phase Lead, degrees	Time-Average CO Conversion, percent	N_2O Phase Lead, degrees	Time-Average CO Conversion, percent
10	24.70	160	81.58
20	22.73	170	78.70
30	22.29	180	76.78
40	23.64	190	72.98
45	27.53	200	68.60
50	26.60	210	64.67
50	24.52	220	60.10
55	87.34	230	56.99
60	88.84	240	52.40
60	89.00	250	47.78
65	89.29	260	42.90
70	88.77	270	38.73
70	89.83	280	34.63
80	90.10	290	30.94
90	89.62	300	26.85
100	89.50	310	27.10
110	88.86	320	23.10
120	88.24	330	22.64
130	87.11	340	21.65
140	85.32	350	21.14
150	83.33	359	20.49

Table D.7

Time-Average CO Conversion for Cycling Frequency of 11.11 mHz

N_2O Phase Lead, degrees	Time-Average CO Conversion, percent	N_2O Phase Lead, degrees	Time-Average CO Conversion, percent
5	26.75	180	60.46
10	23.51	180	60.58
20	23.41	185	58.01
30	22.64	190	55.88
40	22.96	190	59.06
50	22.80	195	52.75
60	22.63	200	54.36
70	23.00	205	48.29
80	23.49	210	28.57
90	23.85	215	26.19
100	24.12	220	26.36
110	24.10	220	27.64
120	24.45	230	26.57
130	25.28	240	25.37
140	25.27	250	25.32
150	26.82	260	24.39
150	26.17	270	23.61
155	65.82	280	23.55
160	65.79	290	21.92
160	64.18	300	23.33
165	65.51	310	22.06
170	63.56	320	21.66
170	65.14	340	21.16
175	60.39	350	19.57

Table D.8

Time-Average CO Conversion for Cycling Frequency of 15.15 mHz

N_2O Phase Lead, degrees	Time-Average CO Conversion, percent	N_2O Phase Lead, degrees	Time-Average CO Conversion, percent
10	26.78	160	23.95
20	24.79	170	24.26
30	23.41	180	23.55
40	23.33	190	22.97
50	23.23	200	23.00
60	22.74	210	23.03
70	23.11	220	22.06
80	23.18	230	22.12
90	23.05	240	21.98
100	23.05	260	21.72
110	22.68	280	21.44
120	23.80	300	20.73
130	23.36	320	20.22
140	23.91	340	20.04
150	23.30	359	20.02

APPENDIX E
MATLAB PROGRAMS FOR THE N_2O+CO REACTION FEED CYCLING

The computer programs used for the calculations in chapter 3 are listed in this Appendix. All the programs are written for use with MATLAB version 4.0a. The various programs and their respective applications are as follows.

PHASE.M: Calculates the time-average CO conversion from the experimental data

TRANS1.M: Calculates the steady-state CO conversion and the bifurcation points for the surface-phase transition model

TRANSBF.M: Determines the bifurcation boundaries at a given temperature based on surface-phase transition model

DYNSOL.M: Calculates the time-average CO conversion for variable-phase feed composition cycling. The function N2ODYN.M evaluates the values of the derivatives for the differential equations

```
% Program PHASE.M
% This program calculates the time-average CO conversion
% from the experimental data stored in the file PHA.M
pha
sco=4.99;% percentage CO in the cylinder
sn2o=10.01;% percentage N2O in the cylinder
tt=input('Cycle period in seconds')
nphi=input('Phase lead =  ')
[m n]=size(xx);
for i=1:m
time(i)=xx(i,1)-xx(1,1);
coi(i)=(xx(i,3)*sco)/xx(i,2);
n2oi(i)=(xx(i,4)*sn2o)/xx(i,2);
co2(i)=xx(i,6);
end
coiav=0;
n2oav=0;
for i=1:m
coiav=coiav+coi(i);
n2oav=n2oav+n2oi(i);
end
coiav=coiav/m;
n2oav=n2oav/m;
for i=1:m
co(i)=0;
n2o(i)=0;
end
for i=1:m
if xx(i,8)==0
co(i)=coi(i);
end
if xx(i,9)==0
n2o(i)=n2oi(i);
end,end
plot(time,co,'--',time,co2,'-',time,n2o,':')
xlabel('Time, sec.'),ylabel('% CO2 in reactor')
pause
k=input('start and end cycle for time integration')
% example k=[3 15];
n1=k(1);
n2=k(2);
n11=0;
for i=1:m
if co(i)==0
if co(i+1)>0
n11=n11+1;
end,end
m1=i;
if n11==n1, break, end
end
n22=0;
for i=1:m
```

```
if co(i)==0
if co(i+1)>0
n22=n22+1;
end, end
m2=i;
if n22==n2, break, end
end
co21=0;
co1=0;
for i=m1:(m2-1)
co21=co21+((co2(i)+co2(i+1))/2.)*(time(i+1)-time(i));
co1=co1+((co(i)+co(i+1))/2.)*(time(i+1)-time(i));
end
conv=(co21*100.)/co1;
w=1000./tt;
coiav
n2oav
w
conv
s1=['Phase lead =',num2str(nphi),' w = ',num2str(w)];
s2=['%CO Conversion = 'num2str(conv)];
s3=['Feed %CO = 'num2str(coiav),' %N2O = 'num2str(n2oav)];
v=axis;
vx=v(2)/2;
vy1=0.95*v(4);
vy2=0.91*v(4);
vy3=0.87*v(4);
text(vx,vy1,s1)
text(vx,vy2,s2)
text(vx,vy3,s3)
name=input('Data file name:','s'), % Enter the data file name
vy4=1.02*v(4);
text(0,vy4,name)
```

```
% Program TRANS1.M
% This program calculates the steady-state CO conversion
% and bifurcation points for the phase-transition model
% For the predictions at 520 K use program TRANS2.M
%
% The experimental data files are listed in Appendix C
%a190, b190, c190, % data files for 461 K
%a210, b210, c210, % data files for 480 K
a230, b230, c230, % data files at 499 K
temp=499;% enter the temperature K
v=[0,2.,0,104];% for 461 K use v=[0, 1, 0, 104]
axis(v)
%plot the experimental CO conversion Vs feed CO
plot(y(:,1),y(:,2),'o')
hold
plot(x(:,1),x(:,2),'*')
plot(z(:,1),z(:,2),'+')
%d190, % input data file for the program
%d210
d230
sumerr=0.0;
psi1=5; psi2=100;
%kin=input(' K1,K-1, K2, K3,Nco')
kin=[28000 15.75 118 2000  1.0015];
for itime=1:1
for jtime=1:2
nrtsav=1;
itime
coi1=data(itime,2);
coi2=data(itime,3);
n2oi=data(itime,1);
xbpll=data(itime,4);
xbplh=data(itime,5);
xbphl=data(itime,6);
xbphh=data(itime,7);
numco=data(itime,8);
for j=1:numco
for l=1:6
xxx(j,l)=0.0 ;
end
end
for j=1:numco
for l=1:5
res1(j,l)=0;
end,end
count=0;
xdelta=(coi2-coi1)/numco;
% calculate reactor N2O by solving 4th order equation
for i=1:numco
count=count+1;
cop=coi2-xdelta*(i-1);
n2op=n2oi;
```

```
k1=kin(1)*((temp/499)^0.5);
k1a=kin(2)/cop;
k2=kin(3);
k3=kin(4)/cop;
if jtime==2
k2=kin(3)/psi1;k3=kin(4)/(psi2*cop);
end
f=n2op/cop;
nco=kin(5);
del=k1*(1-f);
k10=f*k1-f*k2*(1+k1a/k3);
gama=k1a+nco*del;
k15=k1*f*(nco-1)+k2*k1a*f/k3;
k14=f*k1*nco-f*k2;
k11=f*(k1-k2);
k16=k11+f*nco*del*k2/k3;
aa=-f*k2*k14*k14/k3;
bb=f*k2*k14*k14/k3-f*gama*k2*k14/k3-k14*k16-k2*k10*k15;
cc=k14*(gama*k2*f/k3+k16)-(gama*k16+del*k14)-k2* ...
(del*k15+k10*(gama-del));
dd=gama*k16+del*k14-gama*del-k2*del*(gama-del);
ee=gama*del;
z=[aa,bb,cc,dd,ee];
zroot=roots(z);
[m,l]=size(zroot);
for ir=1:4
xroot(ir)=0.0;
end
n=0;
%eliminate imaginary and negative real roots
for ir=1:m
if imag(zroot(ir,1))==0.0
if real(zroot(ir,1))>0.
n=n+1;
xroot(n)=zroot(ir,1);
end
end
end
%matrix xxx stores the positive real roots(N2O)
xxx(i,1)=count;
xxx(i,2)=n;
xxx(i,3)=xroot(1);
if n>1
xxx(i,4)=xroot(2);
end
if n>2
xxx(i,5)=xroot(3);
end
if n>3
xxx(i,6)=xroot(4);
end
end
```

```
for i=1:numco
xdelta=(coi2-coi1)/numco;
coi=coi2-xdelta*(i-1);
k1a=kin(2)/coi;
k3=kin(4)/coi;
if jtime==2
k3=kin(3)/(cop*psi2);
end
nroot=0;
for ic=3:5
n2o=xxx(i,ic);
if n2o>0.
f=n2oi/coi;
co=1-f*(1-n2o);
ic=ic-2;
coconv(ic)=(1-co)*100.;
tco(ic)=(k1*co-k2*(1+k1a/k3)*f*n2o)/(k1a+k1*nco*co-k2*f*n2o);
to(ic)=(1-tco(ic))/(1+k3*tco(ic)/(k2*n2o*f));
coco2(ic)=k3*to(ic)*tco(ic)*100;
res5(i,ic)=tco(ic);
res1(i,ic)=coconv(ic);
if tco(ic)>=0.0
if tco(ic)<1.001
if to(ic)>=0.0
if to(ic)<1.001
if (tco(ic)+to(ic))<1.001
if coconv(ic)>=0.
if coconv(ic)<100.
nroot=nroot+1;
end, end, end, end, end, end, end, end, end
% determine the lower and upper bifurcation points
res1(i,4)=coi;
res1(i,5)=nroot;
if jtime==1
res2=res1;
res5a=res5;
end
end
end
xdelta=(coi2-coi1)/numco;
for i=1:numco
coi=res2(i,4);
nroot=res2(i,5);
if nroot==3
if nrtsav==1
xbbpt=coi+xdelta/2;
nrtsav=3;
num1=i;
end,end
if nroot==1
if nrtsav==3
num2=i;
```

```
xlbpt=coi+xdelta/2;
nrtsav=0;
end,end
end
nrtsav=1;
num4=0;
for i=1:numco
coi=res1(i,4);
nroot=res1(i,5);
if nroot==3
if nrtsav==1
num3=i;
nrtsav=3;
xhbpt1=coi+xdelta/2;
end,end
if nroot==1
if nrtsav==3
num4=i;
xlbpt1=coi+xdelta/2;
nrtsav=0;
end, end
end
if num4==0, num4=numco;end
%create the vectors a and b for plotting
for i=1:(num2-1)
a1(i)=res2(i,4);
b1(i)=res2(i,1);
end
for i=num3:(num4-1)
b3(i+1-num3)=res1(i,3);
a3(i+1-num3)=res1(i,4);
if res1(i,3)==0
b3(i+1-num3)=res1(i,1);
end
end
num3a=num2-num3;
for i=1:num3a-1
a2(num3a-i)=res2((num3+i),4);
b2(num3a-i)=res2((num3+i),2);
end
for i=num4:numco
a4(i+1-num4)=res1(i,4);
b4(i+1-num4)=res1(i,1);
end
a=[a1 a2 a3 a4];
b=[b1 b2 b3 b4];
plot(a,b)
ra1=[a1' b1'];
ra2=[a2' b2'];
a5=[a3 a4];
b5=[b3 b4];
ra3=[a5' b5'];
```

```
xlabel('CO % in feed'),ylabel('% CO Conversion')
xlb(itime)=xlbpt;
xhb(itime)=xhbpt1;
% save the data in ASCII files
save res56 res2 /ascii
save res55 res1 /ascii
end
```

```
% Program TRANSBF.M
% This program generates the upper and lower bifuraction
% boundaries at the specified temperature using the
% phase-transition model
temp=input('Temperatere, k'), %Enter the temperature value
for japan=1:50
%Generate 50 points for drawing a smooth curve
sumerr=0.0;
psi1=5; psi2=100;
%kin=input(' K1,K-1, K2, K3,Nco')
kin=[28000 15.75 118 2000 1.0015];
for itime=1:1
for jtime=1:2
nrtsav=1;
coi1=0.0;
coi2=1.0;
n2oi=0.25+0.028*japan;
numco=500;
for j=1:numco
for l=1:6
xxx(j,l)=0.0 ;
end, end
for j=1:numco
for l=1:5
res1(j,l)=0;
end,end
count=0;
xdelta=(coi2-coi1)/numco;
% calculate reactor N2O by solving 4th order equation
for i=1:numco
count=count+1;
cop=coi2-xdelta*(i-1);
n2op=n2oi;
k1=kin(1)*((temp/499)^0.5);
k1a=kin(2)/cop;
k2=kin(3);
k3=kin(4)/cop;
if jtime==2
k2=kin(3)/psi1;k3=kin(4)/(psi2*cop);
end
f=n2op/cop;
nco=kin(5);
del=k1*(1-f);
k10=f*k1-f*k2*(1+k1a/k3);
gama=k1a+nco*del;
k15=k1*f*(nco-1)+k2*k1a*f/k3;
k14=f*k1*nco-f*k2;
k11=f*(k1-k2);
k16=k11+f*nco*del*k2/k3;
aa=-f*k2*k14*k14/k3;
bb=f*k2*k14*k14/k3-f*gama*k2*k14/k3-k14*k16-k2*k10*k15;
cc=k14*(gama*k2*f/k3+k16)-(gama*k16+del*k14)-k2* ...
```

```
(del*k15+k10*(gama-del));
dd=gama*k16+del*k14-gama*del-k2*del*(gama-del);
ee=gama*del;
z=[aa,bb,cc,dd,ee];
zroot=roots(z);
[m,l]=size(zroot);
for ir=1:4
xroot(ir)=0.0;
end
n=0;
%eliminate imaginary and negative real roots
for ir=1:m
if imag(zroot(ir,1))==0.0
if real(zroot(ir,1))>0.
n=n+1;
xroot(n)=zroot(ir,1);
end, end, end
%matrix xxx stores the positive real roots(N2O)
xxx(i,1)=count;
xxx(i,2)=n;
xxx(i,3)=xroot(1);
if n>1
xxx(i,4)=xroot(2);
end
if n>2
xxx(i,5)=xroot(3);
end
if n>3
xxx(i,6)=xroot(4);
end
end
for i=1:numco
xdelta=(coi2-coi1)/numco;
coi=coi2-xdelta*(i-1);
k1a=kin(2)/coi;
k3=kin(4)/coi;
if jtime==2
k3=kin(3)/(cop*psi2);
end
nroot=0;
for ic=3:5
n2o=xxx(i,ic);
if n2o>0.
f=n2oi/coi;
co=1-f*(1-n2o);
ic=ic-2;
coconv(ic)=(1-co)*100.;
tco(ic)=(k1*co-k2*(1+k1a/k3)*f*n2o)/(k1a+k1*nco*co-k2*f*n2o);
to(ic)=(1-tco(ic))/(1+k3*tco(ic)/(k2*n2o*f));
coco2(ic)=k3*to(ic)*tco(ic)*100;
res5(i,ic)=tco(ic);
res1(i,ic)=coconv(ic);
```

```
if tco(ic)>=0.0
if tco(ic)<1.001
if to(ic)>=0.0
if to(ic)<1.001
if (tco(ic)+to(ic))<1.001
if coconv(ic)>=0.
if coconv(ic)<100.
nroot=nroot+1;
end, end, end, end, end, end, end, end, end
% determine the lower and upper bifurcation points
res1(i,4)=coi;
res1(i,5)=nroot;
if jtime==1
res2=res1;
res5a=res5;
end, end, end
xdelta=(coi2-coi1)/numco;
for i=1:numco
coi=res2(i,4);
nroot=res2(i,5);
if nroot==3
if nrtsav==1
xhbpt=coi+xdelta/2;
nrtsav=3;
num1=i;
end,end
if nroot==1
if nrtsav==3
num2=i;
xlbpt=coi+xdelta/2;
nrtsav=0;
end,end, end
nrtsav=1;
num4=0;
for i=1:numco
coi=res1(i,4);
nroot=res1(i,5);
if nroot==3
if nrtsav==1
num3=i;
nrtsav=3;
xhbpt1=coi+xdelta/2;
end,end
if nroot==1
if nrtsav==3
num4=i;
xlbpt1=coi+xdelta/2;
nrtsav=0;
end, end, end
if num4==0, num4=numco;end
bfres(japan,1)=xlbpt;
bfres(japan,2)=xhbpt1;
```

```
bfres(japan,3)=n2oi;
clear a a1 a2 a3 a4 b b1 b2 b3 b4
% save the results in an ASCII file
save bfr230 bfres /ascii
end, end
exit
```

```
%           Program 'DYNSOL.M'
%       This program solves the differential equations for the dynamic
%       system. The equations are defined in a function file'N3ODYN.M'
%       The program furhter calculates the time average CO conversion
%       for the specified number of cycles.
%
%       The program inputs include the model parameter values for the
%       differential equations. N2O phase lead, Cycle period and the
%       number of cycles for integration.
%
global x10 x20
global k1 k11 k2 k3 nco am n2op cop factor
global as k4 k41
diary n2o36
omgin=[ .15 0.25];% omgin contains the values of freque
result=[0 0 0 0 0 0];% stores the results
cosh=0.95; cosl=0.1;% limits of surface CO   phase-transi   model
%Enter the initial conditions for integration.x0
%where x0(1)=CO x0(2)=N2O x0(3)=CO2 xo(4)=surface CO
%x0(5)=surface oxygen  x0(6)=adsorbed CO2
x0=[ 0.003 0.000001 0.000011 0.85 0.000001 0.00009].';
for ii=1:2
n2op=1.2364;
cop=1.2157;
%       define the model parameters as global variables
psi1=1; psi2=1;
psin1=5; psin2=100;%enhancement factors for (1x1) surface-phase
k1=28000;%dimensionless CO adsorption rate parameter
k11=15.75;%dimensionless CO desorption rate parameter
k2in=118;% dimensionless N2O dissociation rate parameter
k3in=2000/cop;% dimensionless surface reaction rate parameter
am=0.4*cop;%ratio of bulk volume to metal surface capacitances
nco=1.0015;% CO self-exclusion factor
factor=n2op/cop;% ratio of feed N2O to feed CO
k4=400;% dimensionless CO2 adsorption rate parameter
k41=12000;% dimensionless CO2 desorption rate parameter
as=0.024; % ratio of bulk volume to alumina surface capacitances
omega=omgin(ii);% cycling frequency mHz
cycle=1000/omega;% cycle period in seconds
angle=90;% N2O phase lead
%num=input('number of cycles for integration')
%num1=input('No. of steps per cycle')
num1=1000.;
num=8;
%       The following values of v, q0, tk and p should be
%       changed depending on experimental conditions
v=2.15*1e-4; % v=volume of the reactor cu.m.
tk=499; % temperature of the reactor K
p=103; % pressure in the reactor kPa
q0=185; %total feed flow to the reactor sccm
tt=(q0*1e-6/60)*(tk/273.15)*(101.325/p)*(cycle/v);
t0=0; tf=tt;nlead=(angle*tt)/360;
```

```
%tol    -accuracy used to calculate desirable error. A very low
%       -value of 'tol' will prevent integration of diff. equations
%       using Runge-Kutta-Fehlberg method
%
tol=0.0001;
%
%       integrate a system of ordinary differential equations using
%       4th and 5th order Runge-Kutta formulas. The differential equations
%       (any number) are listed in a separate function file 'n2odyn.m'
% INPUT:
% t0    - Initial value of t: dimensionless
% tf    - Final value of t: dimensionless
% x0    - Initial value column-vector.
% tol   - The desired accuracy.
%
% The Fehlberg coefficients:
%
alpha = [1/4  3/8  12/13  1  1/2]';
beta  = [ [    1      0      0      0      0   0]/4
          [    3      9      0      0      0   0]/32
          [ 1932  -7200   7296      0      0   0]/2197
          [ 8341 -32832  29440   -845      0   0]/4104
          [-6080  41040 -28352   9295  -5643   0]/20520 ]';
gamma = [ [902880  0  3953664  3855735  -1371249  277020]/7618050
          [ -2090  0    22528    21970    -15048  -27360]/752400 ]';
pow = 1/5;

% Initialization
t = t0;
hmax = (tf - t)/(5);
hmin = (tf - t)/(num*100000);
h0 = (tf - t)/(num1) ;h=h0;
%count=1; %counts the number of cycles
x00=[0 0 0 0 0 0 0 0];
% matrix x00 stores the values at the begining of each
% quadrant for all the cycles.
flag=1;% flag=1 for N2O phase lead of less than 180 degree
% flag=2 for N2O phase lead of greater than 180 degree
% flag=3 for N2O phase lead of 180 degree
if nlead>tt/2
nlead=tt-nlead;
flag=2; end
for i=1:num
sum1=0; sum2=0; sum3=0;
osold=x0(5);% surface oxygen value at the start of the cycle
h=h0;
nk=0;% counts the number of integration steps per cycle
jk=1;
if (nlead==tt/2) | (angle==180)
jk=2; flag=3;end
% perform the integration for each of the four quadrants
for jj=1:jk:4
```

```
if jj==1
x10=0;x20=2;t0=0;tf=nlead;
end
if jj==2
x10=2;x20=2;t0=nlead;tf=tt/2;
end
if jj==3
x10=2;x20=0;t0=tt/2;tf=t0+nlead;
end
if jj==4
x10=0;x20=0;t0=nlead+tt/2;tf=tt;
end
if flag==2
dummy=x20; x20=x10; x10=dummy; end
h=h0;
t=t0;
x00=[x00; i jj x0.'];
x = x0(:);cos=x(4);
if cos>=cosh, iflag=1; end, % iflag=1 for (1x1) phase
if cos<=cosl, iflag=0; end, % iflag=0 for (5x20) phase
f = x*zeros(1,6);
tout0=t0;
xout0=x.';
tau = tol * max(norm(x, 'inf'), 1);
n=0;
rout=[ 0 0];% stores the integration results of time Vs CO2
% for plotting pupose. The number of colums of rout should
% be adjusted to include other results i.e. CO, N2O, surface
% oxygen, surface CO etc.
%
% The main loop
%
  while (t < tf ) & (h >= hmin)
     if t + h > tf, h = tf - t; end
% Check for the surface-phase transition
if iflag==1
if cos<=cosl
iflag=0;
end, end
if iflag==0
if cos>=cosh
iflag=1
end, end
if iflag==1, psi1=1; psi2=1; end
if iflag==0, psi1=psin1; psi2=psin2; end
   k2=k2in/psi1; k3=k3in/psi2;
    n=n+1;
       nk=nk+1;
% Corrections for physically unreasonable values
if x(4)>1, x(4)=1/1.00001; end
if x(4)<0, x(4)=1e-8; end
if x(5)<0, x(5)=1e-8; end
```

```
if x(5)>1, x(5)=1/1.00001; end
if x(1)>2, x(1)=2/1.000001; end
if x(1)<0, x(1)=1e-8; end
if x(2)<0, x(2)=1e-8; end
if x(2)>2, x(2)=2/1.000001; end
if x(3)<0, x(3)=1e-8; end
if x(6)<0, x(6)=1e-8; end
      % Compute the slopes
      temp = n2odyn(t,x);
      f(:,1) = temp(:);
      for j = 1:5
         temp = n2odyn( t+alpha(j)*h, x+h*f*beta(:,j));
         f(:,j+1) = temp(:);
      end

      % Estimate the error and the acceptable error
      delta = norm(h*f*gamma(:,2),'inf');
      tau = tol*max(norm(x,'inf'),1.0);
      temp2=x+h*f*gamma(:,1);
      % Update the solution only if the error is acceptable
      if (delta <= tau)
         t = t + h;
         x = temp2;
   yy=x.';
   sum1=sum1+(t-tout0)*(yy(1)+xout0(1))/2;% Area under CO curve
   sum2=sum2+(t-tout0)*(yy(2)+xout0(2))/2;% Area under N2O curve
   sum3=sum3+(t-tout0)*(yy(3)+xout0(3))/2;% Area under CO2 curve
   tout0=t;
   xout0=yy;
if i==(num-1), zf=[t  yy(3)];
rout=[rout;zf];
end
        x0=x;
        h0=h; cos=x0(4);
      end
      % Update the step size
     if delta ~= 0.0
        h = min(hmax, 0.8*h*(tau/delta)^pow);
     end
   end
   if (t < tf)
      disp('SINGULARITY LIKELY.')
      t
   end
% save the results for plotting the CO2 response curve for
% the second last cycle
if i==(num-1)
if jj==1, save res11 rout /ascii, end
if jj==2, save res12 rout /ascii, end
if jj==3, save res13 rout /ascii, end
if jj==4, save res14 rout /ascii, end
clear rout
```

```
end
        if (t < tf), break, end
        clear  x
    end
        if (t<tf),break,end
        conv(i.1)=i;
        conv(i,2)=(tt-sum1)*100./tt;% conversion based on CO curve
        conv(i,3)=(tt-sum2)*factor*100./tt;% conversion based on N2O curve
        conv(i,4)=sum3*100./tt;% conversion based on CO2 curve
       pctos=(x0(5)-osold)*100./osold;% change in surface oxygen
        conv(i,5)=pctos; conv(i,6)=angle;
        conv(i,:),nk
     x0
        end
    angle
        conv
result=[result
conv];
clear conv
        end
% save the time-average conversions in ASCII file
save res31 result /ascii
diary off
exit


%Function N2ODYN.M
function xdot=n2odyn(t,x)
% This function evaluates the derivatives
global x10 x20 k1 k11 k2 k3 nco am factor k4 k41 as
xdot(1)=x10-x(1)-k1*x(1)*(1-x(4)-x(5))*((1-nco*x(4))/ ...
(1-x(4)))+k11*x(4);
xdot(2)=x20-x(2)-k2*x(2)*(1-x(4)-x(5));
xdot(3)=k3*x(4)*x(5)-x(3)-k4*x(3)*(1-x(6))+k41*x(6);
xdot(4)=am*(k1*x(1).*(1-x(4)-x(5))*((1-nco*x(4))/ ...
(1-x(4)))-k11*x(4)-k3*x(4).*x(5));
xdot(5)=am*(factor*k2*x(2).*(1-x(4)-x(5))-k3*x(4).*x(5));
xdot(6)=as*(k4*x(3)*(1-x(6))-k41*x(6));
```

APPENDIX F
STEADY-STATE EXPERIMENTAL DATA FOR THE NO+CO REACTION

This appendix contains the experimental data for the NO+CO reaction steady-state multiplicity described in Chapter 4. All of the experiments were carried out with a constant feed flow of 185 cm^3 (STP)/min and at a constant reactor pressure of 103 kPa. A total of 26 sets of experiments were carried out in the temperature range of 465-520 K. Each set of experiments was carried out at a constant reactor temperature by holding the feed concentration of one of the reactants (NO or CO) constant while varying (stepwise increasing or decreasing) the feed concentration of the other reactant. The results for each set of experiments are listed in the individual tables. A time of 4 hours was allowed to reach the steady-state, however, it was observed that the steady-state was not achieved in 4 hours immediately following the high-to-low or low-to-high conversion bifurcation. The data for the experiments where the steady-state was not reached in 4 hours, is also tabulated with asterisk marks but was not used in the figures in Chapter 4. The conversions and the selectivity are calculated as follows.

F.1 CO conversion:

$$CO\ Conversion = \frac{Reactor\ CO_2\%}{Feed\ CO\%} \times 100\ \% \qquad (F.1)$$

F.2 NO Conversion:

It is possible to calculate the feed NO% from the reactor compositions. Therefore, the NO conversion was calculated by two different methods as follows.

$$NO\ Conversion = \frac{(Feed\ NO\% - Reactor\ NO\%)}{Feed\ NO\%} \times 100\% \quad (F.2)$$

$$NO\ Conversion = \frac{(Feed\ NO\% - Reactor\ NO\%)}{Calculated\ Reactor\ NO\%} \times 100\% \quad (F.3)$$

where

$$Calculated\ Feed\ NO\% = Reactor\ NO\% + Reactor\ N_2O\% + Reactor\ CO_2\% \quad (F.4)$$

Equation (F.4) can be easily derived from equation (4.) of Chapter 4. It was found that the NO conversion calculated by these two methods matched within 5%. The NO conversions listed in the tables is the average of the two values.

F.3 N_2O Selectivity:

$$N_2O\ Selectivity = \frac{2 \times Reactor\ N_2O\%}{(Feed\ NO\% - Reactor\ NO\%)} \quad (F.5)$$

In the above equation the feed NO% is the average value of the measured feed NO% and the calculated feed NO% from equation (F.4)

Table F.1

NO+CO High-Conversion Steady-State Data at 465 K with 0.45% NO in the Feed

Feed CO%	Reactor N_2O%	Reactor NO%	CO Conversion	NO Conversion	N_2O Selectivity
0.0839	0.0448	0.2964	100.00	32.20	0.6348
0.1356	0.0775	0.2270	100.00	48.99	0.7108
0.1867	0.1051	0.1556	100.00	65.32	0.7172
0.2373	0.1248	0.0875	100.00	80.55	0.6889
0.2662	0.1289	0.0540	100.00	87.99	0.6518
0.2962	0.1242	0.0319	100.00	92.93	0.5925
0.3267	0.1126	0.0212	100.00	95.34	0.5188
0.3554	0.1102	0.0277	100.00	94.11	0.4965
0.3864*	0.0319	0.3100	20.28	28.67	0.5098
0.4165*	0.0178	0.3425	11.61	20.04	0.4100
0.4458	0.0111	0.3583	7.96	15.94	0.3210
0.6563	0.0090	0.3627	4.97	14.84	0.2793

(* indicates steady-state was not reached)

Table F.2

NO+CO Low-conversion Steady-State Data at 465 K with 0.45% NO in the Feed

Feed CO%	Reactor N_2O%	Reactor NO%	CO Conversion	NO Conversion	N_2O Selectivity
0.9656	0.0164	0.3533	4.00	17.48	0.4324
0.8640	0.0123	0.3632	3.25	14.65	0.3868
0.7638	0.0105	0.3670	3.25	13.61	0.3550
0.6656	0.0093	0.3698	3.32	12.82	0.3333
0.5641	0.0100	0.3688	4.13	13.16	0.3493
0.4865	0.0142	0.3636	7.65	15.79	0.4122
0.3866	0.0147	0.3637	9.47	15.77	0.4273
0.3361	0.0146	0.3631	11.10	15.91	0.4207
0.2865	0.0151	0.3634	12.74	15.84	0.4370
0.2371	0.0159	0.3623	15.52	16.09	0.4530
0.1865	0.0183	0.3582	20.64	17.04	0.4926
0.1555	0.0207	0.3536	26.17	18.11	0.5247
0.1254	0.0277	0.3381	39.23	21.70	0.5869
0.0958	0.0646	0.2300	100.00	44.99	0.6793
0.0664	0.0504	0.2702	100.00	35.07	0.6797

Table F.3

NO+CO High-Conversion Steady-State Data at 465 K with 0.7% NO in the Feed

Feed CO%	Reactor N_2O%	Reactor NO%	CO Conversion	NO Conversion	N_2O Selectivity
0.0744	0.0471	0.5397	100.00	20.64	0.6686
0.1359	0.0885	0.4379	100.00	35.66	0.7276
0.2366	0.1531	0.2993	100.00	56.90	0.7748
0.3351	0.2024	0.1597	100.00	77.14	0.7512
0.4358	0.2111	0.0607	100.00	91.38	0.6565
0.4666	0.2066	0.0643	100.00	91.05	0.6314
0.4965*	0.0527	0.5036	19.59	25.50	0.6086
0.5262	0.0327	0.5625	11.25	16.84	0.5702
0.5563	0.0237	0.5893	7.78	13.01	0.5335
0.5657	0.0188	0.6044	6.15	10.90	0.5040
0.5965	0.0149	0.6153	4.90	9.39	0.4627
0.6266	0.0139	0.6227	4.44	8.66	0.4672

(* indicates steady-state was not reached)

Table F.4

NO+CO Low-Conversion Steady-State Data at 465 K with 0.7% NO in the Feed

Feed CO%	Reactor N_2O %	Reactor NO%	CO Conversion	NO Conversion	N_2O Selectivity
0.9657	0.0274	0.5885	5.25	13.82	0.5781
0.8650	0.0214	0.6058	4.52	11.27	0.5533
0.7639	0.0182	0.6160	4.54	9.95	0.5318
0.6669	0.0167	0.6158	4.99	9.77	0.4978
0.5648	0.0178	0.6109	6.04	10.28	0.5050
0.4958	0.0203	0.5961	10.95	12.98	0.4549
0.4048	0.0193	0.5981	12.62	12.54	0.4481
0.3732	0.0162	0.6054	12.11	11.36	0.4154
0.3434	0.0139	0.6155	12.29	10.21	0.3954
0.3117	0.0126	0.6160	12.58	9.88	0.3711
0.2817	0.0119	0.6190	12.78	9.38	0.3693
0.2512	0.0116	0.6198	14.49	9.32	0.3619
0.2207	0.0118	0.6206	16.27	9.24	0.3714
0.1993	0.0121	0.6198	17.31	9.22	0.3817
0.1703	0.0135	0.6176	20.73	9.55	0.4116
0.1398	0.0181	0.6043	30.62	11.41	0.4623
0.1083*	0.0606	0.4689	100.0	29.75	0.6060

(* indicates steady-state was not reached)

Table F.5

NO+CO High-Conversion Steady-State Data at 465 K with 1.2% NO in the Feed

Feed CO%	Reactor N_2O%	Reactor NO%	CO Conversion	NO Conversion	N_2O Selectivity
0.0833	0.0578	1.0557	100.00	11.91	0.8101
0.1862	0.1313	0.8853	100.00	26.31	0.8307
0.2858	0.2040	0.7118	100.00	40.72	0.8344
0.3858	0.2707	0.5280	100.00	55.71	0.8151
0.4164	0.2904	0.4726	100.00	60.27	0.8099
0.4465	0.3079	0.4269	100.00	64.14	0.8063
0.4768	0.3238	0.3893	100.00	67.42	0.8038
0.5071	0.3357	0.3502	100.00	70.73	0.7933
0.5363	0.3493	0.3181	100.00	73.53	0.7905
0.5663	0.3598	0.2874	100.00	76.18	0.7827
0.5962	0.3649	0.2645	100.00	78.19	0.7696
0.6256	0.3514	0.2954	100.00	76.08	0.7470
0.6566*	0.0689	1.0099	18.11	15.76	0.7293
0.6862	0.0507	1.0543	12.47	11.79	0.7191
0.7161	0.0507	1.0543	11.91	11.78	0.7199

(* indicates steady-state was not reached)

Table F.6

NO+CO Low-Conversion Steady-State Data at 465 K with 1.2% NO in the Feed

Feed CO%	Reactor N_2O%	Reactor NO%	CO Conversion	NO Conversion	N_2O Selectivity
1.1656	0.0554	1.0524	7.91	12.30	0.7507
1.0649	0.0388	1.0927	5.88	8.72	0.7437
0.9646	0.0311	1.1125	5.09	7.01	0.7418
0.8644	0.0265	1.1343	5.06	5.65	0.7800
0.7646	0.0235	1.1393	5.18	5.15	0.7593
0.6664	0.0218	1.1427	5.36	4.78	0.7596
0.5662	0.0209	1.1416	5.79	4.68	0.7458
0.4860	0.0352	1.1042	11.01	7.71	0.7631
0.4361	0.0328	1.1081	11.42	7.30	0.7519
0.3959	0.0310	1.1107	12.02	7.03	0.7385
0.3659	0.0293	1.1134	12.27	6.73	0.7289
0.3355	0.0279	1.1166	12.55	6.42	0.7275
0.3062	0.0267	1.1195	13.16	6.18	0.7241
0.2756	0.0263	1.1205	14.08	6.06	0.7275
0.2448	0.0268	1.1183	16.38	6.23	0.7214
0.2164	0.0270	1.1187	18.85	6.24	0.7243
0.1967	0.0283	1.1072	21.50	6.86	0.6928
0.1664	0.0320	1.1027	28.31	7.40	0.7256
0.1348	0.0432	1.0780	46.36	9.55	0.7589
0.1046*	0.0729	1.0072	100.00	15.52	0.7874

(* indicates steady-state was not reached)

Table F.7

NO+CO High-Conversion Steady-State Data at 485 K with 0.45% NO in the Feed

Feed CO%	Reactor N_2O%	Reactor NO%	CO Conversion	NO Conversion	N_2O Selectivity
0.0582	0.0320	0.3285	100.00	24.27	0.6046
0.1369	0.0804	0.2232	100.00	49.87	0.7242
0.2068	0.1133	0.1281	100.00	71.48	0.7059
0.2565	0.1253	0.0671	100.00	85.07	0.6554
0.2860	0.1234	0.0385	100.00	91.42	0.6013
0.3153	0.1114	0.0201	100.00	95.52	0.5202
0.3456	0.0917	0.0112	100.00	97.51	0.4187
0.3761	0.0715	0.0075	100.00	98.34	0.3213
0.3963	0.0558	0.0066	100.00	98.55	0.2492
0.4263	0.0476	0.0096	100.00	97.94	0.2082
0.4562*	0.0487	0.2597	27.72	41.28	0.5330

(* indicates steady-state was not reached)

Table F.8

NO+CO Low-Conversion Steady-State Data at 485 K with 0.45% NO in the Feed

Feed CO%	Reactor N_2O%	Reactor NO%	CO Conversion	NO Conversion	N_2O Selectivity
0.9858	0.0202	0.3522	4.45	18.57	0.4991
0.8863	0.0175	0.3585	4.24	16.83	0.4775
0.7856	0.0153	0.3635	4.06	15.36	0.4577
0.6861	0.0142	0.3650	4.21	14.73	0.4434
0.5852	0.0134	0.3673	4.56	14.11	0.4365
0.4866	0.0130	0.3678	5.24	13.87	0.4308
0.3852	0.0134	0.3668	6.62	14.04	0.4390
0.2851	0.0147	0.3642	8.91	14.49	0.4670
0.2360	0.0155	0.3633	11.10	14.78	0.4829
0.1858	0.0170	0.3607	16.36	15.73	0.4974
0.1564	0.0182	0.3569	20.27	16.48	0.5091
0.1251	0.0236	0.3432	32.45	19.75	0.5520
0.0964	0.0784	0.1845	100.00	53.83	0.7122
0.0677	0.0534	0.2795	100.00	34.06	0.7325
0.0463	0.0341	0.3283	100.00	23.36	0.6749

Table F.9

NO+CO High-Conversion Steady-State Data at 485 K with 0.7% NO in the Feed

Feed CO%	Reactor N_2O%	Reactor NO%	CO Conversion	NO Conversion	N_2O Selectivity
0.0764	0.0349	0.5454	100.00	19.52	0.5250
0.1356	0.0736	0.4449	100.00	34.21	0.6341
0.1956	0.1099	0.3644	100.00	46.77	0.6857
0.2554	0.1433	0.2829	100.00	59.04	0.7026
0.3156	0.1727	0.1989	100.00	71.32	0.6982
0.3754	0.1888	0.1217	100.00	82.44	0.6610
0.4361	0.1858	0.0591	100.00	91.44	0.5885
0.4963	0.1576	0.0245	100.00	96.44	0.4742
0.5262	0.1359	0.0159	100.00	97.69	0.4038
0.5558	0.1092	0.0114	100.00	98.34	0.3227
0.5858	0.0930	0.0102	100.00	98.53	0.2718
0.6152	0.1052	0.0232	100.00	96.78	0.3012
0.6455*	0.0408	0.4891	19.89	27.91	0.4293
0.6758*	0.0199	0.5617	11.04	17.08	0.3419
0.6962	0.0111	0.5908	7.66	12.71	0.2558
0.7258	0.0065	0.6083	5.64	10.16	0.1869

(* indicates steady-state was not reached)

Table F.10

NO+CO Low-Conversion Steady-State Data at 485 K with 0.7% NO in the Feed

Feed CO%	Reactor N_2O%	Reactor NO%	CO Conversion	NO Conversion	N_2O Selectivity
1.0617*	0.0365	0.5620	8.44	19.02	0.5529
0.9861	0.0226	0.6073	5.66	12.34	0.5283
0.9046	0.0179	0.6221	4.93	10.13	0.5101
0.8021	0.0144	0.6354	4.45	8.27	0.5023
0.6995	0.0135	0.6351	4.96	8.16	0.4775
0.5979	0.0126	0.6342	5.18	7.92	0.4607
0.4952	0.0125	0.6349	6.09	7.80	0.4641
0.3934	0.0131	0.6334	7.44	7.89	0.4809
0.2916	0.0144	0.6289	10.63	8.45	0.4944
0.2399	0.0153	0.6277	13.28	8.66	0.5123
0.2094	0.0164	0.6226	[illegible]	9.33	0.5098
0.1797	0.0177	0.6184	20.[illegible]	9.86	0.5215
0.1485	0.0210	0.6070	[illegible]	11.30	0.5408
0.1204	0.0903	0.3705	100.00	41.66	0.6686
0.0884	0.0740	0.4433	100.00	31.74	0.7063
0.0574	0.0460	0.5397	100.00	19.49	0.6978

(* indicates steady-state was not reached)

Table F.11

NO+CO High-Conversion Steady-State Data at 485 K with 1.2% NO in the Feed

Feed CO%	Reactor N_2O%	Reactor NO%	CO Conversion	NO Conversion	N_2O Selectivity
0.0862	0.0621	1.0545	100.00	12.23	0.8455
0.1860	0.1340	0.8938	100.00	25.94	0.8560
0.2868	0.2036	0.7222	100.00	40.13	0.8411
0.3860	0.2697	0.5364	100.00	55.15	0.8177
0.4859	0.3254	0.3816	100.00	68.11	0.7987
0.5366	0.3474	0.3172	100.00	73.58	0.7863
0.5859	0.3619	0.2566	100.00	78.66	0.7654
0.6159	0.3692	0.2191	100.00	81.77	0.7512
0.6460	0.3733	0.1852	100.00	84.60	0.7341
0.6769	0.3754	0.1555	100.00	87.08	0.7161
0.6958	0.3745	0.1364	100.00	88.66	0.7020
0.7255	0.3698	0.1131	100.00	90.61	0.6778
0.7565	0.3632	0.0980	100.00	91.89	0.6539
0.7858	0.3601	0.0982	100.00	91.96	0.6408
0.8157*	0.0694	1.0181	17.37	16.16	0.7063
0.8465	0.0434	1.0898	10.11	9.88	0.7258
0.8765	0.0319	1.1227	7.01	7.06	0.7479
0.9067	0.0320	1.0889	7.01	8.66	0.6193

(* indicates steady-state was not reached)

Table F.12

NO+CO Low-Conversion Steady-State Data at 485 K with 1.2% NO in the Feed

Feed CO%	Reactor N_2O%	Reactor NO%	CO Conversion	NO Conversion	N_2O Selectivity
1.0560	0.0120	1.0173	11.40	13.37	0.1523
1.0060	0.0076	1.0716	7.41	8.91	0.1444
0.9459	0.0060	1.0915	6.12	7.29	0.1392
0.8863	0.0048	1.1053	5.24	6.16	0.1316
0.7857	0.0219	1.1229	5.37	5.91	0.6204
0.6866	0.0196	1.1181	5.24	5.78	0.5702
0.5855	0.0196	1.1178	5.93	5.74	0.5744
0.4864	0.0196	1.1177	6.91	5.70	0.5786
0.3857	0.0210	1.1143	9.07	5.96	0.5928
0.2855	0.0241	1.1116	14.12	6.42	0.6309
0.2358	0.0264	1.1045	19.08	7.02	0.6327
0.2061	0.0268	1.1018	21.45	7.12	0.6336
0.1749	0.0320	1.0883	30.36	8.28	0.6504
0.1460	0.1040	0.8886	100.00	23.95	0.7410
0.1144	0.1026	0.8943	100.00	22.50	0.7852
0.0861	0.0798	0.9687	100.00	16.95	0.8036

Table F.13

NO+CO High-Conversion Steady-state Data at 505 K with 0.45% NO in the Feed

Feed CO%	Reactor N_2O%	Reactor NO%	CO Conversion	NO Conversion	N_2O Selectivity
0.0840	0.0474	0.3002	100.00	32.59	0.6743
0.1864	0.1016	0.1658	100.00	63.71	0.7102
0.2858	0.1236	0.0434	100.00	90.49	0.6059
0.3164	0.1128	0.0222	100.00	95.13	0.5265
0.3454	0.0950	0.0104	100.00	97.72	0.4318
0.3762	0.0692	0.0057	100.00	98.75	0.3111
0.3961	0.0516	0.0043	100.00	99.06	0.2310
0.4258	0.0280	0.0034	100.00	99.26	0.1244
0.4555	0.0063	0.0034	100.00	99.26	0.0277
0.4960*	0.0067	0.0074	86.74	98.35	0.0304
0.5060*	0.0815	0.3308	16.28	29.81	1.1500

(* indicates steady-state was not reached)

Table F.14

NO+CO Low-Conversion Steady-State Data at 505 K with 0.45% NO in the Feed

Feed CO%	Reactor N_2O%	Reactor NO%	CO Conversion	NO Conversion	N_2O Selectivity
1.0256	0.0185	0.3506	4.87	19.21	0.4410
0.9858	0.0151	0.3588	4.06	16.79	0.4128
0.8857	0.0135	0.3632	3.99	15.57	0.3982
0.7856	0.0125	0.3696	4.23	14.44	0.3965
0.6853	0.0114	0.3662	4.26	14.30	0.3666
0.5857	0.0112	0.3662	4.64	14.06	0.3666
0.4856	0.0115	0.3653	5.56	14.18	0.3734
0.3859	0.0125	0.3626	7.54	14.86	0.3876
0.2864	0.0145	0.3580	11.24	15.99	0.4182
0.2356	0.0161	0.3534	15.03	17.09	0.4348
0.2051	0.0173	0.3502	18.48	17.90	0.4464
0.1753	0.0191	0.3459	22.82	18.86	[illegible]
0.1460	0.0292	0.3174	41.30	25.73	0.5259
0.1139	0.0812	0.1741	100.00	57.08	0.6896
0.0853	0.0553	0.2711	100.00	36.95	0.6923
0.0571	0.0315	0.3285	100.00	24.12	0.5997

Table F.15

NO+CO High-Conversion Steady-State Data at 505 K with 0.7% NO in the Feed

Feed CO%	Reactor N_2O%	Reactor NO%	CO Conversion	NO Conversion	N_2O Selectivity
0.0864	0.0492	0.5319	100.00	22.16	0.6480
0.1857	0.1114	0.3759	100.00	45.22	0.7173
0.2874	0.1628	0.2407	100.00	65.39	0.7160
0.3856	0.1925	0.1081	100.00	84.40	0.6581
0.4365	0.1898	0.0560	100.00	91.90	0.5977
0.4662	0.1786	0.0348	100.00	94.95	0.5453
0.4964	0.1584	0.0206	100.00	97.00	0.4749
0.5267	0.1358	0.0122	100.00	98.22	0.4023
0.5572	0.1098	0.0075	100.00	98.91	0.3231
0.5864	0.0853	0.0051	100.00	99.26	0.2497
0.6160	0.0584	0.0044	100.00	99.36	0.1705
0.6468	0.0391	0.0050	100.00	99.28	0.1133
0.6763	0.0423	0.0114	100.00	98.40	0.1202
0.6972*	0.0548	0.4562	22.80	33.37	0.4790
0.7465	0.0213	0.5864	7.68	14.02	0.4433

(* indicates steady-state was not reached)

Table F.16

NO+CO Low-Conversion Steady-State Data at 505 K with 0.7% NO in the Feed

Feed CO%	Reactor N_2O%	Reactor NO%	CO Conversion	NO Conversion	N_2O Selectivity
0.9458	0.0200	0.5741	7.44	15.79	0.3699
0.8855	0.0148	0.5972	5.54	12.18	0.3551
0.7856	0.0128	0.6059	5.14	10.76	0.3476
0.6864	0.0119	0.6108	5.16	9.97	0.3487
0.5858	.0114	0.6144	5.58	9.46	0.3516
0.4855	.0119	0.6133	6.47	9.49	0.3662
0.3858	0.0131	0.6096	8.48	9.95	0.3847
0.2860	0.0160	0.5999	12.90	11.20	0.4183
0.2360	0.0179	0.5938	16.[illegible]9	1[illegible]	0.4361
0.2062	0.0215	0.5934	24.15	[illegible]	0.4834
0.1752	0.0244	0.5822	31.28	[illegible]0	0.4954
0.1467	0.1123	0.3082	100.00	50.82	0.6902
0.1147	0.0785	0.4344	100.00	34.36	0.6844
0.0864	0.0523	0.5184	100.00	23.53	0.6531
0.0578	0.0304	0.5867	100.00	14.63	0.6035

Table F.17

NO+CO High-Conversion Steady-state Data at 505 K with 1.2% NO in the Feed

Feed CO%	Reactor N_2O%	Reactor NO%	CO Conversion	NO Conversion	N_2O Selectivity
0.0860	0.0578	1.0399	100.00	12.75	0.7608
0.1834	0.1219	0.8878	100.00	25.80	0.7896
0.2858	0.1877	0.7186	100.00	39.92	0.7863
0.3863	0.2466	0.5400	100.00	54.48	0.7629
0.4858	0.2977	0.3864	100.00	67.39	0.7456
0.5856	0.3331	0.2657	100.00	77.71	0.7191
0.6861	0.3469	0.1498	100.00	87.43	0.6661
0.7366	0.3392	0.1020	100.00	91.42	0.6242
0.7856	0.3225	0.0632	100.00	94.67	0.5746
0.8156	0.3083	0.0476	100.00	95.99	0.5418
0.8464	0.2840	0.0365	100.00	96.92	0.4952
0.8765	0.2665	0.0278	100.00	97.65	0.4604
0.9054	0.2439	0.0223	100.00	98.12	0.4193
0.9359	0.2209	0.0197	100.00	98.34	0.3781
0.9659	0.2086	0.0239	100.00	98.01	0.3550
0.9956*	0.0614	0.9792	16.57	18.50	0.5492
1.0257	0.0353	1.0593	9.04	11.25	0.5255
1.0558	0.0258	1.0888	6.24	8.52	0.5086
1.0771	0.0206	1.1049	4.68	6.98	0.4961

(* indicates steady-state was not reached)

Table F.18

NO+CO Low-Conversion Steady-State Data at 505 K with 1.2% NO in the Feed

Feed CO%	Reactor N_2O%	Reactor NO%	CO Conversion	NO Conversion	N_2O Selectivity
1.0258	0.0416	1.0446	10.59	12.76	0.5445
0.9862	0.0310	1.0777	8.12	9.77	0.5313
0.8860	0.0259	1.0953	7.28	8.17	0.5310
0.7856	0.0239	1.1032	7.33	7.47	0.5362
0.6860	0.0230	1.1118	7.99	6.95	0.5542
0.5854	0.0217	1.1155	8.49	6.53	0.5568
0.4854	0.0231	1.1152	10.65	6.68	0.5789
0.3859	0.0266	1.1015	14.59	7.60	0.5865
0.2852	0.0328	1.0822	23.81	9.16	0.6005
0.2555	0.0373	1.0670	30.96	10.46	0.5982
0.2259	0.0430	1.0513	40.11	11.83	0.6093
0.1954	0.1748	0.6557	100.00	40.72	0.7646
0.1670	0.1262	0.8499	100.00	27.41	0.7847
0.1353	0.0952	0.9460	100.00	20.38	0.7860
0.1072	0.0697	1.0151	100.00	15.12	0.7706
0.0759	0.0468	1.0693	100.00	10.59	0.7388

Table F.19

NO+CO High-Conversion Steady-State Data at 520 K with 0.235% CO in the Feed

Feed NO%	Reactor N_2O%	Reactor NO%	CO Conversion	NO Conversion	N_2O Selectivity
1.9010	0.1508	1.4589	100.00	22.09	0.7286
1.8006	0.1489	1.3647	100.00	23.08	0.7265
1.7004	0.1480	1.2788	100.00	23.92	0.7358
1.6007	0.1475	1.1910	100.00	24.95	0.7448
1.4998	0.1459	1.1006	100.00	26.16	0.7481
1.3999	0.1452	1.0069	100.00	27.[illegible]4	0.7512
1.3006	0.1426	0.9115	100.00	29.60	0.7440
1.2000	0.1423	0.8120	100.00	32.03	0.7438
1.0996	0.1379	0.7189	100.00	34.39	0.7320
1.0005	0.1364	0.6115	100.00	38.33	0.7175
0.9001	0.1338	0.5038	100.00	43.15	0.6995
0.7992	0.1316	0.4101	100.00	47.94	0.6966
0.6997	0.1274	0.3324	100.00	52.33	0.6984
0.5987	0.1224	0.2468	100.00	58.96	0.6903
0.4991	0.1157	0.1581	100.00	68.62	0.6691
0.3995	0.1010	0.0707	100.00	82.46	0.6077
0.3490	0.0864	0.0320	100.00	90.89	0.5414
0.2989	0.0584	0.0085	100.00	97.17	0.4001
0.2689	0.0325	0.0025	100.00	99.07	0.2435
0.2388	0.0021	0.0008	100.00	99.66	0.0177
0.2087*	0.0010	0.0005	75.08	99.74	0.0104
0.1787*	0.0001	0.0008	69.30	99.53	0.0012

(* indicates steady-state was not reached)

Table F.20

NO+CO Low-Conversion Steady-State Data at 520 K with 0.235% CO in the Feed

Feed NO%	Reactor N_2O%	Reactor NO%	CO Conversion	NO Conversion	N_2O Selectivity
0.1393	0.0086	0.1062	6.06	20.73	0.6148
0.1887	0.0098	0.1529	6.07	16.27	0.6552
0.2388	0.0109	0.1987	7.18	14.52	0.6426
0.2889	0.0120	0.2440	8.67	13.62	0.6214
0.3390	0.0130	0.2883	9.33	12.88	0.6072
0.3891	0.0141	0.3320	10.04	12.43	0.5953
0.4393	0.0147	0.3755	10.06	11.89	0.5758
0.4890	0.0152	0.4168	10.33	11.71	0.5446
0.5388	0.0169	0.4585	11.88	11.91	0.5401
0.5895	0.0183	0.5102	13.18	11.13	0.5692
0.6898	0.0201	0.6136	14.28	9.54	0.6192
0.7891	0.0241	0.7072	18.10	9.50	0.6488
0.8901	0.0257	0.8058	19.69	8.84	0.6578
0.9904	0.0299	0.8927	23.84	9.32	0.6515
1.0404	0.0316	0.9379	25.67	9.39	0.6502
1.1397	0.0334	1.0192	27.28	9.65	0.6128
1.2405	0.0417	1.0864	35.78	11.40	0.5959
1.3409	0.0457	1.1673	40.28	11.84	0.5822
1.4402	0.1753	0.8337	100.00	37.55	0.6896

Table F.21

NO+CO High-Conversion Steady-State Data at 520 K with 0.31% CO in the Feed

Feed NO%	Reactor N_2O%	Reactor NO%	CO Conversion	NO Conversion	N_2O Selectivity
2.4015	0.2381	1.7039	100.00	26.69	0.7646
2.2013	0.2244	1.5529	100.00	27.53	0.7589
1.9999	0.2155	1.3797	100.00	29.30	0.7524
1.8068	0.2100	1.2192	100.00	31.21	0.7584
1.6002	0.2026	1.0400	100.00	34.01	0.7554
1.3996	0.1982	0.8548	100.00	38.11	0.7529
1.2000	0.1923	0.6656	100.00	43.77	0.7420
1.0003	0.1856	0.4657	100.00	52.50	0.7206
0.7992	0.1748	0.3059	100.00	61.52	0.7149
0.6997	0.1685	0.2218	100.00	68.31	0.7048
0.5989	0.1609	0.1380	100.00	77.15	0.6907
0.4994	0.1372	0.0576	100.00	88.53	0.6173
0.4499	0.1151	0.0274	100.00	93.93	0.5432
0.3998	0.0819	0.0085	100.00	97.88	0.4183
0.3492	0.0379	0.0014	100.00	99.60	0.2180
0.2989	0.0042	0.0003	96.54	99.90	0.0279
0.2488*	0.0039	0.0006	76.86	99.76	0.0318
0.1987*	0.0362	0.0535	33.58	72.73	0.5075
0.1486	0.0198	0.0670	16.04	52.92	0.5240

(* indicates steady-state was not reached)

Table F.22

NO+CO Low-Conversion Steady-State Data at 520 K with 0.31% CO in the Feed

Feed NO%	Reactor N_2O%	Reactor NO%	CO Conversion	NO Conversion	N_2O Selectivity
0.1393	0.0046	0.1203	2.00	10.94	0.6174
0.1887	0.0057	0.1665	2.65	9.74	0.6315
0.2388	0.0070	0.2112	3.29	9.54	0.6250
0.2889	0.0080	0.2555	3.07	8.99	0.6285
0.3389	0.0089	0.2981	4.93	9.77	0.5477
0.3891	0.0099	0.3401	5.48	9.96	0.5218
0.4393	0.0105	0.3803	5.36	10.04	0.4878
0.4891	0.0114	0.4197	6.48	10.59	0.4[illegible]19
0.5390	0.0122	0.4633	7.10	10.46	0.4410
0.5895	0.0131	0.5154	7.24	9.51	0.4778
0.6897	0.0148	0.6218	8.71	8.07	0.5397
0.7892	0.0163	0.7241	9.62	7.12	0.5862
0.8898	0.0181	0.8214	11.09	6.85	0.5990
0.9904	0.0200	0.9172	12.09	6.64	0.6122
1.0404	0.0206	0.9646	12.27	6.51	0.6129
1.1398	0.0221	1.0436	13.76	7.14	0.5493
1.2400	0.0237	1.1243	15.05	7.61	0.5095
1.3403	0.0244	1.2147	15.25	7.47	0.4947
1.4404	0.0260	1.3003	16.27	7.64	0.4803
1.5409	0.0292	1.3828	18.38	8.06	0.4782
1.6410	0.0324	1.4716	20.86	8.26	0.4864
1.7400	0.0314	1.5745	20.14	7.57	0.4843
1.8406	0.0354	1.6596	23.64	7.99	0.4888
1.9407	0.0377	1.7437	25.38	8.20	0.4812
2.0402	0.0393	1.8337	26.44	8.16	0.4796
2.1409	0.0418	1.9575	27.80	7.35	0.5369
2.2416	0.0432	2.1085	28.76	5.92	0.6510
2.3406	0.0463	2.2103	31.65	5.85	0.6742
2.4407	0.0476	2.2899	33.29	6.18	0.6313

Table F.23

NO+CO High-Conversion Steady-Sate Data at 520 K with 0.435% CO in the Feed

Feed NO%	Reactor N_2O%	Reactor NO%	CO Conversion	NO Conversion	N_2O Selectivity
2.4010	0.3091	1.5625	100.00	33.59	0.7812
2.2013	0.2981	1.3939	100.00	35.57	0.7740
2.0008	0.2928	1.2233	100.00	38.08	0.7781
1.8005	0.2884	1.0417	100.00	41.56	0.7783
1.6007	0.2787	0.8580	100.00	45.90	0.7654
1.4997	0.2794	0.7654	100.00	48.62	0.7715
1.4000	0.2757	0.6662	100.00	52.02	0.7634
1.3008	0.2700	0.5633	100.00	56.14	0.7487
1.2001	0.2627	0.4605	100.00	60.93	0.7311
1.0999	0.2554	0.3789	100.00	65.06	0.7238
1.2000	0.2579	0.4305	100.00	62.90	0.7054
1.0995	0.2487	0.3729	100.00	65.40	0.7054
1.0003	0.2383	0.3043	100.00	69.23	0.6961
0.8995	0.2281	0.2261	100.00	74.72	0.6827
0.7992	0.2118	0.1481	100.00	81.42	0.6527
0.6995	0.1862	0.0748	100.00	89.28	0.5978
0.5988	0.1365	0.0227	100.00	96.19	0.4758
0.4987	0.0569	0.0032	100.00	99.36	0.2305
0.5492	0.0960	0.0090	100.00	98.35	0.3585
0.4987	0.0534	0.0031	100.00	99.37	0.2171
0.4493	0.0095	0.0011	100.00	99.75	0.0426
0.3992	0.0027	0.0015	97.69	99.64	0.0131
0.3490*	0.0068	0.0042	79.24	98.81	0.0391
0.2989*	0.0330	0.1694	22.40	43.41	0.5078
0.2488	0.0187	0.1742	11.25	28.98	0.5258
0.1987	0.0121	0.1487	6.42	23.19	0.5375

(* indicates steady-state was not reached)

Table F.24

NO+CO Low-Conversion Steady-State Data at 520 K with 0.435% CO in the Feed

Feed NO%	Reactor N_2O%	Reactor NO%	CO Conversion	NO Conversion	N_2O Selectivity
0.1480*	0.0220	0.0328	20.88	77.48	0.3859
0.3996	0.0251	0.2942	16.59	24.85	0.4954
0.4987	0.0229	0.3900	14.01	17.69	0.4757
0.5988	0.0239	0.4773	14.12	15.16	0.4623
0.6997	0.0238	0.5913	13.46	12.23	0.4990
0.7993	0.0242	0.6997	13.37	10.53	0.5319
0.9002	0.0250	0.8018	13.56	9.48	0.5483
1.0002	0.0261	0.9147	14.33	8.82	0.6001
1.0998	0.0257	1.0147	13.83	7.80	0.6013
1.2000	0.0264	1.1064	14.03	7.32	0.5833
1.2002	0.0410	1.0294	19.53	10.90	0.5526
1.3504	0.0421	1.1649	19.[illegible]	[illegible]	0.5359
1.5003	0.0428	1.2952	1[illegible]		0.5113
1.6509	0.0424	1.4401			0.5008
1.8008	0.0428	1.5913			0.5065
1.9519	0.0466	1.7264			0.5067
2.1010	0.0460	1.8881			0.5231
2.2520	0.0492	2.1102		[illegible]	0.6736
2.4014	0.0518	2.4014		6.28	1.2870

(* indicates steady-state was not reached)