# **Permutation solvency**

## **Perm solvency: Positivism**

### The kritik throws out the baby with the bathwater – we don’t have to endorse extreme positivism or constructivism – the middle ground is best for science education and epistemology

Kragh 1998 [Helge, professor of history of science, Arhaus University, Denmark, “Social Constructivism, the Gospel of Science, and the Teaching of Physics,” Science & Education, 7, 231-248]

The positivistically coloured conception of science that is still, if only implicitly, the core of most science education is clearly unsatisfactory. The average student of a physics class is taught that physics is a collection of facts about nature, that these can be revealed by following the experimental method, and that consensus about truth is the hallmark of scientific work. The laws of physics are able to predict the outcome of experiments and are, at the same time, technologically useful and a key to unlocking nature's deepest secrets. How the vast body of physical knowledge has come into being is not part of the ordinary physics curriculum, which is also largely silent about the social, philosophical and political aspects of physics. There is surely good reason to broaden the scope of physics education and to present to the students a more realistic picture of how scientists work. Among these are the frequently occurring scientific and technological controversies where experts disagree. According to the received view, disagreement among scientific experts ought not to happen and must be accounted for in terms of individual incompetence or external, hence non-scientific, influence. As pointed out by Collins and Shapin (1983, p. 288), within the positivistic version of science controversies among scientists may therefore generate profound disillusionment about science and thus contribute to anti-science sentiments. Social studies of science has led to much valuable knowledge and in general to a revised and more realistic picture of science. Elements of this knowledge will be relevant for the education of science and ought to enter the curriculum alongside Newton's laws and the properties of elementMy particles. However, it is important to distinguish betwen social or cultural studies of science and the radical versions of constructivism and relativism. As argued by Slezak (1994), modern sociology of scientific knowledge must be wholly rejected and has nothing to offer the science teacher. It is philosophically unsound, has weak empirical support, and is subversive not only to good science education but to honesty and critical thought in general. If the received view of science may cause anti-science sentiments, constructivism is a frontal attack on the entire edifice of science and as such far more damaging. Fortunately the physics teacher does not have to choose between the two extremes. Rejecting constructivism does not mean a rejection of the socio-cultural perspective of science or of specific findings of sociologists of science. Important as it is to avoid the strange blend of relativism and fundamentalism that characterizes modern constructivism, it is equally important not to throw out the baby with the bathwater.

## Perm solvency: philosophy + science

### The perm recognizes the interrelation of scientific knowledge and reality – prefer the perm because it addresses philosophical and politically significant issues in space science

Daley & Frodeman 2008 [Erin Moore, Robert, “Separated at Birth, Signs of Rapprochement: Environmental Ethics and Space Exploration,” *Ethics and the Environment*, 13 (1), Spring, 135-151]

Revolutions in philosophic understanding and cultural worldviews inevitably accompany revolutions in science. As we expand our exploration of the heavens, we will also reflect on the broader human implications of advances in space. Moreover, our appreciation of human impact on Earth systems will expand as we come to see the Earth within the context of the solar system. Most fundamentally, we need to anticipate and wrestle with the epistemological, metaphysical, and theological dimensions of space exploration, including the possibility of extraterrestrial life and the development of the space environment, as it pertains to our common understanding of the universe and of ourselves. Such reflection should be performed by philosophers, metaphysicians, and theologians in regular conversation with the scientists who investigate space and the policy makers that direct the space program. The exploration of the universe is no experimental science, contained and controlled in a laboratory, but takes place in a **vast and dynamic network** of interconnected, interdependent realities. If (environmental) philosophy is to be a significant source of insight, philosophers will need to have a much **broader** range of effective strategies for interdisciplinary collaborations, framing their reflections with the **goal of achieving policy-relevant results**. If it is necessary for science and policy-makers to heed the advice of philosophers, it is equally necessary for **philosophers to speak in concrete terms about real-world problems**. A philosophic questioning about the relatedness of humans and the universe, in collaboration with a pragmatic, interdisciplinary approach to environmental problems, is the **most responsible means of developing both the science and policy for the exploration of the final frontier**.

## **Perm solvency: Contradiction good**

### The alternative tries to achieve transcendence – we should dwell in the contradiction of the affirmative with the kritik

Haraway, 1991 [Donna, historian of science and Professor at the History of

Consciousness Board, University of California, Santa Cruz. She received

her doctorate in biology at Yale, *Simians, Cyborgs, and Women*, pg 187-8 ]

So, I think my problem and 'our' problem is how to have simultaneously an account of radical historical contingency for all knowledge claims and knowing subjects, a critical practice for recognizing our own 'semiotic technologies' for making meanings, and a no-nonsense commitment to faithful accounts of a 'real' world, one that can be partially shared and friendly to earth-wide projects of finite freedom, adequate material abundance, modest meaning in suffering, and limited happiness. Harding calls this necessary multiple desire a need for a successor science project and a postmodern insistence on irreducible difference and radical multiplicity of local knowledges. All components of the desire are paradoxical and dangerous, and their combination is both contradictory and necessary. Feminists don't need a doctrine of objectivity that promises transcendence, a story that loses track of its mediations just where someone might be held responsible for something, and unlimited instrumental power. We don't want a theory of innocent powers to represent the world, where language and bodies both fall into the bliss of organic symbiosis. We also don't want to theorize the world, much less act within it, in terms of Global Systems, but we do need an earth-wide network of connections, including the ability partially to translate knowledges among very different - and power-differentiated - communities. We need the power of modem critical theories of how meanings and bodies get made, not in order to deny meaning and bodies, but in order to live in meanings and bodies that have a chance for a future. Natural, social, and human sciences have always been implicated in hopes like these. Science has been about a search for translation, convertibility, mobility of meanings, and universality - which I call reductionism, when one language (guess whose) must be enforced as the standard for all the translations and conversions. What money does in the exchange orders of capitalism, reductionism does in the powerful mental orders of global sciences: there is finally only one equation. That is the deadly fantasy that feminists and others have identified in some versions of objectivity doctrines in the service of hierarchical and positivist orderings of what can count as knowledge. That is one of the reasons the debates about objectivity falter, metaphorically and otherwise. Immortality and omnipotence are not our goals. But we could use some enforceable, reliable accounts of things not reducible to power moves and agonistic, high status games of rhetoric or to scientistic, positivist arrogance. This point applies whether we are talking about genes, social classes, elementary particles, genders, races, or texts; the point applies to the exact, natural, social, and human sciences, despite the slippery ambiguities of the words objectivity and science as we slide around the discursive terrain. In our efforts to climb the greased pole leading to a usable doctrine of objectivity, I and most other feminists in the objectivity debates have alternatively, or even simultaneously, held on to both ends of the dichotomy, which Harding describes in terms of successor science projects versus postrnodernist accounts of difference and I have sketched in this chapter as radical constructivism versus feminist critical empiricism. It is, of course, hard to climb when you are holding on to both ends of a pole, simultaneously or alternately. It is, therefore, time to switch metaphors.

## **Perm solvency: Materialism good**

### **New scientific knowledge is key to change the world through material struggle**

Haraway, 1991 [Donna, historian of science and Professor at the History of

Consciousness Board, University of California, Santa Cruz. She received

her doctorate in biology at Yale, *Simians, Cyborgs, and Women*, pg 68]

A socialist-feminist science will have to be developed in the process of constructing different lives in interaction with the world. Only material struggle can end the logic of domination. Marx insisted that one must not leap too fast, or one will end in a fantastic utopia, impotent and ignorant. Abundance matters. In fact, abundance is essential to the full discovery and historical possibility of human nature. It matters whether we make ourselves in plenty or in unfulfllied need, including need for genuine knowledge and meaning. But natural history - and its offspring, the biological sciences - has been a discipline based on scarcity. Nature, including human nature, has been theorized and constructed on the basis of scarcity and competition. Moreover, our nature has been theorized and developed through the construction of life science in and for capitalism and patriarchy. That is part of the maintenance of scarcity in the specific form of appropriation of abundance for private and not common good. It is also part of the maintenance of domination in the form of escalating logics and technologies of command-control systems fundamental to patriarchy. To the extent that these practices inform our theorizing of nature, we are still ignorant and must engage in the practice of science. It is a matter for struggle. I do not know what life science would be like if the historical structure of our lives minimized domination. I do know that the history of biology convinces me that basic knowledge would reflect and reproduce the new world, just as it has participated in maintaining an old one.

## **Perm solvency: strategic re-appropriation of science**

### Perm solves best – drawing upon the best science analogically teaches us about epistemology without privileging a master narrative. The alternative’s rejection of science establishes new hegemonies

Code, 2003 [Lorraine, Distinguished Research Professor Emerita of Philosophy at York University in Toronto Canada and a Fellow of the Royal Society of Canada, “What is Natural About Epistemology Naturalized?,” ed. Lynne Hankinson Nelson and Jack Nelson, *Feminist Interpretations of W.V.Quine*, pg 191]

This model is by no means antiscientific, for it must draw on the best available scientific and social scientific evidence to determine how survival can be ensured and enhanced, not just quantitatively, but qualitatively; not by requiring epistemology to “fall into place as a chapter” of ecological science, but by learning, analogically, from the science of ecology. It establishes its (contestable) conception of “best available” evidence in self-critical reflexivity, through which locally, environmentally informed studies of disciplines, their subject matters, and their interdisciplinary relations with one another and within “the world” generate an ongoing skeptical suspicion of presumptions to theoretical hegemony. Although this version of naturalism counts state-of-the-art natural and psychological science among its principal resources, it rejects their claims to joint occupancy of the position of master metanarrative. It is less sanguine than many Quinean naturalists about the before-the-fact reliability of “our” capacities to generalize the relevant features of natural kinds “against the background of the environments in which they operate.”70 For it is wary of the power-infused tendencies of racial/gender/class stereotypes and of essentialized conceptions of “science” and “nature” to take on self-fulfilling, self-perpetuating qualities. Ecology (literally) succeeds only if it is well informed by state-of-theart natural science; yet it fails if it assumes that state-of-the-art science merits uncontested licence to intervene in nature wherever it pleases. Ecology (metaphorically) draws disciplinary conclusions together, maps their interrelations, their impoverishing and mutually sustaining effects within established and putative locations of knowledge-production, and in the social-political-natural world where the effects of institutional knowledge are enacted—for better or worse. The ecological human subject is made by and makes its relations in reciprocity with other subjects and with its (multiple, diverse) environments. Yet this model is not self-evidently benign in the sense of generating a natural, unimpeded unfolding of fully realizable epistemic potential. For ecosystems are as often competitive and as unsentimentally destructive of their less viable members as they are cooperative and mutually sustaining. So if work within the model is to avoid replicating the exclusions endemic to traditional epistemologies, its adherents will have to derive moral-political epistemological guidelines for regulating and adjudicating competing claims for cognitive and epistemic authority.

## Perm solvency: quantum mechanics proves

### Bohr’s quantum physics support our metaphysical and epistemological assumptions and prove that the neg draws false boundaries

Barad, 2003 [Karen, ph.d. in theoretical particle physics, Professor of Feminist Studies, Philosophy, and History of Consciousness at the University of California, Santa Cruz, “Posthumanist Performativity: Toward an Understanding of How Matter Comes to Matter,” Signs: Journal of Women in Culture and Society 2003, vol. 28, no. 3]

Physicist Niels Bohr won the Nobel Prize for his quantum model of the atom, which marks the beginning of his seminal contributions to the development of the quantum theory.17 Bohr’s philosophy-physics (the two were inseparable for him) poses a radical challenge not only to Newtonian physics but also to Cartesian epistemology and its representationalist triadic structure of words, knowers, and things. Crucially, in a stunning reversal of his intellectual forefather’s schema, Bohr rejects the atomistic metaphysics that takes “things” as ontologically basic entities. For Bohr, things do not have inherently determinate boundaries or properties, and words do not have inherently determinate meanings. Bohr also calls into question the related Cartesian belief in the inherent distinction between subject and object, and knower and known. It might be said that the epistemological framework that Bohr develops rejects both the transparency of language and the transparency of measurement; however, even more fundamentally, it rejects the presupposition that language and measurement perform mediating functions. Language does not represent states of affairs, and measurements do not represent measurement-independent states of being. Bohr develops his epistemological framework without giving in to the despair of nihilism or the sticky web of relativism. With brilliance and finesse, Bohr finds a way to hold on to the possibility of objective knowledge while the grand structures of Newtonian physics and representationalism begin to crumble.

### **And, Bohr’s theory of interrelation between subjects and objects is derived from atomic physics**

Barad, 2003 [Karen, ph.d. in theoretical particle physics, Professor of Feminist Studies, Philosophy, and History of Consciousness at the University of California, Santa Cruz, “Posthumanist Performativity: Toward an Understanding of

How Matter Comes to Matter,” Signs: Journal of Women in Culture and Society 2003, vol. 28, no. 3]

Bohr’s break with Newton, Descartes, and Democritus is not based in “mere idle philosophical reflection” but on new empirical findings in the domain of atomic physics that came to light during the first quarter of the twentieth century. Bohr’s struggle to provide a theoretical under standing of these findings resulted in his radical proposal that an entirely new epistemological framework is required. Unfortunately, Bohr does not explore crucial ontological dimensions of his insights but rather focuses on their epistemological import. I have mined his writings for his implicit ontological views and have elaborated on them in the development of an agential realist ontology. In this section, I present a quick overview of important aspects of Bohr’s account and move on to an explication of an agential realist ontology. This relational ontology is the basis for my posthumanist performative account of the production of material bodies. This account refuses the representationalist fixation on “words” and “things” and the problematic of their relationality, advocating instead a causal relationship between specific exclusionary practices embodied as specific material configurations of the world (i.e., discursive practices/(con)figurations rather than “words”) and specific material phenomena (i.e., relations rather than “things”). This causal relationship between the apparatuses of bodily production and the phenomena produced is one of “agential intra-action.” The details follow.

# Science good

## Science self-correcting

### Scientific knowledge production is checked over time to improve a refined view of reality

Kuhn, T.: 1992, 'The Trouble with the Historical Philosophy of Science', Robert and Maurine Distinguished Lecture. Harvard University. Google Books.

Knowledge production is the particular business of the subspecialties, whose practitioners struggle to improve *incrementally* the **accuracy, consistency, breadth of applicability, and simplicity** of the set of beliefs they acquired during their education, their initiation into the practice. It is the beliefs modiﬁed in this process that they transmit to their successors who carry on from there, working with and modifying scientiﬁc knowledge as they go. Occasionally the process runs aground, and the proliferation and reorganization of is usually part of the required remedy. What I am thus suggesting, in an excessively compressed way, is that human practices in general and scientiﬁc practices in particular have evolved over a very long time span, and their development forms very roughly like an evolutionary tree. Some characteristics of the various practices entered early in this evolutionary development and are shared by all human practices. I take power, authority, interest, and other ‘political’ characteristics to he in this early set. With respect to these, scientists are no more immune than anyone else, a fact that need not have occasioned surprise. Other characteristics enter later, at some developmental branching point, and they are thereafter characteristic only of the group of practices formed by further episodes of proliferation among descendants from that branch. The sciences constitute one such group, though their development involved several branch points and much recombination. The characteristics of the members of this group are, in addition to their concern with the study of natural phenomena, the evaluative procedures I’ve already described and others like them. I again have in mind such characteristics as accuracy, consistency, breadth of application, simplicity, and so on—characteristics that are passed, together with illustrations, from one generation of practitioners to the next. These characteristics are somewhat differently understood in the different scientific specialties and subspecialties. And in none of them are they by any means always observed. Nevertheless, in the fields where they have once taken hold, they account for the continuous emergence of more and more refined—and also more specialized—tools for the accurate, consistent, comprehensive, and simple descriptions of nature. Which is only to say that in such fields they’re sufficient to account for the continued development of scientific knowledge. What else is scientific knowledge, and what else would you expect practices characterized by these evaluative tools to produce?

### Scientific knowledge is self-correcting—it changes its beliefs to match the best theories

Kuhn, T.: 1992, 'The Trouble with the Historical Philosophy of Science', Robert and Maurine Distinguished Lecture. Harvard University. Google Books.

Sketching the needed reconceptualization, I’ve indicated three of its main aspects. First, that what scientists produce and evaluate is not belief *tout court* but change of belief, a process which I’ve argued has intrinsic components of circularity, but of a circularity that is not vicious. Second, that what evaluation aims to select is not beliefs that correspond to a so-called real external world, but simply the better or best of the bodies of belief actually present to the evaluators at the time their judgments are reached. The criteria with respect to which evaluation is made are the standard philosopher's set: accuracy, breadth of application, consistency, simplicity, and so on. And, ﬁnally, I've suggested that the plausibility of this view depends upon abandoning the view of science as a single monolithic enterprise, hound by a unique method. Rather, it should be seen as a complex but unsystematic structure of distinct specialties or species, each responsible for a different domain of phenomena, and each dedicated to changing current beliefs about its domain in ways that increase its accuracy and the other standard criteria I've mentioned. For that enterprise, I suggest, the sciences, which must then be viewed as plural, can be seen to retain a very considerable authority.

## Science good: Democracy

### Turn: Social epistemology – autonomous scientific research has epistemic value for the public’s general knowledge

Wilholt 2006 [Torsten, “Scientific Autonomy and Planned Research: The Case of Space Science,” Poiesis and Praxis 4(4), 253-265]

**Our common interest in knowledge evidently surpasses the realm of information that is immediately relevant for political decision. This insight might provide us with a starting-point for a rationale for freedom of research with greater scope.** Beyond as well as within the political sphere, we need knowledge to approach practical problems efficiently. We **also** search for knowledge in order to explain the phenomena of empirical reality and understand our place within it. The sciences can be regarded as the institutionalized endeavor to produce, by means of a cooperative effort, this knowledge that would be inaccessible to the epistemic pursuits of individuals. **All this is well, but it does not yet explain why a principle of freedom of research should apply within the coordinated knowledge-generating enterprise. Could not the cooperative effort be orchestrated by a centralized authority and work equally well (and perhaps better)? A riposte to this question that suggests itself is that** organizing scientific inquiry on the basis of a principle of freedom of research is **simply** the most effective way to find as much relevant knowledge as possible in as quick and reliable manner as possible.I will call this way of justifying freedom of research the argument from social epistemology. **Social epistemology is an emerging subfield of the theory of knowledge, whose core problem is, in the words of Philip Kitcher, “to identify the properties of epistemically well-designed social systems” (1993, 303). Is freedom of research one of these properties? The argument from social epistemology must show that the principle of freedom of research outclasses other possible forms of organizing research – in particular such forms as resort to the external control or planning of research. For this** reason, the fallibility of all attempts to plan and control research has played a central role in many historical instances of the argument from social epistemology. An impressive (and sadly forgotten) example of this is given in a lecture from 1711 by the philosopher and jurist Nicolas Gundling. In this address, much discussed in early 18th century Prussian academia, Gundling called for complete freedom of teaching and research for all four of the universities’ faculties – a shocking conception at the time. (Gundling 1711, cf. Paulsen 1896, 530 f.) To him, this was the only way to increase the stock of known truths, because to purposefully plan a faultless way to new knowledge was in his view impossible.

## **Biology good: Feminism**

Anti-scientific critiques of biology are bad for feminism

Haraway, 1991 [Donna, historian of science and Professor at the History of

Consciousness Board, University of California, Santa Cruz. She received

her doctorate in biology at Yale, *Simians, Cyborgs, and Women*, pg 197-8]

Throughout this reflection on 'objectivity', I have refused to resolve the ambiguities built into referring to science without differentiating its extraordinary range of contexts. Through the insistent ambiguity, I have foregrounded a field of commonalities binding exact, physical, natural, social, political, biological, and human sciences; and I have tied this whole heterogeneous field of academically (and industrially, for example, in publishing, the weapons trade, and pharmaceuticals) institutionalized knowledge production to a meaning of science that insists on its potency in ideological struggles. But, partly in order to give play to both the specificities and the highly permeable boundaries of meanings in discourse on science, I would like to suggest a resolution to one ambiguity. Throughout the field of meanings constituting science, one of the commonalities concerns the status of any object of knowledge and of related claims about the faithfulness of our accounts to a 'real world', no matter how mediated for us and no matter how complex and contradictory these worlds may be. Feminists, and others who have been most active as critics of the sciences and their claims or associated ideologies, have shied away from doctrines of scientific objectivity in part because of the suspicion that an 'object' of knowledge is a passive and inert thing. Accounts of such objects can seem to be either appropriations of a fixed and determined world reduced to resource for the instrumentalist projects of destructive Western societies, or they can be seen as masks for interests, usually dominating interests. For example, 'sex' as an object of biological knowledge appears regularly in the guise of biological determinism, threatening the fragile space for social constructionism and critical theory, with their attendant possibilities for active and transformative intervention, called into being by feminist concepts of gender as socially, historically, and semiotically positioned difference. And yet, to lose authoritative biological accounts of sex, which set up productive tensions with its binary pair, gender, seems to be to lose too much; it seems to be to lose not just analytic power within a particular Western tradition, but the body itself as anything but a blank page for social inscriptions, including those of biological discourse. The same problem of loss attends a radical 'reduction' of the objects of physics or of any other sciences to the ephemera of discursive production and social construction. I I But the difficulty and loss are not necessary. They derive partly from the analytical tradition, deeply indebted to Aristotle and to the transformative history of 'White Capitalist Patriarchy' (how may we name this scandalous Thing?) that turns everything into a resource for appropriation, in which an object of knowledge is finally itself only matter for the seminal power, the act, of the knower. Here, the object both guarantees and refreshes the power 198 of the knower, but any status as agent in the productions of knowledge must be denied the object. It - the world - must, in short, be objectified as thing, not as an agent; it must be matter for the self-formation of the only social being in the productions of knowledge, the human knower. Zoe SofouUs (1988) identified the structure of this mode of knowing in technoscience as 'resourcing' - the second-birthing ofMan through the homogenizing of all the world's body into resource for his perverse projects. Nature is only the raw material of culture, appropriated, preserved, enslaved, exalted, or otherwise made flexible for disposal by culture in the logic of capitalist colonialism. Similarly, sex is only the matter to the act of gender; the productionist logic seems inescapable in traditions of Western binarisms. This analytical and historical narrative logic accounts for my nervousness about the sex/gender distinction in the recent history of feminist theory. Sex is 'resourced' for its re-presentation as gender, which 'we' can control. It has seemed all but impossible to avoid the trap of an appropriationist logic of domination built into the nature/culture binarlsm and its generative lineage, including the sex/gender distinction.

## **AT: Evolutionary biology link**

Evolutionary biology isn’t based on genetic determinism

Kember 2003 [Sarah, Reader in new technologies of communications, Goldsmiths college, London, Cyberfeminism and Artificial Life, pg 192-3]

Without denying that there are distinctions among developmental processes, what the proponents of this theory reject is the hierarchical separation of two kinds of process – internal and external, innate and acquired. There is no autonomy, and no dominance of the gene over the cell, the organism over the environment. The biopolitical relationship which is subsequently outlined is one of interdependence and mutuality. Evolutionary biology is not to be conflated with the biopolitics of autonomy and domination since its central Darwinian principle – natural selection – is itself not genetically determined: ‘Natural selection requires heritable differences in fitness-related attributes. The exact mechanisms of inheritance are not important’ (399). In other words, just because a trait evolves does not mean that it is genetically rather than environmentally determined. In deconstructing the nature/culture divide and the attendant science wars (as exemplified in debates on evolutionary psychology), it is important for Gray to note that ‘the traffic can go both ways’. If there is no autonomous realm of the biological or the social ‘then not only should we be exploring the causal connections between the two, but we should also be exploring potential methodological links. Social scientists who have some enthusiasm for the critique of biological determinism may be less enthusiastic about this’ (404). The methodological link which is implicated here is the already existing, the nascent dialogue which must itself be nature/nurtured given the ease with which any argument can be recuperated and subsumed within oppositional terms. It is important that, especially if there is currently much lip-service being paid to interactionism, feminism and science do not, as Gowaty learned, get ahead of themselves. Getting beyond the sex/gender divide is dependent on a dialogue at, or indeed beyond, the limits of both science and feminism which recognises ‘the ways in which experiential inputs shape physiology and anatomy, and the ways in which physiological and anatomical differences shape behaviour and experience’ (Gray 1997: 406).

## **ALife Good /perm solvency**

The alternative sets up a zero sum game – the perm solves while preventing anti-biologism – prefer our evidence – specific to ALife

Kember 2003 [Sarah, Reader in new technologies of communications, Goldsmiths college, London, Cyberfeminism and Artificial Life, pg 175-177]

This chapter will draw together and develop elements of the feminist critique of alife presented both here and elsewhere. Specifically, it will highlight the constitutive discourses of alife – biology, computer science and cognitive psychology – in order to elaborate the concept of autonomous agency which has been associated with the description and fabrication of alife systems in hardware, software and wetware. These informatic systems are regularly, routinely anthropomorphised and yet denied both meaning and morphology. Their autonomy is not least that mythical autonomy of the technical realm from the contaminating spheres of the social, political and psychological (Keller 1992). My aim then, is to contaminate the notion of autonomy as an ontology, epistemology and ethics of the self in technoscientific culture. In so far as this self can be, and has been designated ‘posthuman’, to what extent does its autonomy signify an era of posthumanism? What is the role of (post)humanism in expanded alife environments, and what is the relationship between feminist and humanist, political and ethical projects? Neither feminism or humanism can be treated as given. Rather, both will be interrogated with respect to their preferred adversaries – biologism, anti-humanism – and indeed with respect to the problem of polarisation which limits debate on all levels to a question of either/or: universalism or relativism; essentialism or constructivism; nature or culture. My argument is that since alife by definition – exemplifies the redundancy of all polarities which lead to nature versus culture, a feminism evolved in alife environments (physical, social, historical, political) must adopt a new stance in relation to this aspect of the new biology. Biology is not reducible to biological essentialism and is arguably insufficiently homogenous to sustain its own claims to universality or counterclaims of social constructivism which rely on the stable division of nature and culture. In its new stance, feminism must relinquish its corner in recognition of the fact that biology, or at least some biology, already has. To extend the metaphor just briefly; this does not mean that the fight is over. There is a great deal of ground to contest even when there are no clearly demarcated territories, and the gendered and racial legacy of territorialism must surely be no great loss. Biology no longer occupies a territory that can consistently and reliably be named ‘nature’, and feminism does not preside over a pure, abstract extrapolation of nurture called ‘culture’. So how should feminists contest the material and metaphoric grounds of human and machine identities, human and machine relations? This question exceeds the boundaries of this book and to an extent those of current feminist thinking on science and technology within which it is situated. This book and the thinking within which it is situated are both part of the problem and potentially part of the solution. To a large extent, the preceding chapters have rehearsed a constructivist argument developed in legitimate opposition not to biology and evolutionary theory per se, but in opposition to attempts to employ them in naturalising and discriminating discourses and practices. To a large extent, ALife appears to rehearse these discourses and practices in its attempt to create virtual worlds and artificial organisms governed purely by Darwinian principles. And yet, fears concerning the association between Darwinism and Social Darwinism may be grounded more in history than in the present. As demonstrated in Chapter 2, ALife is in-formed by ‘a’ biology which is far from unified and in fact highly contested internally. Perhaps the strongest critique of sociobiology comes from within the discipline (Rose et al. 1984; Rose and Appignanesi 1986). Similarly, as demonstrated in Chapter 3, ALife’s foundation upon the hierarchical Platonic distinction of form versus matter is internally contested and ultimately productive. What is produced here is contestation over the terms of embodiment and disembodiment which has served as an invitation to dialogue, and perhaps even – in Donna Haraway’s words – to ‘diffraction’ (Haraway 1997, 2000). Diffraction is an optical metaphor for a slight or sudden change in direction. ALife’s foundational commitment to disembodiment – to kinds of behaviour and not kinds of stuff (Langton 1996 [1989]) – is diffracted by other ALife theorists (Emmeche 1994) and practitioners (Brooks and Flynn 1989; Tenhaaf 1997). My own dialogue with the field is partial in the sense of being invested, located, embodied and incomplete. It arose – organically – from the research process and was nurtured by a growing awareness that diffraction – making a difference – is dependent on, if not synonymous with the internal state of the system. It requires proximity and is not free from contamination. If diffraction is, for Haraway (2000), a contaminated ‘critical consciousness’, I will present dialogue as a critical methodology which entails what, after Isabelle Stengers (1997), might be called ‘risk’. What feminism risks, in a dialogue with alife, is not complicity – complicity as illustrated in Sadie Plant’s (1995, 1996) work bypasses dialogue and is tacit, given – but the complacency of a secure, well-rehearsed oppositional stance. Feminism risks its anti-biologism (and technophobia) by entering a more dynamic relationship based on contest and consent, on centrifugal and centrepetal forces (Bakhtin in Holquist 1981).1 It risks entering a dynamic dialogic relationship which, like the strange attractor in chaos theory, is infinite, complex and will not reach equilibrium or closure.2 There are no clear solutions to the problems raised in a close encounter between feminism and alife – even a very particular ‘cyber’ feminism and an expanded concept of alife – but the encounter itself, as this chapter hopes to show, can make a difference. My project, as has already been suggested, is by no means pristine and is, in a sense, doubly contaminated – not just by the sciences it moves (slightly) closer towards but by the position it started from. I do not mean to trace a linear trajectory from ignorance to enlightenment, not least because accusations of scientific ‘illiteracy’ fly all too often in the face of feminists from scientists entrenched in the science wars and invested primarily in preventing any form of outside intervention.3 What I do mean to trace is the problem of entrenchment and a possible means of averting – in the case of alife – the stalemate of science wars. Dialogue does not entail a one-way movement but a two-way dynamic, and a step into ‘no-man’s’ land for feminism is predicated on the apparent break in biology’s ranks. The unfortunate macho metaphors of combat which suggest themselves in this context signal all too clearly the problem which needs to be addressed. This book is inevitably part of the problem, and where I started from is where I am now but with some hopefully significant differences which developed during a process which I am calling dialogue (not emergence). These differences do not centre so much on greater degrees of assent or dissent between the values of feminism and alife, but on the kinds of complexity opened up through internal contestation and made available to diffraction.

### **A-Life avoids all of their link arguments**

Kember 2003 [Sarah, Reader in new technologies of communications, Goldsmiths college, London, Cyberfeminism and Artificial Life, pg 181-182]

In Life on the Screen, Sherry Turkle (1997) effectively locates the consensual culture of alife through key concepts such as emergence and connectionism. These articulate ‘something that many people want to hear’, and in particular, ‘the non determinism of emergent systems has a special resonance in our time of widespread disaffection with instrumental reason’ (Turkle 1997: 143). ALife’s emphasis on biology rather than physics, bottom-up rather than top-down processing, holism rather than reductionism is ideological rather than purely methodological. It fits, alongside chaos and complexity theory within a zeitgeist identified by Turkle, Emmeche and others as postmodernism and is consonant with ‘a general turn to “softer” epistemologies that emphasise contextual methodologies’ (144). ALife’s ‘constituent agents’ express feelings of fragmentation (theorised within psychoanalysis) while simultaneously decentring and recentring the myth of the autonomous, unitary identity: ‘Today, the recentralisation of emergent discourse in AI is most apparent in how computer agents such as those designed to sort electronic mail or scour the Internet for news are being discussed in popular culture’ (145). Whereas the intelligence of such agents emerges from the function of a distributed system, there is ‘a tendency to anthropomorphise a single agent on whose intelligence the users of the program will come to depend’.

## **Science good: Ecology (bio-d impacts): Perm solves**

### **Only the perm opens space to properly understand the construction of ideas about nature and science—the impact of our better epistemic practices is EXTINCTION**

Code, 2003 [Lorraine, Distinguished Research Professor Emerita of Philosophy at York University in Toronto Canada and a Fellow of the Royal Society of Canada, “What is Natural About Epistemology Naturalized?,” ed. Lynne Hankinson Nelson and Jack Nelson, *Feminist Interpretations of W.V.Quine*, pg 195-6]

The aim of this naturalism will be, amid the instabilities of the postmodern world, to articulate guidelines for adjudicating responsibility claims. Its analyses will focus as closely on how people know one another— both in everyday and in social scientific contexts—as on how they know “facts” about the natural world.82 It will examine social structures of expertise and authority, and intersubjective negotiations that assume—and often presume—to know other persons. Such interactions make knowledge possible; yet they are primary sites of empowerment and disempowerment in patriarchal, racist, and other hierarchically ordered societies. And the knowledge they assume as their starting point is often obscured by stereotyped social identities posing as accurate knowledge of human natural kinds. With its recognition of the inherent sociality of human life, and thus of knowledge construction, as well as its recognition of the constitutive effects of material, social, cognitive, and political circumstances in realizations of subjectivity, an ecological model opens up rhetorical/discursive spaces from which theorists can engage with social and global issues as fully as with questions about the “nature” of knowledge. With its emphasis on the implicit and explicit workings of power within institutions of knowledge-production, the model contests assumptions of pure rationality and the “natural” emergence of truth to work toward understanding the “artefactual” dimensions of reason, knowledge, subjectivity, and nature. Conceived as interactive, finely differentiated, interpretive analyses in which no one can take on all of the pertinent issues, these projects could issue in collaborative enterprises where philosophers would work together, and in concert with other inquirers, for more equitably achieved human survival.

## **Science good: Ecology (bio-d impacts): AT: Coopted**

### Debating ecological impacts scientifically encourages self-reflexivity and isn’t coopted by science

Code, 2003 [Lorraine, Distinguished Research Professor Emerita of Philosophy at York University in Toronto Canada and a Fellow of the Royal Society of Canada, “What is Natural About Epistemology Naturalized?,” ed. Lynne Hankinson Nelson and Jack Nelson, *Feminist Interpretations of W.V.Quine*, pg 193]

They are neither to be repudiated nor transcended in illusory gestures of an impossible self-sufficiency, nor elaborated into a romanticized immersion of self in nature or in Others. Acknowledging the partiality of their knowings and self-knowings, and their potential effects (however small, however local), ecological subjects are well placed to “own” their activities and to be responsible for them.73 The normative possibilities of this model are instrumental, deriving as they do from a hypothetical imperative to the effect that epistemic activity be appraised—in its form and content—according to its success in promoting ecologically sustaining communities, committed to fostering ecological viability within the “natural” world.74 Appraisals of ecological goals and of epistemologies that can promote them and are modeled upon them will proceed in concert, dialectically, integrating epistemology with moral-political-historical-anthropological debate.75 Yet even crossing these boundaries does not turn epistemology into a chapter of ecological science; and single observational S-knows-that-p claims may be neutral in this regard, with no immediate ecological import either way. Consider Ursula Franklin’s “impact studies.”76 During their summer jobs, however menial, however “non-scientific,” engineering students were to record the impact of their work on the immediate environment, an exercise designed to teach them that whatever they did, from house painting, to child minding, to table serving, would produce effects they would not have thought to notice. At issue were not simple empirical claims of the S-knows-that-p variety: Sara knows that paint is messy, that children need good food, that coffee must be served hot. Hence the point is not that such claims take on direct ecological significance. But only by discerning their impact, which extends well beyond one’s first imaginings, can evaluations within a larger ecological network be conducted. Franklin’s purpose was to show that there is no knowledge, and no knowledge-informed practice, that is without consequences, and hence none that should escape critical scrutiny.

## Models good

### Prediction models are key to proper risk analysis

Fitzsimmons 06 Michael Fitzsimmons is a defence analyst in Washington DC. (2006): The Problem of Uncertainty in Strategic Planning, Survival, 48:4, 131-146 http://dx.doi.org/10.1080/00396330601062808

This defence of prediction does not imply that great stakes should be gambled on narrow, singular predictions of the future. On the contrary, the central problem of uncertainty in plan- ning remains that any given prediction may simply be wrong. Preparations for those eventualities must be made. Indeed, in many cases, relatively unlikely outcomes could be enormously consequential, and therefore merit extensive preparation and investment. In order to navigate this complexity, strategists must return to the dis- tinction between uncertainty and risk. While the complexity of the international security environment may make it somewhat resistant to the type of probabilis- tic thinking associated with risk, a risk-oriented approach seems to be the only viable model for national-security strategic planning. The alternative approach, which categorically denies prediction, precludes strategy. As Betts argues, Any assumption that some knowledge, whether intuitive or explicitly formalized, provides guidance about what should be done is a presumption that there is reason to believe the choice will produce a satisfactory outcome – that is, it is a prediction, however rough it may be. If there is no hope of discerning and manipulating causes to produce intended effects, analysts as well as politicians and generals should all quit and go fishing.36 Unless they are willing to quit and go fishing, then, strategists must sharpen their tools of risk assessment. Risk assessment comes in many varieties, but identification of two key parameters is common to all of them: the consequences of a harmful event or condition; and the likelihood of that harmful event or condition occurring. With no perspective on likelihood, a strategist can have no firm perspective on risk. With no firm perspective on risk, strategists cannot purposefully discriminate among alternative choices. Without purposeful choice, there is no strategy.

## AT: Warming models link

### Warming models can simulate climate change

IPCC, 2007, (IPCC assesses the scientific, technical and socio-economic information relevant for the understanding of the risk of human-induced climate change) (The IPCC explains... Reliability of Climate Projection Models) (http://co2now.org/Know-the-Changing-Climate/Scientific-Predictions/ipcc-faq-climate-projection-model-reliability.html)

There is considerable confidence that climate models provide credible quantitative estimates of future climate change, particularly at continental scales and above. This confidence comes from the foundation of the models in accepted physical principles and from their ability to reproduce observed features of current climate and past climate changes. Confidence in model estimates is higher for some climate variables (e.g., temperature) than for others (e.g., precipitation). Over several decades of development, models have consistently provided a robust and unambiguous picture of significant climate warming in response to increasing greenhouse gases. Climate models are mathematical representations of the climate system, expressed as computer codes and run on powerful computers. One source of confidence in models comes from the fact that model fundamentals are based on established physical laws, such as conservation of mass, energy and momentum, along with a wealth of observations. A second source of confidence comes from the ability of models to simulate important aspects of the current climate. Models are routinely and extensively assessed by comparing their simulations with observations of the atmosphere, ocean, cryosphere and land surface. Unprecedented levels of evaluation have taken place over the last decade in the form of organised multi-model ‘intercomparisons’. Models show significant and increasing skill in representing many important mean climate features, such as the large-scale distributions of atmospheric temperature, precipitation, radiation and wind, and of oceanic temperatures, currents and sea ice cover. Models can also simulate essential aspects of many of the patterns of climate variability observed across a range of time scales. Examples include the advance and retreat of the major monsoon systems, the seasonal shifts of temperatures, storm tracks and rain belts, and the hemispheric-scale seesawing of extratropical surface pressures (the Northern and Southern ‘annular modes’). Some climate models, or closely related variants, have also been tested by using them to predict weather and make seasonal forecasts. These models demonstrate skill in such forecasts, showing they can represent important features of the general circulation across shorter time scales, as well as aspects of seasonal and interannual variability. Models’ ability to represent these and other important climate features increases our confidence that they represent the essential physical processes important for the simulation of future climate change. (Note that the limitations in climate models’ ability to forecast weather beyond a few days do not limit their ability to predict long-term climate changes, as these are very different types of prediction – see [FAQ 1.2](http://co2now.org/index.php?option=com_content&task=view&id=40&Itemid=86).) A third source of confidence comes from the ability of models to reproduce features of past climates and climate changes. Models have been used to simulate ancient climates, such as the warm mid-Holocene of 6,000 years ago or the last glacial maximum of 21,000 years ago (see [Chapter 6](http://www.ipcc.ch/pdf/assessment-report/ar4/wg1/ar4-wg1-chapter6.pdf)). They can reproduce many features (allowing for uncertainties in reconstructing past climates) such as the magnitude and broad-scale pattern of oceanic cooling during the last ice age. Models can also simulate many observed aspects of climate change over the instrumental record. One example is that the global temperature trend over the past century (shown in Figure 1) can be modelled with high skill when both human and natural factors that influence climate are included. Models also reproduce other observed changes, such as the faster increase in nighttime than in daytime temperatures, the larger degree of warming in the Arctic and the small, short-term global cooling (and subsequent recovery) which has followed major volcanic eruptions, such as that of Mt. Pinatubo in 1991 (see [FAQ 8.1](http://co2now.org/index.php?option=com_content&task=view&id=45&Itemid=89), Figure 1). Model global temperature projections made over the last two decades have also been in overall agreement with subsequent observations over that period ([Chapter 1](http://www.ipcc.ch/pdf/assessment-report/ar4/wg1/ar4-wg1-chapter1.pdf)). Nevertheless, models still show significant errors. Although these are generally greater at smaller scales, important large-scale problems also remain. For example, deficiencies remain in the simulation of tropical precipitation, the El Niño-Southern Oscillation and the Madden-Julian Oscillation (an observed variation in tropical winds and rainfall with a time scale of 30 to 90 days). The ultimate source of most such errors is that many important small-scale processes cannot be represented explicitly in models, and so must be included in approximate form as they interact with larger-scale features. This is partly due to limitations in computing power, but also results from limitations in scientific understanding or in the availability of detailed observations of some physical processes. Significant uncertainties, in particular, are associated with the representation of clouds, and in the resulting cloud responses to climate change. Consequently, models continue to display a substantial range of global temperature change in response to specified greenhouse gas forcing (see[Chapter 10](http://www.ipcc.ch/pdf/assessment-report/ar4/wg1/ar4-wg1-chapter10.pdf)). Models are also becoming more comprehensive in their treatment of the climate system, thus explicitly representing more physical and biophysical processes and interactions considered potentially important for climate change, particularly at longer time scales. Examples are the recent inclusion of plant responses, ocean biological and chemical interactions, and ice sheet dynamics in some global climate models. In summary, confidence in models comes from their physical basis, and their skill in representing observed climate and past climate changes. Models have proven to be extremely important tools for simulating and understanding climate, and there is considerable confidence that they are able to provide credible quantitative estimates of future climate change, particularly at larger scales. Models continue to have significant limitations, such as in their representation of clouds, which lead to uncertainties in the magnitude and timing, as well as regional details, of predicted climate change. Nevertheless, over several decades of model development, they have consistently provided a robust and unambiguous picture of significant climate warming in response to increasing greenhouse gases.

# Truth defenses

## Truth defenses: Truth exists

### The Truth exists outside of our thoughts—it’s defined by the physical limitations of reality

McLaughlin 09, Amy L, Florida Atlantic Unviersity, “Peircean Polymorphism: Between Realism and Anti-realism,” Transactions of the Charles S. Peirce Society, Volume 45, Number 3, Summer 2009, Project Muse. http://muse.jhu.edu/journals/transactions\_of\_the\_charles\_s\_peirce\_society/v045/45.3.mclaughlin.html

Reality is independent of what individuals think it to be, according to Peirce, and truth is concordance with reality.[14](http://muse.jhu.edu/journals/transactions_of_the_charles_s_peirce_society/v045/45.3.mclaughlin.html" \l "f14) He writes, for instance, "Truth is conformity of a representamen to its object."[15](http://muse.jhu.edu/journals/transactions_of_the_charles_s_peirce_society/v045/45.3.mclaughlin.html" \l "f15) When asked, "[W]hat is that 'object' which serves to define truth?" he responds, "Why it is the *reality*: it is of such a nature as to be independent of representations of it."[16](http://muse.jhu.edu/journals/transactions_of_the_charles_s_peirce_society/v045/45.3.mclaughlin.html" \l "f16) Similarly, Peirce declares elsewhere, "[T]he immediate object of thought in a true judgment *is* the reality."[17](http://muse.jhu.edu/journals/transactions_of_the_charles_s_peirce_society/v045/45.3.mclaughlin.html" \l "f17) Reality serves, for Peirce, not only to ground the scientific enterprise and inquiry more generally, but also to explain the obvious discrepancies between expectations and experience. Why do we think that there is such a thing as reality? Peirce answers: **There must be such a thing**, for we find our opinions constrained; there is something, therefore, which **influences our thoughts, and is not created by them**. We have, it is true, nothing immediately present to us but thoughts. These thoughts, however, have been caused by sensations, and those sensations are constrained by something out of the mind.[18](http://muse.jhu.edu/journals/transactions_of_the_charles_s_peirce_society/v045/45.3.mclaughlin.html" \l "f18) Thus, the assumption of reality is a precursor to inquiry, but it is also a reasonable and warranted assumption. Peirce supposes that the best explanation for the existence of constraints on our sensations and thoughts generally is by reference to some independency. He writes: The question of whether Hamlet was insane is the question whether Shakespeare conceived him to be insane. Consequently, Hamlet is a figment and not a reality. But as to the inkstand being on my table, though I should succeed in persuading myself and all who have seen it that it is a mere optical illusion, yet there will be a limit to this, and by the photographic camera, the balance, new witnesses, etc., it will, at last, I guess, force its recognition upon the world. If so, it has the characteristic which we call reality. There is a blind force about the inkstand by which it crowds its way into our universe in spite of all we can do.[19](http://muse.jhu.edu/journals/transactions_of_the_charles_s_peirce_society/v045/45.3.mclaughlin.html" \l "f19)

## Truth defenses

### Reality is polymorphic – we can try to understand reality even if no single perspective adequately represents truth

McLaughlin 09, Amy L, Florida Atlantic Unviersity, “Peircean Polymorphism: Between Realism and Anti-realism,” Transactions of the Charles S. Peirce Society, Volume 45, Number 3, Summer 2009, Project Muse. http://muse.jhu.edu/journals/transactions\_of\_the\_charles\_s\_peirce\_society/v045/45.3.mclaughlin.html

RPM: The universe—understood to mean the world-as-it-is independently of any particular observations of it—is intrinsically multifaceted and highly complicated. No one perspective on any aspect of the universe can comprehensively represent it. The degree of diversity [End Page 412] and complexity that exists in the universe ultimately thwarts all attempts at generalization or unification (understood as a consolidation of perspectives), and so of formulating any definitive scientific theory (or set of theories). RPM speaks directly to the nature of the universe and the extent to which right scientific representation of it is possible. This latter aspect, having to do with right representation, draws from the theory-ladenness of observation. In this way, the principle makes use of the insights that ordinarily underwrite anti-realist positions. Accepting RPM, though, commits one to accepting that the universe is of a particular nature. Furthermore, it at least allows for the possibility that our best scientific theories, which posit unobservable entities, do indeed speak to reality. These theories cannot, according to the principle, capture the whole of reality; but this in itself is relatively unproblematic. This failure is intrinsic to the nature of representation itself. I argue in what follows that although the truth of RPM cannot be known, it is nonetheless a reasonable principle to adopt, especially as a friendly supplement to the Peircean program.

## Polymorphic reality perm

### Recognizing that reality is polymorphic allows contradicting theories to enrich and cooperate with one another

McLaughlin 09, Amy L, Florida Atlantic Unviersity, “Peircean Polymorphism: Between Realism and Anti-realism,” Transactions of the Charles S. Peirce Society, Volume 45, Number 3, Summer 2009, Project Muse. http://muse.jhu.edu/journals/transactions\_of\_the\_charles\_s\_peirce\_society/v045/45.3.mclaughlin.html

Since the world is fundamentally ambiguous, it is not surprising that different inquirers represent the world in different ways. Nor is the proliferation of different fields in science, or even theories within the same field, surprising. The existence of dramatic differences of perspective, or theoretical differences, is not only consistent with RPM, such differences are to be *expected* on the assumption of RPM. What might be more surprising, on the assumption of RPM, is that there seems to be fairly widespread agreement among inquirers about, e.g. the application of terms. It turns out, however, that this is not a problem for the approach. Most of the time we are content to use terms in the language fairly fluidly, and when we are at our most charitable we allow the assumptions informing the use of terms some leeway as well. We 'bend' or 'stretch' our conceptions to fit current context. Adoption of **RPM** fosters tolerance and openness to different interpretations. Once one accepts that multiple interpretations are possible, [End Page 413] and that none is the definitive interpretation, one becomes much more receptive to alternatives. For example, suppose that persons A and B find themselves holding what appear to be opposing views about some matter. In order to resolve the incompatibility, at least one of them (A or B) must set aside his own presuppositions and try to reinterpret the world from the other's perspective. That is, A (say) must reexamine the matter through the lens of B's assumptions. Thus, the sort of receptivity **fostered by RPM** points one to an examination of the world itself, which fits well with our intuitions about the ways in which scientific disputes in particular should be resolved. Granting both a fundamental ambiguity to reality and a degree of reasonableness to other inquirers, differences in interpretation are best resolved by comparing each interpretation with what it is supposed to represent. **RPM** guarantees that there is a reality with which we can compare our representations, but it also guarantees that multiple interpretations of this reality are possible.[54](http://muse.jhu.edu/journals/transactions_of_the_charles_s_peirce_society/v045/45.3.mclaughlin.html" \l "f54) We *must* take some approach toward the world, and whatever approach we adopt will inform our experience. Every experience is dependent upon what we expect, upon the categories into which we order the experience. Accepting RPM means accepting that those categories will never absolutely suffice. This opens the door for endless reinterpretation. What the world shows us depends upon the structure into which we fit it. In accepting alternative interpretations, we give the world opportunity to show other aspects of itself.[55](http://muse.jhu.edu/journals/transactions_of_the_charles_s_peirce_society/v045/45.3.mclaughlin.html" \l "f55) At the same time we give ourselves opportunity to expand and enrich the boundaries of our experience. At least some of the structure we use for ordering the world comes from our best scientific theories. These theories have evolved over time, and dramatic shifts in the assumptions of fundamental theories have resulted in dramatic re-conceptualizations of the world. The Newtonian picture of a precisely and objectively ordered world, for example, was exchanged for the relativistic picture offered by Einstein. Newtonian mechanics has neither been discarded nor discounted, however. It is recognized as approximately correct—as having application within the appropriate boundaries. RPM helps to explain why this would be the case.[56](http://muse.jhu.edu/journals/transactions_of_the_charles_s_peirce_society/v045/45.3.mclaughlin.html" \l "f56) From a particular perspective, (i.e., given the boundaries of classical mechanics), Newtonian descriptions and relationships are adequate. They cannot tell the whole story, however, which is one of the lessons of relativistic mechanics**. Taking reality to be co-operative in the formation of theories**, **but** nevertheless **always incompletely represented, aids in understanding the history of science.** RPM helps to explain why past theories have been defeated in some sense, as well as why we can maintain aspects of defeated theories. That is, RPM has the explanatory advantage of accounting for theory failures as well as successes. We want not only to explain past successes but also to anticipate future success. If we adopt a uniqueness-based metaphysics, the best we [End Page 414] can hope for our current theories is that they will coincide in some way with those that the community would reach in the long run. RPM, on the other hand, allows current theories a strong connection with reality—a reality that is no longer tied to the unattainable 'end of inquiry', but is rather something to which we have continual (if veiled) access. Furthermore, this approach provides hope that even though the theories we now accept are related in a robust and important way to reality, there is still much to learn. It gives us a motivation to continue to inquire, to strive for a richer understanding of the world. This continual striving fits well with Peirce's account of truth as a regulative ideal that demands continued inquiry. RPM provides a metaphysics that promotes inquiry, thereby aiding in the pursuit of truth. Peirce's account of truth is founded on his theory of inquiry combined with a realist metaphysics. Truth is defined as what would be accepted by the community of inquirers given sufficient inquiry; and inquiry is sufficient where it has accounted for all possible implications. Given RPM, no one approach could ever provide adequate guidance for a full exploration of reality. Nonetheless, this does not undermine Peirce's approach to inquiry. Rather the principle advocated here serves to explain why Peirce's approach to inquiry is the most fruitful. Viewing matters from every angle will give the fullest possible access, and this is just the sort of thing that Peirce's theory of inquiry advocates. The pursuit of inquiry itself is the best hope we have for attaining truth, even if complete truth always evades us. Indeed this very evasion underwrites a need for continuing inquiry, needed because it will expose previously unnoticed aspects of reality and thereby provide a richer and fuller understanding. The approach that is advocated here is a realism based upon an assumption of non-uniqueness. It was suggested above that this approach provides motivation to inquiry, since RPM implies that whatever are our current views they are ultimately incomplete. Thus, continual inquiry is required in order to supplement current theories and enrich our conceptions about the world. Someone might claim, however, that any non-uniqueness based approach cannot serve this function. Non-uniqueness, one might argue, serves much more than uniqueness to *block* the path to inquiry because it fails to provide proper incentive to resolve methodological, theoretical, and experimental differences. The end result, in this case, would be a continual fragmentation of science. At worst, science would degenerate from an organized, communally cohesive activity into a state of chaos and anarchy. Popular discourse could fare much worse, since there are not the same safeguards in place (the process of peer review, conference and panel discussions, procedures for tenure and promotion, etc.) to ensure fairly widespread agreement. Advocates of uniqueness are likely to claim that it is precisely the uniqueness assumption that provides an appropriate check against the [End Page 415] tendency to fragment, whereas non-uniqueness tends to foster such a tendency. The worry seems to be this: If we accept non-uniqueness and therefore agree that no one theory can ever be definitive, then there is no need to resolve disagreements. Thus, whatever fruits might come from inquiry aimed at difference resolution would be lost to the scientific community and to the community of inquirers at large. Considering the objection outlined above serves to clarify some of RPM's implications and, of equal importance, some of the things that are not implied by RPM. Accepting a non-uniqueness account need not be synonymous with accepting that "anything goes." If one's own assumptions are the only guidelines, and assumptions are underwritten *only* by other assumptions, then perhaps anything can go. This is not so, however, if assumptions are underwritten by reality. Reality is one even if there can be no unitary, complete representation of it; and at the very least, what 'goes' must conform with the one reality. Thus, reality can be one while truths can be many.[57](http://muse.jhu.edu/journals/transactions_of_the_charles_s_peirce_society/v045/45.3.mclaughlin.html" \l "f57) Furthermore, it is not the case that each of the representations that conform with reality is on equal footing with every other. Some approaches are more comprehensive than others; some are more precise; some are useful in one context but cumbersome or clumsy in others. Different purposes and contexts will determine, to a fair extent, which approach is most appropriate. Often, it is just such considerations that work against the fragmentation that sometimes occurs in science, and that allow us to work among the fragmentations that occur in public discourse settings. Thus, the program advocated here does not promote fragmentation. In light of RPM it is most reasonable to be open-minded, with respect to the meaning of the terms of a theory as well as with respect to theories themselves. Since there can be no definitive theory (or set of underlying assumptions), appropriate comparisons can only be made by reference to experience. Fortunately, though, experience is understood on this account to be underwritten by reality. While access to reality is restricted to our representations of it, representations can be assessed for their conformity to reality through their implications for experience. Furthermore, RPM itself has practical implications. Accepting RPM has consequences for future behavior, and we can evaluate the hypothesis to some degree by evaluating those habits that its acceptance would instill in the inquirer. In "A Survey of Pragmaticism," Peirce writes the following: [E]very man exercises more or less control over himself by means of modifying his own habits; and the way in which he goes to work to bring this effect about in those cases in which circumstances will not permit him to practice reiterations of the desired kind of conduct in the outer world shows that he is virtually well acquainted with the important principle that *reiterations in the inner world,—fancied* [End Page 416] *reiterations,—if well-intensified by direct effort, produce habits,* just as do reiterations in the outer world; *and these habits will have power to influence actual behavior in the outer world;* especially, if each reiteration be accompanied by a peculiar strong effort that is usually likened to issuing a command to one's future self.[58](http://muse.jhu.edu/journals/transactions_of_the_charles_s_peirce_society/v045/45.3.mclaughlin.html" \l "f58)

### Perm: accept that our scientific truth isn’t objective and continue to pursue the truth using scientific reasoning—this allows for a convergence on Truth

Kitcher 93 Philip, Philosopher of Science @ Columbia University, “The Advancement of Science: Science Without Legend, Objectivity Without Illusions,” Oxford University Press, 1993. Pp. 137-8

The intuitive idea behind the optimistic induction is a very straightforward one. We believe that Priestley was wrong, Lavoisier was wrong, Dalton was wrong, Avogadro was wrong, and so on. But we also think that Lavoisier improved on Priestley, Dalton on Lavoisier, Avogadro on Dalton. So while we do not endorse the claims of our predecessors we do support their sense of themselves as making progress. In consequence, we expect that our suc- cessors will support our sense that we have made further progress.

Antirealists might reply that they too can endorse the progressiveness of the sequence of chemists mentioned, although they would try to explain this in different terms. But this misses the point of the optimistic induction. That induction is intended as a counter to pessimism. The structure of the pessi- mist's argument is as follows: "Suppose that we assign to the theoretical claims of contemporary science the status that the realist suggests. Notice, then, that our predecessors might have done the same. But the realist doesn't want to confer the privileged status on their claims, most of which are now discredited. So how do we justify treating the theoretical claims of contemporary science differently?" To this, the optimistic induction replies as follows: "Sensible realists should abandon the idea that contemporary theoretical claims are literally true in favor of the notion that there has been convergence to the truth. So we accept the pessimist's point. Notice however that this leaves untouched the idea that the historical development can be seen in terms of convergence on the truth, understood in the realist's preferred way. Using the realist's conceptions of increasing truthlikeness of statements (improved false statements, more adequate reference potentials, more complete or more correct schemata) we can take the very same examples that the pessimist uses to undermine our confidence in the literal truth of contemporary theoretical claims and show that they do not invalidate reformed realism's contentions that our theoretical statements are improving, our schemata becoming more complete, our concepts more adequate, when all these are understood in the realist's preferred way. So the pessimistic induction has no force against a properly formulated realism." The optimistic induction, then, is not an at- tempt to show that the history of science must be interpreted in terms of the realist's conception of progress (although it is worth asking the antirealist to explain a rival account of progress in detail and to show that it does not make surreptitious appeals to truth) but to blunt an apparently damaging challenge to explain the difference between the science of the past and the science of the present. Suggestive as they are, these observations strike me as altogether too weak. The pessimistic induction relies critically on the gross analysis of science as a constellation of theories which are supposed to be true and which are, historically, failures. I oppose to it a simple point. The history of science does not reveal to us that we are fallible in some undifferentiated way. Some kinds of claims endure, other kinds are likely to be discarded as inaccurate. Fur- thermore this is exactly what we would have expected given our conception of the relationship between human cognitive systems and nature. According to that conception we are relatively good at finding out some things, and discover others only with relative difficulty. We would expect that, in the latter sorts of endeavors, history would be peppered with false starts and plausible, but misleading, conclusions. With respect to the former projects we would an- ticipate doing better.

## Prediction good - policymaking

### Predicting future catastrophes are key to spur action before extinction is inevitable

Kurasawa, 04 (Fuyuki, Professor of Sociology, “Cautionary Tales: The Glboal Culture of Prevention and the Work of Foresight,” York University of Toronto, Fuyuki, Constellations Volume 11, No 4, 2004).

Acknowledging the fact that the future cannot be known with absolute certainty does not imply abandoning the task of trying to understand what is brewing on the horizon and to prepare for crises already coming into their own. In fact, the incorporation of the principle of fallibility into the work of prevention means that we must be ever more vigilant for warning signs of disaster and for responses that provoke unintended or unexpected consequences (a point to which I will return in the final section of this paper). In addition, from a normative point of view, the acceptance of historical contingency and of the self-limiting character of farsightedness places the duty of preventing catastrophe squarely on the shoulders of present generations. The future no longer appears to be a metaphysical creature of destiny or of the cunning of reason, nor can it be sloughed off to pure random- ness. It becomes, instead, a result of human action shaped by decisions in the present – including, of course, trying to anticipate and prepare for possible and avoidable sources of harm to our successors. Combining a sense of analytical contingency toward the future and ethical responsibility for it, the idea of early warning is making its way into preventive action on the global stage. Despite the fact that not all humanitarian, techno- scientific, and environmental disasters can be predicted in advance, the multipli- cation of independent sources of knowledge and detection mechanisms enables us to foresee many of them before it is too late. Indeed, in recent years, global civil society’s capacity for early warning has dramatically increased, in no small part due to the impressive number of NGOs that include catastrophe prevention at the heart of their mandates.17 These organizations are often the first to detect signs of trouble, to dispatch investigative or fact-finding missions, and to warn the inter- national community about impending dangers; to wit, the lead role of environ- mental groups in sounding the alarm about global warming and species depletion or of humanitarian agencies regarding the AIDS crisis in sub-Saharan Africa, fre- quently months or even years before Western governments or multilateral institu- tions followed suit. What has come into being, then, is a loose-knit network of watchdog groups that is acquiring finely tuned antennae to pinpoint indicators of forthcoming or already unfolding crises.

### Policymaking is good—the plan is a trickle-up action to forwarn disasters—that’s the primary goal of debaters as active citizens

Kurasawa, 04 (Fuyuki, Professor of Sociology, “Cautionary Tales: The Glboal Culture of Prevention and the Work of Foresight,” York University of Toronto, Fuyuki, Constellations Volume 11, No 4, 2004).

The growing public significance of preventive message in global affairs is part and parcel of what Ignatieff has termed an “advocacy revolution,”19 since threat- ened populations and allied organizations are acting as early warning beacons that educate citizens about certain perils and appeal for action on the part of states and multilateral institutions. Global civil society players have devised a host of ‘nam- ing and shaming’ strategies and high-profile information campaigns to this effect, including press conferences, petitions, mass marches, and boycotts, and spectacu- lar stunts that denounce bureaucratic inertia, the reckless pursuit of profit, or the preponderance of national interests in world affairs.20 The advocacy revolution is having both ‘trickle-down’ and ‘trickle-up’ effects, establishing audiences of constituents and ordinary citizens conversant with some of the great challenges facing humanity as well as putting pressure on official institutions to be proactive in their long-term planning and shorter-term responses. None of this would be possible without the existence of global media, whose speed and range make it possible for reports of an unfolding or upcoming disaster to reach viewers or readers in most parts of the world almost instantaneously. Despite the highly selective character of what is deemed newsworthy and state and commercial influence on what is broadcast, several recent attempts to hide evid- ence of acts of mass violence (Tiananmen Square, East Timor, Chechnya, etc.) and crises (e.g., during the Chernobyl nuclear accident in the Soviet Union or the SARS outbreak in China) have failed; few things now entirely escape from the satellite camera, the cellular telephone, or the notebook computer. And although the internet may never become the populist panacea technological determinists have been heralding for years, it remains a key device through which concerned citizens and activists can share and spread information. While media coverage almost always follows a crisis rather than preceding it, the broadcast of shocking images and testimonies can nevertheless shame governments and international organizations into taking immediate steps. The ‘CNN or BBC effect,’ to which we should now add the ‘Al-Jazeera effect,’ is a surprisingly powerful force in impact- ing world public opinion, as the now notorious Abu Ghraib prison photographs remind us. The possibility that the threat of media exposure may dissuade individ- uals and groups from enacting genocidal plans or reckless gambles with our future is one of the lynchpins of prevention in our information-saturated age. Are forewarnings of disasters being heard? The mobilization of official inter- vention and popular interest has certainly been mixed, yet global civil society is having some success in cultivating audiences and advocates coalescing around specific perils (mass human rights violations, ecological devastation, genetic engineering, epidemics, and so on). After Bhopal and Chernobyl, after ‘mad cow disease’ and the war in Iraq, citizens are scrutinizing, questioning and even contesting official expertise in risk assessment more than ever before.21 Hence, in a world where early warnings of cataclysms are often available, pleading ignor- ance or helplessness to anticipate what may come in the future becomes less and less plausible.

### Even if we lose our truth claims, dystopian representations of the world are good—critically challenges the current social order

Kurasawa, 04 (Fuyuki, Professor of Sociology, “Cautionary Tales: The Glboal Culture of Prevention and the Work of Foresight,” York University of Toronto, Fuyuki, Constellations Volume 11, No 4, 2004).

NGOs and social movements active in global civil society have drawn upon the moral imagination in similar ways, introducing dystopian scenarios less as prophecies than as rhetorical devices that act as ‘wake-up calls.’ Dystopias are thrust into public spaces to jolt citizens out of their complacency and awaken their concern for those who will follow them. Such tropes are intended to be controversial, their contested character fostering public deliberation about the potential cataclysms facing humankind, the means of addressing them, and the unintended and unexpected consequences flowing from present-day trends. In helping us to imagine the strengths and weaknesses of different positions towards the future, then, the dystopian imaginary crystallizes many of the great issues of the day. Amplifying and extrapolating what could be the long-term consequences of current tendencies, public discourse can thereby clarify the future’s seeming opaqueness. Likewise, fostering a dystopian moral imagination has a specifically critical function, for the disquiet it provokes about the prospects of later gener- ations is designed to make us radically question the ‘self-evidentness’ of the existing social order.34 If we imagine ourselves in the place of our descendants, the taken- for-granted shortsightedness of our institutionalized ways of thinking and acting becomes problematic. Indifference toward the future is neither necessary nor inevitable, but can be – and indeed ought to be – changed.

### Alt fails—Failing to engage in political discussion and risk analysis leaves debate to dominant techno-scientific groups

Kurasawa, 04 (Fuyuki, Professor of Sociology, “Cautionary Tales: The Glboal Culture of Prevention and the Work of Foresight,” York University of Toronto, Fuyuki, Constellations Volume 11, No 4, 2004).

If fear-mongering is a misappropriation of preventive foresight, resignation about the future represents a problematic outgrowth of the popular acknow- ledgment of global perils. Some believe that the world to come is so uncertain and dangerous that we should not attempt to modify the course of history; the future will look after itself for better or worse, regardless of what we do or wish. One version of this argument consists in a complacent optimism perceiving the future as fated to be better than either the past or the present. Frequently accompanying it is a self-deluding denial of what is plausible (‘the world will not be so bad after all’), or a naively Panglossian pragmatism (‘things will work themselves out in spite of everything, because humankind always finds ways to survive’).37 Much more com- mon, however, is the opposite reaction, a fatalistic pessimism reconciled to the idea that the future will be necessarily worse than what preceded it. This is sustained by a tragic chronological framework according to which humanity is doomed to decay, or a cyclical one of the endless repetition of the mistakes of the past. On top of their dubious assessments of what is to come, alarmism and resigna- tion would, if widely accepted, undermine a viable practice of farsightedness. Indeed, both of them encourage public disengagement from deliberation about scenarios for the future, a process that appears to be dangerous, pointless, or unnecessary. The resulting ‘depublicization’ of debate leaves dominant groups and institutions (the state, the market, techno-science) in charge of sorting out the future for the rest of us, thus effectively producing a heteronomous social order. How, then, can we support a democratic process of prevention from below? The answer, I think, lies in cultivating the public capacity for critical judgment and deliberation, so that participants in global civil society subject all claims about potential catastrophes to examination, evaluation, and contestation. Two norma- tive concepts are particularly well suited to grounding these tasks: the precaution- ary principle and global justice.

### Long term preventative action has empirically worked—we have an ethical duty to address earthly problems

Kurasawa, 04 (Fuyuki, Professor of Sociology, “Cautionary Tales: The Glboal Culture of Prevention and the Work of Foresight,” York University of Toronto, Fuyuki, Constellations Volume 11, No 4, 2004).

Moreover, keeping in mind the sobering lessons of the past century cannot but make us wary about humankind’s supposedly unlimited ability for problem- solving or discovering solutions in time to avert calamities. In fact, the historical track-record of last-minute, technical ‘quick-fixes’ is hardly reassuring. What’s more, most of the serious perils that we face today (e.g., nuclear waste, climate change, global terrorism, genocide and civil war) demand complex, sustained, long-term strategies of planning, coordination, and execution. On the other hand, an examination of fatalism makes it readily apparent that the idea that humankind is doomed from the outset puts off any attempt to minimize risks for our succes- sors, essentially condemning them to face cataclysms unprepared. An a priori pessimism is also unsustainable given the fact that long-term preventive action has had (and will continue to have) appreciable beneficial effects; the examples of medical research, the welfare state, international humanitarian law, as well as strict environmental regulations in some countries stand out among many others. The evaluative framework proposed above should not be restricted to the cri- tique of misappropriations of farsightedness, since it can equally support public deliberation with a reconstructive intent, that is, democratic discussion and debate about a future that human beings would freely self-determine. Inverting Foucault’s Nietzschean metaphor, we can think of genealogies of the future that could perform a farsighted mapping out of the possible ways of organizing social life. They are, in other words, interventions into the present intended to facilitate global civil society’s participation in shaping the field of possibilities of what is to come. Once competing dystopian visions are filtered out on the basis of their analytical credibility, ethical commitments, and political underpinnings and consequences, groups and individuals can assess the remaining legitimate cata- strophic scenarios through the lens of genealogical mappings of the future. Hence, our first duty consists in addressing the present-day causes of eventual perils, ensuring that the paths we decide upon do not contract the range of options available for our posterity.42 Just as importantly, the practice of genealogically- inspired farsightedness nurtures the project of an autonomous future, one that is socially self-instituting. In so doing, we can acknowledge that the future is a human creation instead of the product of metaphysical and extra-social forces (god, nature, destiny, etc.), and begin to reflect upon and deliberate about the kind of legacy we want to leave for those who will follow us. Participants in global civil society can then take – and in many instances have already taken – a further step by committing themselves to socio-political struggles forging a world order that, aside from not jeopardizing human and environmental survival, is designed to rectify the sources of transnational injustice that will continue to inflict need- less suffering upon future generations if left unchallenged.

## Predictions good – IR

### There are political realities that we can use to base predictions off of –not doing so risks extinction and genocide

Fitzsimmons 06 Michael Fitzsimmons is a defence analyst in Washington DC. (2006): The Problem of Uncertainty in Strategic Planning, Survival, 48:4, 131-146 http://dx.doi.org/10.1080/00396330601062808

In spite of its intuitive appeal, applying uncertainty to strategic planning quickly becomes problematic and can even inhibit the flexibility it was meant to deliver. The first question we must ask here is: granting the inevitability of surprise, can we learn anything about the future from the current security environment? We do know a great deal about that. We know, for example, that transnational terrorist networks are actively targeting US interests and allies throughout the world. Also, Iran, a nation with a track record of xenophobic foreign policy and support for terrorism, is building nuclear weapons. The paranoid, totalitar- ian regime in North Korea continues to threaten its neighbours with nuclear weapons, sophisticated missile systems, a million-man army, and thousands of artillery tubes within range of Seoul, the heart of one of Asia’s largest econo- mies. None of these conditions is likely to disappear in the near future. Acknowledging current challenges such as these does not suggest that obvious threats are necessarily the most important ones, or that the future is bound to resemble the present. But it does suggest that uncertainty must be considered within the context of an environment where some significant threats are relatively clear and where certain known contingencies are important to plan for. Unexpected threats notwithstanding, just the three threats noted above constitute a fairly healthy set of challenges for strategic planning to address. And, of course, the Pentagon does plan extensively for these and other specific contingencies. From this perspective, it becomes a bit harder to accept the claim that ‘uncertainty defines the strategic and operational environment today’. Additionally, the notion that today’s future is less certain than yesterday’s is overdrawn. There is more nostalgia than truth behind the characterisation of the Cold War as ‘a time of reasonable predictability’. Presidents from Harry Truman to George H.W. Bush might have taken exception to that description, as might soldiers, sailors, airmen and marines deployed to Korea, Vietnam, Iran, Lebanon, Grenada, Libya, Panama and Iraq, among other places, while Pentagon strate- gists refined plans for war in Central Europe. By the same token, today’s future may not be shrouded in complete mystery. Indeed, much of recent official rhetoric surrounding the ‘war on terror’ echoes that of the Cold War, identifying the emergence of a mortal enemy, in the form of violent radical Islam, and the prospects for a generational struggle against that enemy.11 This rhetoric contrasts sharply with claims that uncertainty is central to strategic planning. The 2006 QDR flirts with a little logical tension when it introduces the term ‘the long war’ and the notion of ‘an era of surprise and uncertainty’ within one page of each other.12 In sum, the justification for emphasising uncertainty in strategic planning is questionable. Strategic uncertainty is neither novel to the current security environment nor overwhelming in the face of some clear challenges facing US national security.

### Denying predictions destroys policymaking – military strategists will use their personal opinions to fill in the void

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However, the weak grounds for emphasising uncertainty are only half the problem: even if justified in theory, applying uncertainty to planning is highly problematic. In the most literal sense, uncertainty makes planning impossi- ble: one cannot plan for that which cannot be known.13 However, traditional rational-choice theory distinguishes this kind of pure uncertainty from risk. Conditions of risk are those that prevail in casinos or the stock market, where future outcomes are unknown, but probabilities can be estimated. Conditions of uncertainty, by contrast, are those where there is no basis even for estimating probabilities.14 If the uncertainty evoked by strategic planners is more properly characterised as risk, then this implies the need for greater variability in the scenarios and planning factors against which strategies are tested. But handling even this weaker form of uncertainty is still quite challeng- ing. If not sufficiently bounded, a high degree of variability in planning factors can exact a significant price on planning. The complexity presented by great variability strains the cognitive abilities of even the most sophisticated decision- makers.15 And even a robust decision-making process sensitive to cognitive limitations necessarily sacrifices depth of analysis for breadth as variability and complexity grows. It should follow, then, that in planning under conditions of risk, variability in strategic calculation should be carefully tailored to available analytic and decision processes. Why is this important? What harm can an imbalance between complexity and cognitive or analytic capacity in strategic planning bring? Stated simply, where analysis is silent or inadequate, the personal beliefs of decision-makers fill the void. As political scientist Richard Betts found in a study of strategic sur- prise, in ‘an environment that lacks clarity, abounds with conflicting data, and allows no time for rigorous assessment of sources and validity, ambiguity allows intuition or wishfulness to drive interpretation ... The greater the ambiguity, the greater the impact of preconceptions.’16 The decision-making environment that Betts describes here is one of political-military crisis, not long-term strategic planning. But a strategist who sees uncertainty as the central fact of his environ- ment brings upon himself some of the pathologies of crisis decision-making. He invites ambiguity, takes conflicting data for granted and substitutes a priori scepticism about the validity of prediction for time pressure as a rationale for discounting the importance of analytic rigour.

### Uncertainty in strategic planning leads to overcompensation of threats—empirically that led to the proliferation of weapons for deterrence

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If the effects of stressing uncertainty were limited to contradictory statements in strategic-planning documents and speeches, the harm would be small and redress would be of largely academic interest. But there is strong circumstan- tial evidence that these effects extend beyond the rhetorical domain. Three examples illustrate problems arising from an aversion to prediction in strategic planning. Current nuclear-weapons policy and posture illustrate the strategic costs that uncertainty can exact in the form of keeping options open. The 2006 QDR shows how uncertainty can inhibit clear strategic choice in the allocation of resources. Finally, the use of intelligence and expert advice in planning for the 2003 invasion of Iraq shows how uncertainty can actually serve to privilege pre-conceptions over analysis and thereby undermine strategic flexibility. Uncertainty in the future security environment has been a key organising principle for the posture and planning of the US nuclear arsenal. In an effort to leave Cold War nuclear-force-sizing logic behind, the 2001 Nuclear Posture Review (NPR) adopted from the 2001 QDR a ‘capabilities-based approach’ to establishing requirements for US nuclear weapons. The premise of the capabili- ties-based approach is that threats cannot be predicted reliably. As a result, in the words of then Under Secretary of Defense for Policy Douglas Feith, ‘instead of our past primary reliance on nuclear forces for deterrence, we will need a broad array of nuclear, non-nuclear and defensive capabilities for an era of uncertainty and surprise’.17 In practical terms, this meant that the numbers and alert levels of deployed nuclear weapons would need to be considerably higher than would be necessary simply to deter Russia and China. While the NPR is classified, the extent to which its policy is underpinned by the strategic importance of uncertainty is made very clear in a private report published in January 2001 by several strategists who, only months later, were writing nuclear policy in the Pentagon.18 The report, published by the National Institute for Public Policy, identifies a variety of plausible ways in which the future security environment might change from the status quo, especially in dangerous directions, and evaluates the potential utility of nuclear weapons in adapting to those changes. It does not attempt to assess the likelihoods of any of those alternative futures and, indeed, dismisses the utility of any such assessment, concluding that ‘there can be no logical integrity in the confident assertion that any given force level, even if judged to be appropriate today, will continue to be so in the future’.19 The problem with this logic, while laudably cautious, is that it does not leave a great deal of scope for deciding on or justifying any course of action what- soever about weapons deployment. If there were no trade-offs involved with having large numbers of nuclear weapons on high alert, this might be a minor problem. But, of course, this is not the case. Beyond the resources they consume, large numbers of nuclear weapons on alert may be unnecessarily provocative in crises, may hamper non-proliferation efforts, and may raise the risk of acci- dental launch by other nuclear powers prompted to maintain high alert levels themselves. The risks of being underprepared for unexpected warfighting con- tingencies must be weighed against these. A 1997 National Academy of Sciences report summarised this trade-off: ‘During the Cold War, reducing the risk of a surprise attack appeared to be more important than the risks generated by maintaining nuclear forces in a continu- ous state of alert. With the end of that era, the opposite view is now more credible.’20 The nature of the risks associated with nuclear weapons is, of course, a complicated and contentious matter. But it is crucially important to recognize that the very terms of a debate regarding trade-offs of risk depend on some ability to assess the relative likelihood of events. Without that, the debate is reduced to a competition of assertions. In this way, emphasizing uncertainty buys flexibility, but at unknown cost, and thus obscures the path toward strategic choice.

### Critiques of scientific intelligence miss the boat—only ignoring this data has empirically led to violence false assumptions

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But standards of evidence and subject-matter expertise are exactly the sorts of factors decision-makers sceptical of the reliability of prediction might be apt to discount. If uncertainty defines the strategic environment, then what greater insight can the expert analyst bring to bear on strategy than the generalist? This attitude could marginalise intelligence analysis in strategic decision-making.

US planning for the aftermath of the Iraq War exemplifies how such mar- ginalisation has played a significant role in recent strategic decision-making. In the judgement of Paul Pillar, the senior US intelligence official for Middle East analysis from 2000 to 2005, ‘what is most remarkable about prewar U.S. intelli- gence on Iraq is not that it got things wrong and thereby misled policymakers; it is that it played so small a role in one of the most important U.S. policy decisions in recent decades’.26 While great volumes of ink have been spilled in the debate over intelligence estimates of Iraqi nuclear, biological or chemical weapons, there is much more clarity about the intelligence community’s estimates of the political environment the US would face in post-war Iraq. Those estimates accu- rately predicted most of the major challenges that developed, from insurgency and sectarian violence to the strengthening of Iran’s geopolitical hand and the galvanising effect on foreign radical Islamists.27 The reported expectations of most key administration officials bore little resemblance to these predictions.28 Rumsfeld’s famous distinction between ‘known unknowns’ and ‘unknown unknowns’ came in response to a reporter’s question on the intelligence supporting assertions of linkages between the Iraqi government and terror- ist organisations.29 The implication of his remark was that presumption of a genuine Iraqi–terrorist linkage was justified because the absence of evidence to support the presumption did not conclusively disprove it. Here, as with the post-war planning assumptions, uncertainty served to level the playing field between facts and analysis, on the one hand, and the preconceptions of senior leadership on the other.

## Predictions perm

### Perm: Recognize that there is uncertainty—but still plan around certain information – perms solves best for flexibility

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It is important not to exaggerate the extent to which data and ‘rigorous assessment’ can illuminate strategic choices. Ambiguity is a fact of life, and scepticism of analysis is necessary. Accordingly, the intuition and judgement of decision-makers will always be vital to strategy, and attempting to subordinate those factors to some formulaic, deterministic decision-making model would be both undesirable and unrealistic. All the same, there is danger in the opposite extreme as well. Without careful analysis of what is relatively likely and what is relatively unlikely, what will be the possible bases for strategic choices? A decision-maker with no faith in prediction is left with little more than a set of worst-case scenarios and his existing beliefs about the world to confront the choices before him. Those beliefs may be more or less well founded, but if they are not made explicit and subject to analysis and debate regarding their appli- cation to particular strategic contexts, they remain only beliefs and premises, rather than rational judgements. Even at their best, such decisions are likely to be poorly understood by the organisations charged with their implementation. At their worst, such decisions may be poorly understood by the decision-makers themselves. Moreover, this style of decision-making is self-reinforcing. A strategist dis- missive of explicit models of prediction or cause and effect is likely to have a much higher threshold of resistance to adjusting strategy in the face of changing circumstances. It is much harder to be proven wrong if changing or emerg- ing information is systematically discounted on the grounds that the strategic environment is inherently unpredictable. The result may be a bias toward momentum in the current direction, toward the status quo. This is the antithesis of flexibility. Facts on the ground change faster than belief systems, so the extent to which a strategy is based on the latter rather than the former may be a rea- sonable measure of strategic rigidity. In this way, undue emphasis in planning on uncertainty creates an intellectual temptation to cognitive dissonance on the one hand, and confirmatory bias on the other. And the effect, both insidious and ironic, is that the appreciation for uncertainty subverts exactly the value that it professes to serve: flexibility.

## **Truth defenses: Realism**

### Realism is key to truth-seeking – this is the best feminist epistemology

Nelson and Nelson, 2003 [Lynn Hankinson, professor of philosophy at University of Washington, Jack, Dean of the Graduate School and Professor of Philosophy at Temple University, *Feminist Interpretations of W.V.Quine*, pg 30]

Thus, realist conceptions of truth make it possible to identify bias that is conducive to truth-seeking as “good,” and bias that “impedes” truth seeking as “bad.” Feminists, Antony argues, must be willing to talk about truth and falsity when critiquing epistemic ideals or particular theories that are sexist, racist, or in some other respect wrongheaded. Finally, Antony argues that the notion of truth is appropriately brought to bear on epistemologies—that what warrants acceptance of a feminist or any other epistemology is the belief that it is likely to be true. Here too, Antony maintains, naturalized epistemology has the advantage. It is likely to be able to “explain the facts” (including the “central” fact of “the long ignored experiences and wisdom of women”) and thus likely to be true. Although Antony seems to dismiss feminist standpoint theory rather precipitously, particularly in light of her own arguments concerning the positive role of bias, we take her arguments concerning the recognition of the positive role of bias and other developments in analytic epistemology as making a strong case that more inclusive approaches in feminist theorizing about science and knowledge—in which the resources of analytic epistemology are seriously considered and evaluated—could contribute to feminist philosophy of science.

## **Truth defenses: Realism**

### Realism is the best feminist epistemology – the alternative robs us of tools for normative decision-making and action

Antony, 2003 [Louise, Professor of Philosophy at the University of Massachusetts, Amherst, “Quine as Feminist: The Radical Import of Naturalized Epistemology,” ed. Lynne HankinsonNelson and Jack Nelson *Feminist Interpretations of W.V.Quine*, pg 100]

It is here that I think feminist philosophy stands to lose the most by rejecting the analytic tradition. The dilemma will be impossible to escape, I contend, for any theory that eschews the notion of truth—for any theory, that is, that tries to steer some kind of middle course between absolutism and relativism. Such theories inevitably leave themselves without resources for making the needed normative distinctions, because they deprive themselves of any conceptual tools for distinguishing the grounds of a statement’s truth from the explanation of a statement’s acceptance. Naturalized epistemology has the great advantage over epistemological frameworks outside the analytic tradition (I have in mind specifically standpoint and postmodern epistemologies) in that it permits an appropriately realist conception of truth, viz., one that allows a conceptual gap between epistemology and metaphysics, between the world as we see it and the world as it is.13 Without appealing to at least this minimally realist notion of truth, I see no way to even state the distinction we ultimately must articulate and defend. Quite simply, an adequate solution to the paradox must enable us to say the following: What makes the good bias good is that it facilitates the search for truth, and what makes the bad bias bad is that it impedes it.

### **Realist epistemology is key to understanding the truth of women’s status and experiences**

Antony, 2003 [Louise, Professor of Philosophy at the University of Massachusetts, Amherst, “Quine as Feminist: The Radical Import of Naturalized Epistemology,” ed. Lynne HankinsonNelson and Jack Nelson *Feminist Interpretations of W.V.Quine*, pg 101-2]

Now that my absolutist leanings are out in the open, let me say one more thing about truth that I hope will forestall a possible misunderstanding of my project here. I do believe in truth, and I have never understood why people concerned with justice have given it such a bad rap. Surely one of the goals of feminism is to tell the truth about women’s lives and women’s experience. Is institutionally supported discrimination not a fact? Is misogynist violence not a fact? And isn’t the existence of ideological denial of the first two facts itself a fact? What in the world else could we be doing when we talk about these things, other than asserting that the world actually is a certain way? Getting at the truth is complicated, and one of the things that complicates it considerably is that powerful people frequently have strong motives for keeping less powerful people from getting at the truth. It’s one job of a critical epistemology, in my view, to expose this fact, to make the mechanisms of such distortions transparent. But if We, as critical epistemologists, lose sight of what we’re after, if we concede that there’s nothing at stake other than the matter of whose “version” is going to prevail, then our projects become as morally bankrupt and baldly self-interested as Theirs. This brings me to the nature of the current discussion. I would like to be clear that in endorsing the project of finding a “feminist epistemology,” I do not mean to be advocating the construction of a serviceable epistemological ideology “for our side.” And when I say that I think naturalized epistemology makes a good feminist epistemology, I don’t mean to be suggesting that the justiWcation for the theory is instrumental. A good feminist epistemology must be, in the first place, a good epistemology, and that means being a theory that is likely to be true. But of course I would not think that naturalized epistemology was likely to be true unless I also thought it explained the facts. And among the facts I take to be central are the long-ignored experiences and wisdom of women.

## **AT: Theory-ladenness link**

Theory-ladenness is inevitable and has no impact

Muller 2004 [F.A., Institute for the History and Foundations of Science, Utrecht University, faculty of physics and astronomy, “Can a Constructive Empiricist Adopt the Concept of Observability,” Philosophy of Science 71]

Kukla has understood Friedman’s argument as establishing the incompatibility between (a) the epistemic policy of CE and (b) the theoryladenness of scientific theories, which Van Fraassen subscribes to (cf. Kukla 1998, 139–141). How can (a) and (b) clash? Kukla says they clash because ‘a composite object of more than 1023 atoms of Carbon exists’ is laden by an acceptable theory, and hence acceptable, but is also about something observable, hence susceptible to belief. So it is believed that individual atoms exist because it is believed that 1023 Carbon-atoms exist, in contradiction to the neutral epistemic attitude about unobservables commanded by CE. But, first of all, to acknowledge that our language is ‘theory-laden’ is to acknowledge the rather banal fact that the use of particular words and expressions in our language is governed in certain ways by the theories we accept; in fact, theories are the main providers of the ‘semantic grammar’ (Wittgenstein) of scientific concepts, they provide the most rules how to use these concepts, and this is constitutive for their meaning. To acknowledge this fact does not commit one to believe any proposition about the world of the theory, whether the proposition is empirical or not. Therefore, the incompatibility between (a) and (b) which Kukla discerns must be a chimera. Specifically, CE does not believe that about 1023 Carbon-atoms exist. When we veridically see a diamond, say, we are prepared to believe that this diamond is observable and that it exists; but we are not prepared to believe that 1023 Carbon-atoms exist, because saying that a diamond consists of 1023 Carbon-atoms is an interpretation of what we see partly in terms of unobservables, which CE may accept but does not believe in. ‘Theory-laden’ and ‘laden-with-unobservables’ are distinct predicates of propositions; it seems that Kukla has confused them. Theory-laden sentences can be empirical (‘laden-with-only-actual-observables’) and non-empirical; only the aforementioned are the type of theoryladen statements susceptible to belief and diszbelief. Empirical propositions can be theory-laden or not: ‘Today at such-and-such place-time on the face of planet Earth the sun is visible’ is empirical and not theory-laden, whereas ‘Today at such-and-such place-time on the face of planet Earth a gigantic, continuously exploding Hydrogen-bomb is visible’ (which is what the sun is according to modern physics) is empirical and theoryladen. As soon as unobservables enter statements about observables, these statements do not involve only actual unobservables and therefore fail to qualify as empirical; these statements can at best be accepted on the basis of CE, never believed as true or as false.3

## **AT: Objectivity link**

### Empirical and realist standards for epistemology are key to evaluate the actual impacts of bias on our research and truth claims

Antony, 2003 [Louise, Professor of Philosophy at the University of Massachusetts, Amherst, “Quine as Feminist: The Radical Import of Naturalized Epistemology,” ed. Lynne Hankinson Nelson and Jack Nelson, *Feminist Interpretations of W.V.Quine*, pg 137-8]

Another thing that a naturalized approach to knowledge offers us is the possibility of an empirical theory of biases. As we’ve already seen, there are different kinds of biases—some are natively present, some are acquired. An empirical study of biases can refine the taxonomy and possibly tell us something about the reliability and the corrigibility of biases of various sorts. It may turn out that we can on this basis get something like a principled sorting of biases into good ones and bad ones, although it will be more likely that we’ll learn that even a “good” bias can lead us astray in certain circumstances.63 One likely upshot of an empirical investigation of bias is a better understanding of the processes by which human beings design research programs. What we decide to study and how we decide to study it are matters in which unconscious biases—tendencies to see certain patterns rather than others, to attend to certain factors rather than others, to act in accordance with certain interests rather than others—play a crucial role. We can’t eliminate the biases—we shouldn’t want to, for we’d have no research programs left if we did—but we can identify the particular empirical presuppositions that lie behind a particular program of research so that we can subject them, if necessary, to empirical critique. One important issue is the saliency of certain properties. Every time a study is designed, a decision is made, tacitly or explicitly, to pay attention to some factors and to ignore others. These “decisions” represent tacit or explicit hypotheses about the likely connection between various aspects of the phenomena under study, hypotheses that can be subjected to empirical scrutiny.

### Assuming the objectivity of a scientific community’s work is key to non-ideological forms of progress even if there is bias

Antony, 2003 [Louise, Professor of Philosophy at the University of Massachusetts, Amherst, “Quine as Feminist: The Radical Import of Naturalized Epistemology,” ed. Lynne Hankinson Nelson and Jack Nelson, *Feminist Interpretations of W.V.Quine*, pg 131-2]

This insight is also borne out by the history of science. As Thomas Kuhn has pointed out, science is at its least successful during the periods in its history when it most closely resembles the popular models of scientific objectivity. During a discipline’s “pre-paradigm” phase, when there is no consensus about fundamental principles, nor even about what to count as the central phenomena, research is anarchic and unproductive. But progress accelerates dramatically when a discipline enters its mature period, marked by the emergence of a theory—a paradigm—capable of organizing the phenomena in a compelling enough way that it commands near-universal acceptance. Kuhn emphasizes that one of the chief benefits a paradigm brings with it is a degree of closure about foundational issues, instilling in members of the community a principled and highly functional unwillingness to reconsider basic assumptions. The paradigm not only settles important empirical controversies, but also decides more methodological matters— what are the acceptable forms of evidence, what is the right vocabulary for discussing things, what are the proper standings for judging research. The fact is that all of these matters are disputable in principle—but a paradigm relieves its adherents of the considerable burden of having constantly to dispute them. But what this means is that the practice and attitudes of scientists working within a paradigm will systematically deviate from the popular ideal of scientific objectivity: They will approach their research with definite preconceptions, and they will be reluctant to entertain hypotheses that conflict with their own convictions. Kuhn’s point, however, is that the existence of such closed-mindedness among working scientists—what he calls “the dogmatism of mature science”—is not to be regretted; that it is actually beneficial to the course of scientific development: “Though preconception and resistance to innovation could very easily choke off scientiWc progress, their omnipresence is nonetheless symptomatic of characteristics upon which the continuing vitality of research depends.”52 Once we appreciate these aspects of mature science, we can explain a great deal about how a fantasy of the pure objectivity of science can take hold independently of any ideological purposes such a fantasy might serve. (This is important if we want a serious, nuanced story about how ideologies work.) The fact that certain tenets of theory are, for all practical purposes, closed to debate can render invisible their actual status as hypotheses. Deeply entrenched theoretical principles, like the laws of thermodynamics or the principle of natural selection, become established “facts.”53 Similarly, the high degree of theoretical background required to translate various numbers and images into observations or data is forgotten by people accustomed to performing the requisite inferences on a daily basis. Consensus and uniformity thus translate into objectivity. The more homogeneous an epistemic community, the more objective it is likely to regard itself, and, if its inquiries are relatively self-contained, the more likely it is to be viewed as objective by those outside the community. This suggests one fairly obvious explanation for the general perception that the physical sciences are more objective than the social sciences: Sociology, political science, economics, and psychology are disciplines that still lack paradigms in Kuhn’s technical sense. Because there is still public debate in these fields about basic theoretical and methodological issues, there can be no credible pretense by any partisan of having hold of the unvarnished truth.

### Their arguments that science is male biased contradict with the critique of objectivity – mainstream epistemology and science don’t adopt value-neutrality wholesale – they are all about exploring contradictions

Antony, 2003 [Louise, Professor of Philosophy at the University of Massachusetts, Amherst, “Quine as Feminist: The Radical Import of Naturalized Epistemology,” ed. Lynne HankinsonNelson and Jack Nelson *Feminist Interpretations of W.V.Quine*, pg 100]

Consider feminist theory: On the one hand, it is one of the central aims of feminist scholarship to expose the male-centered assumptions and interests—the male biases, in other words—underlying so much of received “wisdom.” But on the other hand, there’s an equally important strain of feminist theory that seeks to challenge the ideal of pure objectivity by emphasizing both the ubiquity and the value of certain kinds of partiality and interestedness. Clearly, there’s a tension between those feminist critiques that accuse science or philosophy of displaying male bias and those that reject the ideal of impartiality. The tension blossoms into paradox when critiques of the worst sort are applied to the concepts of objectivity and impartiality themselves. According to many feminist philosophers, the flaw in the ideal of impartiality is supposed to be that the ideal itself is biased: Critics charge either that the concept of “objectivity” serves to articulate a masculine or patriarchal viewpoint (and possibly a pathological one),6 or that it has the ideological function of protecting the rights of those in power, especially men.7 But how is it possible to criticize the partiality of the concept of objectivity without presupposing the very value under attack? Put baldly: If we don’t think it’s good to be impartial, then how can we object to men’s being partial? The critiques of “objectivity” and “impartiality” that give rise to this paradox represent the main source of feminist dissatisfaction with existing epistemological theories. It’s charged that mainstream epistemology will be forever unable to either acknowledge or account for the partiality and locatedness of knowledge, because it is wedded to precisely those ideals of objective or value-neutral inquiry that ultimately and inevitably subserve the interests of the powerful. The valorization of impartiality within mainstream epistemology is held to perform for the ruling elite the critical ideological function of denying the existence of partiality itself.8 Thus Lorraine Code, writing in the APA Newsletter on Feminism and Philosophy,9 charges that mainstream epistemology (or what she has elsewhere dubbed “malestream” epistemology10) has “defined ‘the epistemological project’ so as to make it illegitimate to ask questions about the identities and specific circumstances of these knowers.” It has accomplished this, she contends, by promulgating a view of knowers as essentially featureless and interchangeable, and by donning a “mask of objectivity and value-neutrality.” The transformative potential of a feminist— as opposed to a malestream—epistemology lies in its ability to tear off this mask, exposing the “complex power structure of vested interest, dominance, and subjugation” that lurks behind it. But not only is it not the case that contemporary analytic epistemology is committed to such a conception of objectivity, it was analytic epistemology that was largely responsible for initiating the critique of the empiricistic notions Code is attacking. Quine, Goodman, Hempel, Putnam, Boyd, and others within the analytic tradition have all argued that a certain received conception of objectivity is untenable as an ideal of epistemic practice. The detailed critique of orthodox empiricism that has developed within the analytic tradition is in many ways more pointed and radical that the charges that have been leveled from without. Furthermore, these philosophers, like many feminist theorists, have emphasized not only the ineliminability of bias but also the positive value of certain forms of it. As a result, the problems that arise for a naturalized epistemology are strikingly similar to those that beset the feminist theories mentioned above: Once we’ve acknowledged the necessity and legitimacy of partiality, how do we tell the good bias from the bad bias? What kind of epistemology is going to be able to solve a problem like this? Code asserts that the specific impact of feminism on epistemology has been “to move the question ‘Whose knowledge are we talking about?’ to a central place in epistemological discussion,”11 suggesting that the hope lies in finding an epistemological theory that assigns central importance to consideration of the nature of the subjects who actually do the knowing. I totally agree: No theory that abjures empirical study of the cognizer, or of the actual processes by which knowledge develops, is ever going to yield insight on this question. But more is required than this. If we as feminist critics are to have any basis for distinguishing the salutary from the pernicious forms of bias, we can’t rest content with a description of the various ways in which the identity and social location of a subject make a difference to her beliefs. We need, in addition, to be able to make normative distinctions among various processes of belief-fixation as well. Otherwise, we’ll never escape the dilemma posed by the bias paradox: Either endorse pure impartiality or give up criticizing bias.12

### The critique of objectivity is not a reason to reject it – 3 reasons

Antony, 2003 [Louise, Professor of Philosophy at the University of Massachusetts, Amherst, “Quine as Feminist: The Radical Import of Naturalized Epistemology,” ed. Lynne Hankinson Nelson and Jack Nelson, *Feminist Interpretations of W.V.Quine*, pg 127-8]

There are, in general, two strategies that one can find in the epistemological literature for challenging the ideal of objectivity as impartiality. (I leave aside for the moment the question of why one might want to challenge an epistemic ideal, though this question will figure importantly in what follows.) The first strategy is to prove the impossibility of satisfying the ideal—this involves pointing to the ubiquity of bias. The second strategy is to try to demonstrate the undesirability of satisfying the ideal—this involves showing the utility of bias. The second strategy is employed by some feminist critics, but often the first strategy is thought to be sufficient, particularly when it’s pursued together with the kind of radical critique of institutionalized science discussed above. Thus Jaggar, Code, and others emphasize the essential locatedness of every individual knower, arguing that if all knowledge proceeds from some particular perspective, then the transcendent standpoint suggested by the ideology of objectivity is unattainable. All knowledge is conditioned by the knower’s location, it is claimed; if we acknowledge that, then we cannot possibly believe that anyone is “objective” in the requisite sense. But the appeal to the de facto partiality of all knowledge is simply not going to justify rejecting the ideal of objectivity, for three reasons. In the first place, the wanted intermediate conclusion—that Dragnet objectivity is impossible—does not follow from the truism that all knowers are located. The Dragnet conception of impartiality is perfectly compatible with the fact that all knowers start from some particular place. The Dragnet theory, like all empiricist theories, holds that knowledge is a strict function of the contingencies of experience. It therefore entails that differences in empirical situation will lead to differences in belief, and to that extent validates the intuition that all knowledge is partial.49 Thus the neutrality recommended by the Dragnet theory does not enjoin cognizers to abjure the particularities of their own experience, only to honor certain strictures in drawing conclusions from that experience. Impartiality is not a matter of where you are, but rather how well you do from where you sit. In the second place, even if it could be shown to be impossible for human beings to achieve perfect impartiality, that fact in itself would not speak against Dragnet objectivity as an ideal. Many ideals—particularly moral ones—are unattainable, but that does not make them useless, or reveal them to be inadequate as ideals.50 The fact—and I have no doubt that it is a fact—that no one can fully rid oneself of prejudices, neurotic impulses, selfish desires, and other psychological detritus, does not impugn the moral or the cognitive value of attempting to do so. Similarly, the fact that no one can fully abide by the cognitive strictures imposed by the standards of strict impartiality doesn’t entail that one oughtn’t to try. The real test of the adequacy of a norm is not whether it can be realized, but (arguably) whether we get closer to what we want if we try to realize it. But the third and most serious problem with this tack is that it is precisely the one that is going to engender the bias paradox. Notice that the feminist goal of exposing the structures of interestedness that constitute patriarchy and other forms of oppression requires doing more than just demonstrating that particular interests are being served. It requires criticizing that fact, showing that there’s something wrong with a society in which science selectively serves the interests of one dominant group. And it’s awfully hard to see how such a critical stand can be sustained without some appeal to the value of impartiality.

## **AT: Objectivity link – Physics good**

### **Rejecting objectivity trades off with learning post-Newtonian physics**

Haraway, 1991 [Donna, historian of science and Professor at the History of

Consciousness Board, University of California, Santa Cruz. She received

her doctorate in biology at Yale, *Simians, Cyborgs, and Women*, pg 186 ]

I, and others, started out wanting a strong tool for deconstructing the truth claims of hostile science by showing the radical historical specificity, and so contestability, of every layer of the onion of scientific and technological constructions, and we end up with a kind of epistemological electro-shock therapy, which far from ushering us into the high stakes tables of the game of contesting public truths, lays us out on the table with self-induced multiple personality disorder. We wanted a way to go beyond showing bias in science (that proved too easy anyhow), and beyond separating the good scientific sheep from the bad goats of bias and misuse. It seemed promising to do this by the strongest possible constructionist argument that left no cracks for reducing the issues to bias versus objectivity, use versus misuse, science versus pseudo-science. We unmasked the doctrines of objectivity because they threatened our budding sense of collective historical subjectivity and agency and our 'embodied' accounts of the truth, and we ended up with one more excuse for not learning any post-Newtonian physics and one more reason to drop the old feminist self-help practices of repairing our own cars. They're just texts anyway, so let the boys have them back. Besides these textualized postmodern worlds are scary, and we prefer our science fiction to be a bit more utopic, maybe like Womall Oil the Edge of Time or even Walldergrotmd.

## **AT: Objectivity link - Realism**

### **Even if scientific value-neutrality is ideological, this doesn’t make its claims false – proving bias just proves the science is bad**

Antony, 2003 [Louise, Professor of Philosophy at the University of Massachusetts, Amherst, “Quine as Feminist: The Radical Import of Naturalized Epistemology,” ed. Lynne Hankinson Nelson and Jack Nelson, *Feminist Interpretations of W.V.Quine*, pg 123-6]

Suppose for the sake of argument that the empirical claims of the radical critics are largely correct. Suppose, that is, that in contemporary U.S. society institutionalized inquiry does function to serve the specialized needs of a powerful ruling elite (with trickle-down social goods permitted insofar as they generate profits or at least don’t impede the fulfillment of ruling-class objectives). Imagine also that such inquiry is very costly, and that the ruling elite strives to socialize those costs as much as possible. In such a society, there will be a great need to obscure this arrangement. The successful pursuit of the agendas of the ruling elite will require a quiescent—or, as it’s usually termed, “stable”—society, which would surely be threatened if the facts were known. Also required is the acquiescence of the scientists and scholars, who would like to view themselves as autonomous investigators serving no masters but the truth and who would deeply resent the suggestion (as anyone with any selfrespect would) that their honest intellectual efforts subserve any baser purpose. How can the obfuscation be accomplished? One possibility would be to promote the idea that science is organized for the sake of public rather than private interests. But the noble lie that science is meant to make the world a better place is a risky one. It makes the public’s support for science contingent upon science’s producing tangible and visible public benefits (which may not be forthcoming) and generates expectations of publicity and accountability that might lead to embarrassing questions down the road. An altogether more satisfactory strategy is to promote the idea that science is value-neutral—that it’s organized for the sake of no particular interests at all! Telling people that science serves only the truth is safer than telling people that science serves them, because it not only hides the truth about who benefits, but deflects public attention away from the whole question. Belief in the value-neutrality of science can thus serve the conservative function of securing unconditional public support for what are in fact ruling-class initiatives. Any research agenda whatsoever— no matter how pernicious—can be readily legitimated on the grounds that it is the natural result of the self-justifying pursuit of truth, the more or less inevitable upshot of a careful look at the facts. It will enhance the lie that science is objective to augment it with the lie that scientists as individuals are especially “objective,” either by nature or by dint of their scientific training. If laypersons can be brought to believe this, then the lie that scientific practice can transcend its compromised setting becomes somewhat easier to swallow. And if scientists can be brought to embrace this gratifying self-image, then the probability of their acquiescence in the existing system will be increased. Scientists will find little cause for critical reflection on their own potential biases (since they will believe that they are more able than others to put aside their own interests and background beliefs in the pursuit of knowledge), and no particular incentive to ponder the larger question of who actually is beneWting from their research.47 Now in such a society, the widespread acceptance of a theory of knowledge like the Dragnet theory would clearly be a good thing from the point of view of the ruling elite. By fostering the epistemic attitudes it fosters, the Dragnet theory helps confer special authority and status on science and its practitioners and deXects critical attention away from the material conditions in which science is conducted. Furthermore, by supporting Dragnet objectivity as an epistemic ideal, the theory prepares the ground for reception of the ideology of the objectivity of science. In a society in which people have a reason to believe that science is successful in yielding knowledge, the Dragnet theory and the ideology of objectivity will in fact be mutually reinforcing. If one believes that science must be objective to be good, then if one independently believes that science is good, one must also believe that science is objective! The Dragnet theory, taken together with propagandistic claims that science is value-neutral, etc., offers an explanation of the fact that science leads to knowledge. Against the background belief that knowledge is actually structured the way the Dragnet theory says it is, the success of science seems to conWrm the ideology. We can conclude from all this that the Dragnet theory, along with the ideal of objectivity it sanctions, has clear ideological value, in the sense that acceptance of the theory may play a causal role in people’s acceptance of the ideology of scientiWc objectivity. But we cannot infer from this fact either that the Dragnet theory is false or that its ideals are flawed. Such an inference depends on conflating what are essentially prescriptive claims (claims about how science ought to be conducted) with descriptive claims (claims about how science is in fact conducted). It’s one thing to embrace some particular ideal of scientific method and quite another to accept ideologically useful assumptions about the satisfaction of that ideal within existing institutions.48 Note that in a society such as the one I’ve described, the ideological value of the Dragnet theory depends crucially on how successfully it can be promulgated as a factual characterization of the workings of the intellectual establishment. It’s no use to get everyone to believe simply that it would be a good thing if scientists could put aside their prior beliefs and their personal interests; people must be brought to believe that scientists largely succeed in such divestitures. The ideological cloud of Dragnet objectivity thus comes not so much from the belief that science ought to be value-free, as from the belief that it is value-free. And of course it’s precisely the fact that science is not value-free in the way it’s proclaimed to be that makes the ideological ploy necessary in the first place. If science as an institution fails to live up to its own ideal of objectivity, then the character of existing science entails nothing about the value of the ideal, nor about the character of some imagined science which did live up to it. In fact, notice that the more we can show that compromised science is bad science (in the sense of leading to false results), the less necessary we make it to challenge the Dragnet theory itself. A good part of the radical case, after all, is made by demonstrating the ways in which scientific research has been distorted by some of the very factors a Dragnet epistemologist would cite as inhibitors of epistemic progress: prejudiced beliefs, undefended hunches, material desires, ideological commitments.

## **AT: Objectivity – naturalized epistemology good**

### Our naturalized realist epistemology solves the impact to the kritik and avoids our turns

Antony, 2003 [Louise, Professor of Philosophy at the University of Massachusetts, Amherst, “Quine as Feminist: The Radical Import of Naturalized Epistemology,” ed. Lynne Hankinson Nelson and Jack Nelson, *Feminist Interpretations of W.V.Quine*, pg 139]

The naturalized approach proceeds by showing the empirical inadequacy of the theory of mind and knowledge that makes perfect neutrality seem like a good thing. But at the same time that it removes the warrant for one epistemic ideal, it gives support for new norms, ones that will enable us to criticize some biases without presupposing the badness of bias in general. The naturalized approach can therefore vindicate all of the insights feminist theory has produced regarding the ideological functions of the concept of objectivity without undercutting the critical purpose of exposing androcentric and other objectionable forms of bias, when they produce oppressive falsehoods.

## **AT: Empiricism link**

### **Empiricism isn’t based on neutrality – all epistemic norms are subject to theory confirmation**

Antony, 2003 [Louise, Professor of Philosophy at the University of Massachusetts, Amherst, “Quine as Feminist: The Radical Import of Naturalized Epistemology,” ed. Lynne Hankinson Nelson and Jack Nelson, *Feminist Interpretations of W.V.Quine*, pg 129-30]

The short answer is this: because the best empirical theories of knowledge and mind do not sanction pure neutrality as sound epistemic policy. The fact is that the Dragnet theory is wrong. We know this for two reasons: First, the failure of externalism tells us that its foundationalist underpinnings are rotten, and second, current work in empirical psychology tells us that its empiricist conception of the mind is radically incorrect. But if the Dragnet theory is wrong about the structure of knowledge and the nature of the mind, then the main source of warrant for the ideal of epistemic neutrality is removed. It becomes an open question whether divestiture of emotions, prior beliefs, and moral commitments hinders, or aids, the development of knowledge. The fact that we find ourselves wondering about the value of a proposed epistemic ideal is itself a consequence of the turn to a naturalized epistemology. As I explained in the second section, Quine’s critique of externalism entailed that epistemic norms themselves were among the presuppositions being subjected to empirical test in the ongoing process of theory confirmation. This in itself authorizes the project of criticizing norms—it makes coherent and gives point to a project which could be nothing but an exercise in skepticism, to an externalist’s way of thinking.

## **AT: Mainstream/positivist/empiricist epistemology/Enlightenment thought bad**

### Feminist critics of mainstream epistemology mistakenly conflate it with empiricism and ignore the critical work of analytical philosophies in undermining absolute objectivity while maintaining a notion of realism

Antony, 2003 [Louise, Professor of Philosophy at the University of Massachusetts, Amherst, “Quine as Feminist: The Radical Import of Naturalized Epistemology,” ed. Lynne HankinsonNelson and Jack Nelson, *Feminist Interpretations of W.V.Quine*, pg 102-6]

One difficulty that confronts anyone who wishes to assess the need for a “feminist alternative” in epistemology is the problem of finding out exactly what such an epistemology would be an alternative to. What is “mainsteam” epistemology anyway? Lorraine Code is more forthright than many in her willingness to name the enemy. According to her, “mainstream epistemology,” the proper object of feminist critique, is “postpositivist empiricist epistemology: the epistemology that still dominates in Anglo-American philosophy, despite the best efforts of socialist, structuralist, hermeneuticist, and other theorists of knowledge to deconstruct or discredit it.”14 By the “epistemology that still dominates in Anglo-American philosophy,” Code would have to be referring to the set of epistemological theories that have developed within the analytic paradigm, for analytic philosophy has been, in fact, the dominant philosophical paradigm in the English-speaking academic world since the early twentieth century.15 This means, at the very least, that the agents of sexism within academic philosophy—the individuals who have in fact been the ones to discriminate against women as students, job applicants, and colleagues—have been, for the most part, analytic philosophers, a fact that on its own makes the analytic paradigm an appropriate object for feminist scrutiny. But this is not the main reason that Code and others seek to “deconstruct or discredit” analytic epistemology. The fact that the analytic paradigm has enjoyed such an untroubled hegemony within this country during the twentieth century—the period of the most rapid growth of American imperial power—suggests to many radical social critics that analytic philosophy fills an ideological niche. Many feminist critics see mainstream analytic philosophy as the natural metaphysical and epistemological complement to liberal political theory, which, by obscuring real power relations within the society, makes citizens acquiescent or even complicit in the growth of oppression, here and abroad. What is it about analytic philosophy that would enable it to play this role? Some have argued that analytic or “linguistic” philosophy, together with its cognate Welds (such as formal linguistics and computationalist psychology), is inherently male, “phallogocentric.”16 Others have argued that the analytic paradigm, because of its emphasis on abstraction and formalization and its valorization of elite skills, may be an instrument of cognitive control, serving to discredit the perspectives of members of nonprivileged groups.17 But most of the radical feminist critiques of “mainstream” epistemology (which, as I said, must denote the whole of analytic epistemology) are motivated by its presumed allegiance to the conceptual structures and theoretical commitments of the Enlightenment, which provided the general philosophical background to the development of modern industrialized “democracies.”18 By this means, “mainstream” epistemology becomes identified with “traditional” epistemology, and this traditional epistemology becomes associated with political liberalism. Feminist theorists like Alison Jaggar and Sandra Harding, who have both written extensively about the connection between feminist political analysis and theories of knowledge, have encouraged the idea that acceptance of mainstream epistemological paradigms is tantamount to endorsing liberal feminism. Jaggar contends that the connection lies in the radically individualistic conception of human nature common to both liberal political theory and Enlightenment epistemology. In a chapter entitled “Feminist Politics and Epistemology: Justifying Feminist Theory,” she writes: Just as the individualistic conception of human nature sets the basic problems for the liberal political tradition, so it also generates the problems for the tradition in epistemology that is associated historically and conceptually with liberalism. This tradition begins in the 17th century with Descartes, and it emerges in the 20th century as the analytic tradition. Because it conceives humans as essentially separate individuals, this epistemological tradition views the attainment of knowledge as a project for each individual on her or his own. The task of epistemology, then, is to formulate rules to enable individuals to undertake this project with success.19 Harding, in a section of her book called “A Guide to Feminist Epistemologies,” surveys what she sees as the full range of epistemological options open to feminists. She imports the essentially conservative political agenda of liberal feminism, which is focused on the elimination of formal barriers to gender equality, into mainstream epistemology, which she labels “feminist empiricism”: “Feminist empiricism argues that sexism and androcentrism are social biases correctable by stricter adherence to the existing methodological norms of scientific inquiry.”20 Harding takes the hallmark of feminist empiricism (which on her taxonomy is the only alternative to feminist standpoint and postmodernist epistemologies) to be commitment to a particular conception of objectivity, which, again, is held to be part of the legacy of the Enlightenment. In her view, acceptance of this ideal brings with it faith in the efficacy of “existing methodological norms of science” in correcting biases and irrationalities within science, in the same way that acceptance of the liberal ideal of impartiality brings with it faith in the system to eliminate political and social injustice. In Harding’s mind, as in Jaggar’s, this politically limiting conception of objectivity is one that can be traced to traditional conceptions of the knowing subject, specifically to Enlightenment conceptions of “rational man.” The message, then, is that mainstream epistemology, because it still operates with this traditional conception of the self, functions to limit our understanding of the real operations of power, and of our place as women within oppressive structures. A genuine feminist transformation in our thinking therefore requires massive overhaul, if not outright repudiation, of central aspects of the tradition. This is clearly the message that political scientist Jane Flax gleans from her reading of feminist philosophy; she argues that feminist theory ought properly to be viewed as a version of postmodern thought, since postmodern theorists and feminist theorists are so obviously engaged in a common project: Postmodern philosophers seek to throw into radical doubt beliefs still prevalent in (especially American) culture but derived from the Enlightenment. . . ,21 feminist notions of the self, knowledge and truth are too contradictory to those of the Enlightenment to be contained within its categories. The way to feminist future(s) cannot lie in reviving or appropriating Enlightenment concepts of the person or knowledge.22 But there are at least two serious problems with this argument. The first is that the “tradition” that emerges from these critiques is a gross distortion and oversimplification of the early modern period. The critics’ conglomeration of all classical and Enlightenment views into a uniform “traditional” epistemology obscures the enormous amount of controversy surrounding such notions as knowledge and the self during the seventeenth and eighteenth centuries, and encourages crude misunderstandings of some of the central theoretical claims. Specifically, this amalgamation makes all but invisible a debate that has enormous relevance to discussions of bias and objectivity, viz., the controversy between rationalists and empiricists about the extent to which the structure of the mind might constrain the development of knowledge.23 The second problem is that the picture of analytic epistemology that we get once it’s allied with this oversimplified “traditional” epistemology is downright cartoonish. When we look at the actual content of the particular conceptions of objectivity and scientific method that the feminist critics have culled from the modern period, and which they subsequently attach to contemporary epistemology, it turns out that these conceptions are precisely the ones that have been the focus of criticism among American analytic philosophers from the 1950s onward. The feminist critics’ depiction of “mainstream” epistemology utterly obscures this development in analytic epistemology, and in glossing over the details of the analytic critique of positivism, misses points that are of crucial relevance to any truly radical assault on the liberal ideology of objectivity.24 The second problem is partly a consequence of the first. The feminist critics, almost without exception, characterize mainstream epistemology as “empiricist.” But one of the chief accomplishments of the analytic challenge to positivism was the demonstration that a strictly empiricistic conception of knowledge is untenable. As a result, much of analytic epistemology has taken a decidedly rationalistic turn. Neglect of the rationalist/empiricist debate and misunderstanding of rationalist tenets make the critics insensitive to these developments and blind to their implications.

## **AT: Enlightenment thought bad**

### **Enlightenment thought isn’t absolutist – its about the contingency of truth and inherent partiality of knowledge**

Antony, 2003 [Louise, Professor of Philosophy at the University of Massachusetts, Amherst, “Quine as Feminist: The Radical Import of Naturalized Epistemology,” ed. Lynne Hankinson Nelson and Jack Nelson, *Feminist Interpretations of W.V.Quine*, pg 114]

“Pure” objectivity—if that means giving equal weight to every hypothesis consistent with the data, or if it means drawing no conclusions beyond what can be supported by the data—is thus a nonstarter as an epistemic norm from a rationalist’s point of view. The rationalists were in effect calling attention to the value of a certain kind of partiality: If the mind were not natively biased—i.e., disposed to take seriously certain kinds of hypotheses and to disregard or fail to even consider others—then knowledge of the sort that human beings possess would itself be impossible. There are simply too many ways of combining ideas, too many different abstractions that could be performed, too many distinct extrapolations from the same set of facts, for a pure induction machine to make much progress in figuring out the world. The realization that perfect neutrality was not necessarily a good thing, and that bias and partiality are potentially salutary, is thus a point that was strongly present in the early modern period, pace Jaggar and Flax. There was no single “traditional” model of mind; the model that can properly be said to underwrite the conceptions of rationality and objectivity that Jaggar brings under feminist attack is precisely a model to which Descartes and the other rationalists were opposed, and, ironically, the one that, on the face of it, assigns the most significance to experience. And although it is the cognitive essentialists who are charged with deflecting attention away from epistemically significant characteristics of the knower, it was in fact these same essentialists, in explicit opposition to the empiricists, who championed the idea that human knowledge was necessarily “partial.”

## **AT: Bias link**

### The neg has the burden of proving that the biases they outline actually decrease the truth of our claims

Antony, 2003 [Louise, Professor of Philosophy at the University of Massachusetts, Amherst, “Quine as Feminist: The Radical Import of Naturalized Epistemology,” ed. Lynne Hankinson Nelson and Jack Nelson, *Feminist Interpretations of W.V.Quine*, pg 136]

But what’s the alternative? A naturalized approach to knowledge, because it requires us to give up neutrality as an epistemic ideal, also requires us to take a different attitude toward bias. We know that human knowledge requires biases; we also know that we have no possibility of getting a priori guarantees that our biases incline us in the right direction. What all this means is that the “biasedness” of biases drops out as a parameter of epistemic evaluation. There’s only one thing to do, and it’s the course always counseled by a naturalized approach: We must treat the goodness or badness of particular biases as an empirical question. A naturalistic study of knowledge tells us biases are good when and to the extent that they facilitate the gathering of knowledge—that is, when they lead us to the truth. Biases are bad when they lead us away from the truth. One important strategy for telling the difference between good and bad biases is thus to evaluate the overall theories in which the biases figure. This one point has important implications for feminist theory in general and for feminist attitudes about universalist or essentialist theories of human nature in particular.

### **Biased theories can still be tested**

Antony, 2003 [Louise, Professor of Philosophy at the University of Massachusetts, Amherst, “Quine as Feminist: The Radical Import of Naturalized Epistemology,” ed. Lynne Hankinson Nelson and Jack Nelson, *Feminist Interpretations of W.V.Quine*, pg 136-7]

As we saw in section two, much of the feminist criticism raised against cognitive essentialism focused on the fact that rationalist and Kantian theories of the human essence were all devised by men, and based, Quine as Feminist 137 allegedly, on exclusively male experience. Be that so—it would still follow from a naturalized approach to the theory of knowledge that it is an empirical question whether or not “androcentrism” of that sort leads to bad theories. Partiality does not in general compromise theories; as we feminists ourselves have been insisting, all theorizing proceeds from some location or other. We must therefore learn to be cautious of claims to the effect that particular forms of partiality will inevitably and systematically influence the outcome of an investigation. Such claims must be treated as empirical hypotheses, subject to investigation and challenge, rather than as enshrined first principles.

### **No impact to bias and distortion of science if its ubiquitous and inevitable**

Antony, 2003 [Louise, Professor of Philosophy at the University of Massachusetts, Amherst, “Quine as Feminist: The Radical Import of Naturalized Epistemology,” ed. Lynne Hankinson Nelson and Jack Nelson, *Feminist Interpretations of W.V.Quine*, pg 128-9]

A similar problem afflicts the variation on this strategy that attempts to base a critique of the norm of objectivity on the androcentric features of its source. Even if it could be established that received epistemic norms originated in the androcentric fantasies of European white males (and I meant to give some reason to question this in the second section), how is that fact supposed to be elaborated into a critique of those norms? All knowledge is partial—let it be so. How then does the particular partiality of received conceptions of objectivity diminish their worth? The question that must be confronted by anyone pursuing this strategy is basically this: If bias is ubiquitous and ineliminable, then what’s the good of exposing it? It seems to me that the whole thrust of feminist scholarship in this area has been to demonstrate that androcentric biases have distorted science and, indeed, distorted the search for knowledge generally. But if biases are distorting, and if we’re all biased in one way or another, then it seems there could be no such thing as an undistorted search for knowledge. So what are we complaining about? Is it just that we want it to be distorted in our favor, rather than in theirs? We must say something about the badness of the biases we expose or our critique will carry no normative import at all.

## **AT: Bias link - Realism**

### Bias criticisms assume a false universality of truth. Dominant knowledge is bad because its false, not because its biased, which is inevitable.

Antony, 2003 [Louise, Professor of Philosophy at the University of Massachusetts, Amherst, “Quine as Feminist: The Radical Import of Naturalized Epistemology,” ed. Lynne Hankinson Nelson and Jack Nelson, *Feminist Interpretations of W.V.Quine*, pg 134-5]

But what are we anti-externalist, naturalized epistemologists to say about this? We can’t simply condemn the members of the dominant class for their “bias,” for their lack of “open-mindedness” about our point of view. To object to the hegemony of ruling-class opinion on this basis would be to tacitly endorse the discredited norm of neutral objectivity. “Biased” they are, but then, in a very deep sense, so are we. The problem with ruling-class “prejudices” cannot be the fact that they are deeply-held beliefs, or beliefs acquired “in advance” of the facts—for the necessity of such kinds of belief is part of the human epistemic condition. The real problem with the ruling-class worldview is not that it is biased; it’s that it is false. The epistemic problem with ruling-class people is not that they are closed-minded; it’s that they hold too much power. The recipe for radical epistemological action then becomes simple: Tell the truth and get enough power so that people have to listen. Part of telling the truth, remember, is telling the truth about how knowledge is actually constructed—advocates of feminist epistemology are absolutely correct about that. We do need to dislodge those attitudes about knowledge that give unearned credibility to elements of the ruling-class worldview, and this means dislodging the hold of the Dragnet theory of knowledge. But we must be clear: The Dragnet theory is not false because it’s pernicious; it’s pernicious because it is false.

### **Turn: criticizing bias locks us into the illusion of objectivity**

Antony, 2003 [Louise, Professor of Philosophy at the University of Massachusetts, Amherst, “Quine as Feminist: The Radical Import of Naturalized Epistemology,” ed. Lynne Hankinson Nelson and Jack Nelson, *Feminist Interpretations of W.V.Quine*, pg 135-6]

Whether we are talking in general about the ideology of scientific objectivity, or about particular sexist and racist theories, we must be willing to talk about truth and falsity. If we criticize such theories primarily on the basis of their ideological function, we risk falling prey to the very illusions about objectivity that we are trying to expose. I think this has happened to some extent within feminist epistemology. Because so much of feminist criticism has been oblivious to the rationalistic case that can be made against the empiricistic conception of mind at work in the Dragnet theory, empiricistic assumptions continue to linger in the work of even the most radical feminist epistemologists. This accounts, I believe, for much of the ambivalence about Dragnet objectivity expressed even by those feminist critics who argue most adamantly for its rejection. This ambivalence surfaces, not surprisingly, in discussions about what to do about bad biases, where positive recommendations tend to fall perfectly in line with the program of liberal reformism. Lorraine Code’s discussion of stereotypical thinking provides a case in point.58 Code emphasizes, quite correctly, the degree to which stereotypical assumptions shape the interpretation of experience, both in science and in everyday life. But despite her recognition of the “unlikelihood of pure objectivity,”59 the “unattainability of pure theory-neutrality,”60 and her acknowledgment of the necessary role of background theory in science, her recommendations for reforming everyday epistemic practice are very much in the spirit of liberal exhortations to open-mindedness. She sees a difference between a scientist’s reliance on his or her paradigm, and ordinary dependence on stereotypes: It is not possible for practitioners to engage in normal science without paradigms to guide their recognition of problems, and their problem-solving endeavours. Stereotype-governed thinking is different in this respect, for it is both possible and indeed desirable to think and to know in a manner not governed by stereotypes.61 But it’s by no means clear that it is possible. I sense that Code has not appreciated the depth of human reliance on theories that cannot be shown to be “derived from the facts alone.” In characterizing certain kinds of background belief and certain forms of “hasty generalization” as stereotypes, she is presupposing a solution to the very problem that must be solved: viz., telling which of the background theories that we routinely bring to bear on experience are reliable and which ones are not.

## AT: Causation link

### There are infinite numbers of causal chains based on values—in the end it’s the experts that matter

Fitzsimmons 06 Michael Fitzsimmons is a defence analyst in Washington DC. (2006): The Problem of Uncertainty in Strategic Planning, Survival, 48:4, 131-146 http://dx.doi.org/10.1080/00396330601062808

Uncertainty is not a new phenomenon for strategists. Clausewitz knew that ‘many intelligence reports in war are contradictory; even more are false, and most are uncertain’. In coping with uncertainty, he believed that ‘what one can reasonably ask of an officer is that he should possess a standard of judgment, which he can gain only from knowledge of men and affairs and from common sense. He should be guided by the laws of probability.’34 Granted, one can certainly allow for epistemological debates about the best ways of gaining ‘a standard of judgment’ from ‘knowledge of men and affairs and from common sense’. Scientific inquiry into the ‘laws of probability’ for any given strate- gic question may not always be possible or appropriate. Certainly, analysis cannot and should not be presumed to trump the intuition of decision-makers. Nevertheless, Clausewitz’s implication seems to be that the burden of proof in any debates about planning should belong to the decision-maker who rejects formal analysis, standards of evidence and probabilistic reasoning. Ultimately, though, the value of prediction in strategic planning does not rest primarily in getting the correct answer, or even in the more feasible objective of bounding the range of correct answers. Rather, prediction requires decision- makers to expose, not only to others but to themselves, the beliefs they hold regarding why a given event is likely or unlikely and why it would be impor- tant or unimportant. Richard Neustadt and Ernest May highlight this useful property of probabilistic reasoning in their renowned study of the use of history in decision-making, Thinking in Time. In discussing the importance of probing presumptions, they contend: The need is for tests prompting questions, for sharp, straightforward mechanisms the decision makers and their aides might readily recall and use to dig into their own and each others’ presumptions. And they need tests that get at basics somewhat by indirection, not by frontal inquiry: not ‘what is your inferred causation, General?’ Above all, not, ‘what are your values, Mr. Secretary?’ ... If someone says ‘a fair chance’ ... ask, ‘if you were a betting man or woman, what odds would you put on that?’ If others are present, ask the same of each, and of yourself, too. Then probe the differences: why? This is tantamount to seeking and then arguing assumptions underlying different numbers placed on a subjective probability assessment. We know of no better way to force clarification of meanings while exposing hidden differences ... Once differing odds have been quoted, the question ‘why?’ can follow any number of tracks. Argument may pit common sense against common sense or analogy against analogy. What is important is that the expert’s basis for linking ‘if’ with ‘then’ gets exposed to the hearing of other experts before the lay official has to say yes or no.’35

## AT: Barad

### **They link to the K—Barad’s use of quantum physics as a justification enshrines another different form of absolute truth**

Kukla 2008 Rebecca, University of South Florida, “Naturalizing Objectivity,” Perspectives on Science, Volume 16, number 3, Fall 2008. Project Muse http://muse.jhu.edu/journals/perspectives\_on\_science/v016/16.3.kukla.html

One would expect any bottom-up, naturalized account of objectivity to be very wary of generalizing from the epistemic ideals and ontological picture that underwrite the practices of one scientific paradigm to epistemological and ontological truth *tout court*. Yet, despite Barad's early claim that physics should not be treated as a privileged practice that provides a complete ontological picture (24), she seems oddly willing to accept that experimental quantum mechanics reveals universal, transcendental truths that are apparently independent of any natural context of inquiry and observation. She takes quantum mechanics to refute both the individualist metaphysics of classical physics, and the self-effacing, aperspectival ideals of 'classical' objectivity. But since entities are produced by 'cuts' within practice, on her account, one would expect her to say that practices other than those of experimental quantum mechanics might well involve entities **[End Page 296]** and ideals that are well-captured by this classical picture. Why assume that the ontology produced by experimental physics shows us anything about how we should understand the metaphysics of organisms, people, numbers, or any of the other things that the practices of experimental physics will not help us disclose? And whereas Daston and Galison seek to reveal the type of objectivity that strives for an erasure of the knowing subject as a limited, historically situated epistemic ideal, Barad rejects this version of objectivity outright as simply wrong-headed. But Daston and Galison offer rich examples of the epistemic productivity of the ideal of mechanical objectivity in specific contexts; why reject it rather than contextualize it? We should not be surprised if close attention to our various epistemic practices reveals an "anarchic"[11](http://muse.jhu.edu/journals/perspectives_on_science/v016/16.3.kukla.html" \l "f11) ontology, with different sets of practices disclosing different kinds of objects, and being governed by different norms of accountability to phenomena. Given Barad's commitment to naturalized objectivity, her apparent faith that quantum mechanics in particular reveals transcendental ontological truths and transcendental constraints on our norms of objectivity is baffling.

# AT: Social constructivism/reps

## **Perm solves social constructivism**

### **Social constructivist epistemologies are infinitely regressive – we should read the social and the scientific through one another rather than rejecting either one. Physical light diffraction disproves the idea of mutual exclusivity**

Barad, 2003 [Karen, ph.d. in theoretical particle physics, Professor of Feminist Studies, Philosophy, and History of Consciousness at the University of California, Santa Cruz, “Posthumanist Performativity: Toward an Understanding of

How Matter Comes to Matter,” Signs: Journal of Women in Culture and Society 2003, vol. 28, no. 3]

The move toward performative alternatives to representationalism shifts the focus from questions of correspondence between descriptions and reality (e.g., do they mirror nature or culture?) to matters of practices/ doings/actions. I would argue that these approaches also bring to the forefront important questions of ontology, materiality, and agency, while social constructivist approaches get caught up in the geometrical optics of reflection where, much like the infinite play of images between two facing mirrors, the epistemological gets bounced back and forth, but nothing more is seen. Moving away from the representationalist trap of geometrical optics, I shift the focus to physical optics, to questions of diffraction rather than reflection. Diffractively reading the insights of feminist and queer theory and science studies approaches through one another entails thinking the “social” and the “scientific” together in an illuminating way. What often appears as separate entities (and separate sets of concerns) with sharp edges does not actually entail a relation of absolute exteriority at all. Like the diffraction patterns illuminating the indefinite nature of boundaries—displaying shadows in “light” regions and bright spots in “dark” regions—the relation of the social and the scientific is a relation of “exteriority within.” This is not a static relationality but a doing—the enactment of boundaries—that always entails constitutive exclusions and therefore requisite questions of accountability.3 My aim is to contribute to efforts to sharpen the theoretical tool of performativity for science studies and feminist and queer theory endeavors alike, and to promote their mutual consideration. In this article, I offer an elaboration of performativity—a materialist, naturalist, and posthumanist elaboration—that allows matter its due as an active participant in the world’s becoming, in its ongoing “intraactivity.”

## **AT: Soc con bad/realism good**

### Representationalism is a bad starting point

Barad, 2003 [Karen, ph.d. in theoretical particle physics, Professor of Feminist Studies, Philosophy, and History of Consciousness at the University of California, Santa Cruz, “Posthumanist Performativity: Toward an Understanding of

How Matter Comes to Matter,” Signs: Journal of Women in Culture and Society 2003, vol. 28, no. 3]

Representationalism separates the world into the ontologically disjoint domains of words and things, leaving itself with the dilemma of their linkage such that knowledge is possible. If words are untethered from the material world, how do representations gain a foothold? If we no longer believe that the world is teeming with inherent resemblances whose signatures are inscribed on the face of the world, things already emblazoned with signs, words lying in wait like so many pebbles of sand on a beach there to be discovered, but rather that the knowing subject is enmeshed in a thick web of representations such that the mind cannot see its way to objects that are now forever out of reach and all that is visible is the sticky problem of humanity’s own captivity within language, then it begins to become apparent that representationalism is a prisoner of the problematic metaphysics it postulates. Like the frustrated would-be runner in Zeno’s paradox, representationalism never seems to be able to get any closer to solving the problem it poses because it is caught in the impossibility of stepping outward from its metaphysical starting place. Perhaps it would be better to begin with a different starting point, a different metaphysics.14

## **AT: Social constructivism**

### Prefer a performative and material conception of reality based on what we can observe to representationalism or social constructivism

Barad, 2003 [Karen, ph.d. in theoretical particle physics, Professor of Feminist Studies, Philosophy, and History of Consciousness at the University of California, Santa Cruz, “Posthumanist Performativity: Toward an Understanding of

How Matter Comes to Matter,” Signs: Journal of Women in Culture and Society 2003, vol. 28, no. 3]

It is hard to deny that the power of language has been substantial. One might argue too substantial, or perhaps more to the point, too substantializing. Neither an exaggerated faith in the power of language nor the expressed concern that language is being granted too much power is a novel apprehension specifically attached to the early twenty-first century. For example, during the nineteenth century Nietzsche warned against the mistaken tendency to take grammar too seriously: allowing linguistic structure to shape or determine our understanding of the world, believing that the subject and predicate structure of language reflects a prior ontological reality of substance and attribute. The belief that grammatical categories reflect the underlying structure of the world is a continuing seductive habit of mind worth questioning. Indeed, the representationalist belief in the power of words to mirror preexisting phenomena is the metaphysical substrate that supports social constructivist, as well as traditional realist, beliefs. Significantly, social constructivism has been the object of intense scrutiny within both feminist and science studies circles where considerable and informed dissatisfaction has been voiced.1 A performative understanding of discursive practices challenges the representationalist belief in the power of words to represent preexisting things. Performativity, properly construed, is not an invitation to turn everything (including material bodies) into words; on the contrary, performativity is precisely a contestation of the excessive power granted to language to determine what is real. Hence, in ironic contrast to the misconception that would equate performativity with a form of linguistic monism that takes language to be the stuff of reality, performativity is actually a contestation of the unexamined habits of mind that grant language and other forms of representation more power in determining our ontologies than they deserve.2

## **AT: Soc con link**

The kritik is a conspiracy theory – it mischaracterizes science on the basis of non-scientist perspectives—collapses into cynicism

Haraway, 1991 [Donna, historian of science and Professor at the History of

Consciousness Board, University of California, Santa Cruz. She received

her doctorate in biology at Yale, *Simians, Cyborgs, and Women*, pg 183 ]

Academic and activist feminist enquiry has repeatedly tried to come to terms with the question of what we might mean by the curious and inescapable term 'objectivity'. We have used a lot of toxic ink and trees processed into paper decrying what they have meant and how it hurts us. The imagined 'they' constitute a kind of invisible conspiracy of masculinist scientists and philosophers replete with grants and laboratories; and the imagined 'we' are the embodied others, who are not allowed not to have a body, a finite point of view, and so an inevitably disqualifying and polluting bias in any discussion of consequence outside our own little circles, where a 'mass'-subscription journal might reach a few thousand readers composed mostly of science-haters. At least, I confess to these paranoid fantasies and academic resentments lurking underneath some convoluted reflections in print under my name in the feminist literature in the history and philosophy of science. We, the feminists in the debates about science and technology, are the Reagan era's 'special interest groups' in the rarefied realm of epistemology, where traditionally what can count as knowledge is policed by philosophers codifying cognitive canon law. Of course, a special interest group is, by Reaganoid definition, any collective historical subject which dares to resist the stripped-down atomism of Star Wars, hypermarket, postmodern, media-simulated citizenship. Max Headroom doesn't have a body; therefore, he alone sees everything in the great communicator's empire of the Global Network. No wonder Max gets to have a naIve sense of humour and a kind of happily regressive, pre-oedipal sexuality, a sexuality which we ambivalently - and dangerously incorrectly - had imagined was reserved for lifelong inmates of female and colonized bodies, and maybe also white male computer hackers in solitary electronic confinement. It has seemed to me that feminists have both selectively and flexibly used and been trapped by two poles of a tempting dichotomy on the question of objectivity. Certainly I speak for myself here, and I offer the speculation that there is a collective discourse on these matters. On the one hand, recent social studies of science and technology have made available a very strong social constructionist argument for all forms of knowledge claims, most certainly and especiaIly scientific ones? In these tempting views, no insider's perspective is privileged, because all drawings of inside-outside boundaries in knowledge are theorized as power moves, not moves towards truth. So, from the strong social constructionist perspective, why should we be cnwed by scientists' descriptions of their activity and accomplishments; they and their patrons have stakes in throwing sand in our eyes. They teIl parables about objectivity and scientific method to students in the first years of their initiation, but no practitioner of the high scientific arts would be caught dead acting on the textbook versions. Social constructionists make clear that official ideologies about objectivity and scientific method are particularly bad guides to how scientific knowledge is actuaIly made. Just as for the rest of us, what scientists believe or say they do and what they reaIly do have a very loose fit. The only people who end up actuaIly believing and, goddess forbid, acting on the ideological doctrines of disembodied scientific objectivity enshrined in elementary textbooks and technoscience booster literature are nonscientists, including a few very trusting philosophers. Of course, my designation of this last group is probably just a reflection of residual disciplinary chauvinism from identifying with historians of science and too much time spent with a microscope in early adulthood in a kind of disciplinary pre-oedipal and modernist poetic moment when ceIls seemed to be ceIls and organisms, organisms. Pace, Gertrude Stein. But then came the law of the father and its resolution of the problem of objectivity, solved by always already absent referents, deferred signifieds, split subjects, and the endless play of signifiers. Who wouldn't grow up warped? Gender, race, the world itself - all seem just effects of warp speeds in the play of signifiers in a cosmic force field. All truths become warp speed effects in a hyper-real space of simulations. But we cannot afford these particular plays on words the projects of crafting reliable knowledge about the 'natural' world cannot be given over to the genre of paranoid or cynical science fiction. For political people, social constructionism cannot be aIlowed to decay into the radiant emanations of cynicism.

### **Scientific and material phenomena are real – the kritik is as bad as conservative and religious rejections of things like global warming science**

Haraway, 1991 [Donna, historian of science and Professor at the History of

Consciousness Board, University of California, Santa Cruz. She received

her doctorate in biology at Yale, *Simians, Cyborgs, and Women*, pg 185 ]

In any case, social constructionists could maintain that the ideological doctrine of scientific method and all the philosophical verbiage about epistemology were cooked up to distract our attention from getting to know the world effectively by practising the sciences. From this point of view, science - the real game in town, the one we must play - is rhetoric, the persuasion of the relevant social actors that one's manufactured knowledge is a route to a desired form of very objective power. Such persuasions must take account of the structure of facts and artefacts, as weIl as of language mediated actors in the knowledge game. Here, artefacts and facts are parts of the powerful art of rhetoric. Practice is persuasion, and the focus is very much on practice. All knowledge is a condensed node in an agonistic power field. The strong programme in the sociology of knowledge joins with the lovely and nasty tools of semiology and deconstruction to insist on the rhetorical nature of truth, including scientific truth. History is a story Western culture buffs teIl each other; science is a contestable text and a power field; the content is the form.3 Period. The form in science is the artefactual-social rhetoric of crafting the world into effective objects. This is a practice of world-changing persuasions that take the shape of amazing new objects - like microbes, quarks, and genes. But whether or not they have the structure and properties of rhetorical objects, late twentieth-century scientific entities - infective vectors (microbes), elementary particles (quarks), and biomolecular codes (genes) - are not Romantic or modernist objects with internal laws of coherence! They are momentary traces focused by force fields, or they are information vectors in a barely embodied and highly mutable semiosis ordered by acts of recognition and misrecognition. Human nature, encoded in its genome and its other writing practices, is a vast library worthy of Umberto Eco's imagined secret labyrinth in The Nallle ofthe Rose (1980). The stabilization and storage of this text of human nature promise to cost more than its writing. This is a terrifying view of the relationship of body and language for those of us who would stiIl like to talk about reality with more confidence than we aIlow the Christian right's discussion of the Second Coming and their being raptured out of the final destruction of the world. We would like to think our appeals to real worlds are more than a desperate lurch away from cynicism and an act of faith like any other cult's, no matter how much space we generously give to all the rich and always historically specific mediations through which we and everybody else must know the world.

## **AT: Social constructivism**

### Social constructivism is reductionist and ignores the relationships between the social and the material

Barad, 2003 [Karen, ph.d. in theoretical particle physics, Professor of Feminist Studies, Philosophy, and History of Consciousness at the University of California, Santa Cruz, “Posthumanist Performativity: Toward an Understanding of

How Matter Comes to Matter,” Signs: Journal of Women in Culture and Society 2003, vol. 28, no. 3]

Crucial to understanding the workings of power is an understanding of the nature of power in the fullness of its materiality. To restrict power’s productivity to the limited domain of the “social,” for example, or to figure matter as merely an end product rather than an active factor in further materializations, is to cheat matter out of the fullness of its capacity. How might we understand not only how human bodily contours are constituted through psychic processes but how even the very atoms that make up the biological body come to matter and, more generally, how matter makes itself felt? It is difficult to imagine how psychic and sociohistorical forces alone could account for the production of matter. Surely it is the case—even when the focus is restricted to the materiality of “human” bodies—that there are “natural,” not merely “social,” forces that matter. Indeed, there is a host of material-discursive forces— including ones that get labeled “social,” “cultural,” “psychic,” “economic,” “natural,” “physical,” “biological,” “geopolitical,” and “geological”— that may be important to particular (entangled) processes of materialization. If we follow disciplinary habits of tracing disciplinary-defined causes through to the corresponding disciplinary-defined effects, we will miss all the crucial intra-actions among these forces that fly in the face of any specific set of disciplinary concerns.12

## **Social constructivism alt fails**

The alt makes it impossible to collaborate with scientists-supercharges the link to realism

Kragh 1998 [Helge, professor of history of science, Arhaus University, Denmark, “Social Constructivism, the Gospel of Science, and the Teaching of Physics,” Science & Education, 7, 231-248]

Ever since its take-off in the seventeenth century, science has continually been attacked and alternative forms of understanding nature have been proposed as substitutes for the scientific world view (Toulmin 1973; Dessaur et al. 1975; Nowotny and Rose 1979). According to one tradition of anti-science, the romantic or utopian tradition, science embodies cold rationality and violates nature by interfering with her - the scientist being likened to a rapist. The romantic tradition emphasizes subjectively and intuitively gained insight and opposes the canons of objectivity characteristic of ordinary science. Whereas the romantic opposition is anti-science and aspires to establishing a radically different view of nature, pseudosciences (such as astrology and parapsychology) are not against science in the same manner, but accuse established science of neglecting or dismissing a wide range of phenomena that cannot be explained according to the orthodox theories of science. The pseudo-scientist attacks scientific monopoly rather than science per se and argues for the right to other kinds of knowledge than the one defined officially. Still other kinds of counter-movements, sometimes referred to as critical or alternative science, focus on the political consequences of science. Rather than regarding science as a liberating forcc, a Prometheus, they see il as a modern Frankenstein monster which is opposed to true democratic and human development. As indicated, the anti-science movement is extremely heterogeneous. It forms a wide spectrum including, for example, conservative religiously based criticism as well as Marxist-oriented attempts to redefine science. Vitalism, anti-intellectualism and versions of Lebellsphilosophie form one pool which historically has followed the development of modern science and is associated with the romantic opposition. Of more interest is the more recent philosophically and sociologically bascd science criticism that emerged in the 1960s, in part inspired by Kuhn's influential revolt against positivism and critical rationalism. Although Kuhn's views cannot be interpreted in favor of anti-science, Paul Feyerabend's vigorous criticism can reasonably be seen as belonging to the anti-science tradition. According to Feyerabend, science is purely ideological and executes a mental dictatorship on line with the one of the church in the Middle Ages. He therefore called for an abolition of obligatory science in schools and for a stop of government support to all science activities. Coming from a very different corner, a somewhat similar kind of anti-science is represented by left-wing philosophers and sociologists associated with the so-called Frankfurt school. According to Marcuse and Habermas, science is manipulative 'instrumental reason' and by its very nature opposed to political consciousness and human liberation. The exploitation, violence and social manipulation associated with modern science are not merely the results of wrong applications of science, but are claimed to be elements inherent in science itself. These brief remarks only sketch a few of the anti-science or counterculture positions that characterized the debate in the 1970s. Although noisy, they had relatively little influence on academic life and attempts to establish alternative forms of science - such as a new physics based on Tao philosophy or other fonus of Eastern wisdom - failed to deliver what they promised. But at the same time as the anti-science movement declined an academically more respectable variant of science criticism saw the light of day, the sociology of scientific knowledge tradition associated with the Edinburgh school's strong programme. The works of the Edinburgh and Bath sociologists marked the start of an epistemic turn in sociology of science which since then has continued and developed into modern social constructivism. Since this epistemic turn owed much to Kuhn's theory of science, and Kuhnian notions are still among the ammunition used by relativist historians and sociologists, it should be emphasized that Kuhn was never happy about the ways his theory was used. In fact, he came to disagree strongly with the social constructivist trend. Thus, in 1992 he explicitly criticized the view that 'what passes for scientific knowledge becomes, then, simply the belief of the winners'. Kuhn stated his position as follows: '1 am among those who have found the claims of the strong program absurd: an example of deconstruction gone mad. And the more qualified sociological and historical formulations that currently strive to replace it are, in my view, scarcely more satisfactory' (Kuhn 1992, p. 9). Although constructivist sociologists deny that they arc against science, and although there are significant differences between their views and those of, say, Feyerabend or Marcuse, there are also similarities. In particular, by regarding science as a social construction they deny that the scientific world view is grounded in nature and should therefore be given higher priority than any other world view. In practice, if not in theory, modern social constructivism has contributed to a revival of anti-science sentiments and a renewed polemics about the role of science in society and education. Some scientists have reacted strongly against the new form of 'higher superstition' and in general the relations between scientists and science analysts have hardened. The debate that followed the publication of Paul Gross and Norman Levitt's Higher Superstition (1994) a brilliant, provocative and caustic attack on postmodernist science studies has shown that at least some scientists have had enough of relativist sociologists' self-confident conceptions of science. The physicist Steven Weinberg and the embryologist Lewis Wolpert arc among those who have dismissed the new brand of science studies (Weinberg 1992, Wolpert 1992). The result has been an almost insurmountable barrier between the scientists and those who study science from a social and cultural perspective, unfortunately also threatening the relationship between science and more conventional forms of history and philosophy of science. As pointed out by the chemist Jay Labinger, not only are scientists in practice excluded from the purportedly interdisciplinary science studies programmes, but potential collaborators are frightened away by the extreme relativism and anti-science attitudes of many science studies scholars. As Labinger puts it, 'Trying to convince scientists to do something based on the premise that they are all wrong is not likely to be very successful' (Labinger 1995, p. 301).

## **AT: Social constructivism – Science is flexible/reflexive**

### **Science is flexible and open to new evidence – not orthodox or suppressive of ideas**

Kragh 1998 [Helge, professor of history of science, Arhaus University, Denmark, “Social Constructivism, the Gospel of Science, and the Teaching of Physics,” Science & Education, 7, 231-248]

(iii) Fallibility and corrigibility. As stressed by Allan Franklin, among others, theories of physics are not only fallible, but also corrigible. It is a matter of fact that theories are routinely proved wrong by experiments and that scientists defending a theory often accept experimental data which contradict their theory. Scientists often feel forced to abandon a theory in the light of experimental evidence, in spite of having a vested interest in the theory. They do not normally cling to orthodox theories or suppress data disagreeing with these. As Franklin has remarked: 'Among the must important kinds of experiment are those that refute a well-confirmed theory or those that confirm an implausible theory. It is an experimenter's hope to find such unexpected results' (Franklin 1990, p. 137). Such behavior seems incompatible with constructivists' notion of experiment, according to which an experiment can only be successful if it confirms the expected.

## **AT: Social Constructivism - Can’t explain physics**

Social constructivism fails – not a superior narrative and can’t explain physics

Kragh 1998 [Helge, professor of history of science, Arhaus University, Denmark, “Social Constructivism, the Gospel of Science, and the Teaching of Physics,” Science & Education, 7, 231-248]

SCIENCE AS A SOCIAL CONSTRUCTION Social or cultural constructivist science studies exist in numerous variants, some more radical than others. There are even those which arc compatible with empiricism and realism (Sismondo 1993). For the sake of brevity I ignore the differences between social, cultural, epistemic and other versions of constructivism and merely refer to 'constructivism' (which should not, of course, be confused with the didactical method of the same name). I shall here be concerned only with those versions which, in one way or other, consider science to be socially (or culturally) constructed in the strong sense that 'scientific knowledge originates in the social world rather than the natural world' (Woolgar 1983, p. 244). Insofar as constructivists acknowledge the existence of a natural world independent of social groups and mechanisms they regard it as merely a constraint that mildly influences the scientists' accounts, if at all. According to Harry Collins, an important school in the social studies of science embraces 'an explicit relativism in which the natural world has a small or non-existent role in the construction of scientific knowledge' (Collins 1981, p. 3). The epistemology characteristic of constructivists is either relativistic or agnostic, in the sense that they do not admit any distinction between true and false accounts of nature (for which reason 'true' and 'false' invariably appear in citation marks in constructivist literature). Denying the existence of an objective nature, or declaring it without interest, scientists' accounts are all there is, and it is with these accounts the constructivist sociologist is solely concerned. How, then, do scientists manage to produce their results and build up a corpus of consensual knowledge about what they call nature? Not by wrestling with nature's secrets either in the laboratory or at the writing desk, but by negotiations, political decisions, rhetorical tricks, and social power. Since truth and logic arc always relative to a given local framework, scientists' beliefs about nature are not inherently superior to those of any other group. As Andrew Pickering has concluded in his book about the social construction of the quark concept: 'The world of REP [high energy physics] was socially produced.... there is no obligation upon anyone framing a view of the world to take account of what twentieth-century science has to say' (Pickering 1984, pp. 406, 413). The constructivist view of science is clearly at odds with the conventional view favoured by almost all scientists and science educators, namely, that science is a way of understanding increasingly more about nature by following certain rules, techniques and methods that are collectively known under the somewhat misleading term 'the scientific method'. The excesses of social constructivism have been severely criticized by scientists, philosophers and non-constructivist sociologists who have argued that the constructivist project is absurd, inconsistent, or empirically unfounded (e.g., Murphy 1994; Landan 1990; Cole 1992). There is, for example, the problem of reflexivity. If there are no context independent truths, why should we believe that the constructivist account of science is better or more true than the conventional account'? After all, the constructivist claims that his arguments are superior to those of the realist and not merely superior within the constructivist framework (in which ease the claim would be tautological). Moreover, it is not clear at all what constructivists mean by the term 'social', which they often use in a way that would ordinarily include cognitive factors. Is it reasonable to say that the views of a scientist are governed by social interests when he defends a theory he believes is true? In that case all decisions in science would be social just because science takes place in a social context and the whole argument would tend to be tautological and hence empty. Petor Galison, among others, has argued against the constructivist conflation of 'social' and 'cognitive' and has shown how episodes in modern physics do not fit the constructivist programme (Galison 1987, pp. 258 9).

### **Constructivism can’t explain major scientific discoveries in physics**

Kragh 1998 [Helge, professor of history of science, Arhaus University, Denmark, “Social Constructivism, the Gospel of Science, and the Teaching of Physics,” Science & Education, 7, 231-248]

However, these and other objections seem to have had no major effect. Constructivism continues to be a popular approach among social and humanist scholars, although with signs of growing insulation and secterianism. Rather than addressing some of the more general weaknesses in the constructivist programme, I want to mention a few specific points that this approach to science studies seems unable to explain in a satisfactory way. (i) Discoveries, Physicists sometimes happen to discover phenomena that are absolutely unexpected. Examples are Rontgen's discovery of his rays in 1895 and Kammerlingh-Onnes's discovery of superconductivity in 1911. In other cases the phenomena were predicted by theory, but the experimentalists were in fact unaware of the predictions, such as was the case with Anderson's discovery of the positron in 1932 and Penzias and Wilson's observation of the cosmic microwave nloiMion in 19ti5. Such discoveries and their subsequent histories are accounted for in a natural way according to the conventional view - that the objects or phenomena exist in nature or can be produced in the laboratory - but they seem inexplicable according to the constructivist theory. Yet another type of discovery that strongly indicates that knowledge in physics reflects how nature works, and not solely how scientists negotiate, is the quantitatively precise and confirmed predictions. The famous 1846 discovery of Neptune, guided by the predictions of celestial mechanics, is a case in point and so is the determination of the eleetron's anomalous magnetic moment in the late 1940s. From the point of view of constructivism, all this must appear either a mystery, a coincidence, or the result of conspiracy.

## **AT: Social constructivism – can’t explain scientific progress**

### **Constructivism can’t explain scientific discovery**

Kragh 1998 [Helge, professor of history of science, Arhaus University, Denmark, “Social Constructivism, the Gospel of Science, and the Teaching of Physics,” Science & Education, 7, 231-248]

(li) Progress. The progress of scientific knowledge is at the same time one of the most characteristic features of science and one of the most controversial. The subject can be discussed endlessly, but can it be seriously contested that physicists in the 1990s know more about electrical currents than Faraday did in the 1830s7 Philosophical niceties apart, did Royle know as much about chemical reactions as we do? It should be uncontroversial that we can accurately predict and account for vastly more about nature than our ancestors could two or three hundred years ago, and in this simple sense progress must be considered a fact. All the same, “progressivism” has a bad reputation among constructivists, who either ignore or deny scientific progress. And no wonder, for roughly continual progress over many generations does not fit with constructivist ideas. Within this framework there is no explanation of scientific progress.

### **Stable experimental results disprove constructivism**

Kragh 1998 [Helge, professor of history of science, Arhaus University, Denmark, “Social Constructivism, the Gospel of Science, and the Teaching of Physics,” Science & Education, 7, 231-248]

(v) Stable experimental results. Social constructivists exploit to the extreme the fact that in many situations of experimental science the experimentalists disagree. However, in most or all cases this can be explained without recourse to sociological mechanisms. More to the point, the emphasis on disagreement in experiments is grossly exaggerated. Without exact documentation, I will claim that most experiments in physics do not give rise to controversies or major disagreements of the kind known from cold fusion, gravitational wave research or the Millikan-Ehrenhaft dispute concerning the existence of subelectrons. In most experiments the data quickly stabilize and consensus is achieved in an undramatic and straightforward way. Of course there are exceptions, and of course these are particularly interesting from a philosophical and sociological point of view, but to ignore the ordinary consensual experiments give a highly distorted picture of the role played by experiment in science. Moreover, the very fact that at least some experimental results are stable and constant over time and space seems difficult to explain from a constructivist perspective. The value of the gravitational constant has not changed much since the days of Cavendish, it has just become more precise; and the results do not depend the sligthest on the experimentalists' religion, nationality or cultural settings. Such permanence cannot easily be explained by the constructivist.

## **AT: Reps/Language**

### Granting primacy to language and representations renders nature as passive and immobile—there is no reason to trust language over scientific facts

Barad, 2003 [Karen, ph.d. in theoretical particle physics, Professor of Feminist Studies, Philosophy, and History of Consciousness at the University of California, Santa Cruz, “Posthumanist Performativity: Toward an Understanding of How Matter Comes to Matter,” Signs: Journal of Women in Culture and Society 2003, vol. 28, no. 3]

Language has been granted too much power. The linguistic turn, the semiotic turn, the interpretative turn, the cultural turn: it seems that at every turn lately every “thing”—even materiality—is turned into a matter of language or some other form of cultural representation. The ubiquitous puns on “matter” do not, alas, mark a rethinking of the key concepts (materiality and signification) and the relationship between them. Rather, it seems to be symptomatic of the extent to which matters of “fact” (so to speak) have been replaced with matters of signification (no scare quotes here). Language matters. Discourse matters. Culture matters. There is an important sense in which the only thing that does not seem to matter anymore is matter. What compels the belief that we have a direct access to cultural representations and their content that we lack toward the things represented? How did language come to be more trustworthy than matter? Why are language and culture granted their own agency and historicity while matter is figured as passive and immutable, or at best inherits a potential for change derivatively from language and culture? How does one even go about inquiring after the material conditions that have led us to such a brute reversal of naturalist beliefs when materiality itself is always already figured within a linguistic domain as its condition of possibility?

## Reps inevitable

### **Our scientific representations are inevitable—reality exists independent of our perspectives**

McLaughlin 09, Amy L, Florida Atlantic Unviersity, “Peircean Polymorphism: Between Realism and Anti-realism,” Transactions of the Charles S. Peirce Society, Volume 45, Number 3, Summer 2009, Project Muse. http://muse.jhu.edu/journals/transactions\_of\_the\_charles\_s\_peirce\_society/v045/45.3.mclaughlin.html

If, as Peirce claims, all thought is in signs, it is always representation of some sort.[2](http://muse.jhu.edu/journals/transactions_of_the_charles_s_peirce_society/v045/45.3.mclaughlin.html" \l "f2) The question naturally arises, then, as to what is being represented. Further one might wonder whether it is possible to any degree to "get behind" the representations to the things represented. Peirce suggests that such "getting behind" any representation is never entirely possible. He writes: "The meaning of a representation can be nothing but a representation. In fact, it is nothing but the representation itself stripped of irrelevant clothing. But this clothing can never be completely stripped off; it is only changed for something more diaphanous."[3](http://muse.jhu.edu/journals/transactions_of_the_charles_s_peirce_society/v045/45.3.mclaughlin.html" \l "f3) Thus, the best we can do is to 'strip' our thoughts of whatever seems irrelevant, and even here we cannot ever succeed entirely. We are forever bound to a representational mode. For many, the foregoing insight serves to underwrite an anti-realist position. This is not how it serves Peirce's view, of course, as he was a committed realist.[4](http://muse.jhu.edu/journals/transactions_of_the_charles_s_peirce_society/v045/45.3.mclaughlin.html" \l "f4) Peirce writes that "reality is independent … of what you or I or any finite number of men may think about it."[5](http://muse.jhu.edu/journals/transactions_of_the_charles_s_peirce_society/v045/45.3.mclaughlin.html" \l "f5) Furthermore, given Peirce's claim that this thesis about reality is a fundamental tenet of logic—inquiry would never get off of the ground unless we supposed there to be something independent of ourselves after which we are inquiring—Peirce's realism is integral to his views.

# **Misc answers**

## **Relativism bad**

Relativism assumes knowledge is available without mediation and justifies the calculation of bodies

Latour, 1993 [Bruno, philosopher, sociologist and anthropologist of science, founder of Actor Network Theory, professor and vice president of research at Sciences Po Paris, *We Have Never Been Modern*, pg. 113]

This amounts to not taking the practice of relativism, or even the word relativism, very seriously. To establish relations; to render them commensurable; to regulate measuring instruments; to institute metrological chains; to draw up dictionaries of correspondences; to discuss the compatibility of norms and standards; to extend calibrated networks; to set up and negotiate valorimeters — these are some of the meanings of the word ‘relativism’ (Latour, 1988d). Absolute relativism, like its enemy brother rationalism, forgets that measuring instruments have to be set up. By ignoring the work of instrumentation, by conflating science with nature, one can no longer understand anything about the notion of commensurability itself. They neglect even more thoroughly the enormous efforts Westerners have made to ‘take the measure’ of other peoples, to ‘size them up’ by rendering them commensurable and by creating measuring standards that did not exist before - via military and scientific expeditions.

## AFF: Cede the political link – Space science

### Space science is a key area of public deliberation and debate – otherwise political agents with agendas like industry and the military will take over and skew results to their favor

Wilholt 2006 [Torsten, “Scientific Autonomy and Planned Research: The Case of Space Science,” Poiesis and Praxis 4(4), 253-265]

A crucial problem is that science policy decisions are often achieved by processes of scant transparency to the public. In a democratic society, the public should participate in the determination of collective knowledge aims**.** In the absence of public attention, space science easily becomes a plaything for political agents who are dealing with aims and interests that have little or nothing to do with acquiring new knowledge, such as industry policy, international politics, military use, or propaganda. The incoherencies and instabilities in space science programs world-wide are not a consequence of a lack of scientific autonomy, but often result from a lack of democratic culture in science policy making**.** It is therefore of greatest concern**, also to the scientific community,** to frame decision-making processes in space policy in a more transparent way **and to generate more public attention for them. It follows that if the space science community wants to substantiate its claim to reliable support for its own chosen research projects, then it can best do so by convincing the democratic public of the following three ideas: (1) That basic as well as application-oriented research in space science aims at knowledge which is of collective interest, (2) that only the sustained, stable and coherent support of space science programs can bring research closer to attaining these knowledge aims and (3) that procedures of decision-making that forego the best available expert knowledge, which can be found within the respective scientific disciplines, constitute an irresponsible handling of limited resources.** Since it proves unfeasible to screen off the fate of space science from the sphere of politics by means of an appeal to a strong and sweeping principle of freedom of research, the future of space science will remain a political question through and through, which will ideally be resolved by means of democratic deliberation.

## **AT: Capitalism K of science**

### Anti-capitalist critiques of science aren’t a reason to reject it

Antony, 2003 [Louise, Professor of Philosophy at the University of Massachusetts, Amherst, “Quine as Feminist: The Radical Import of Naturalized Epistemology,” ed. Lynne Hankinson Nelson and Jack Nelson, *Feminist Interpretations of W.V.Quine*, pg 126-7]

A materialist analysis of institutionalized science leads to awareness of such phenomena as the commoditization of knowledge, the “rationalization” of scientific research, and the proletarianization of scientists. Such phenomena make the limits of liberal reformism perfectly clear: Not even the most scrupulous adherence to prescribed method on the part of individual scientists could by itself effect the necessary transformations. But it’s possible for even a Dragnet theorist to acknowledge these limits, and to do so without giving up the ideal of neutral objectivity. I began by considering the claim, defended by several feminist theorists, that “traditional” epistemology limits the possibilities for exposing the machinations of the elite because it endorses the rules of the elite’s Quine as Feminist 127 game. On the contrary, I’ve argued; since a big part of the lie that needs exposing is the fact that capitalist science doesn’t follow its own rules, the task of exposing the ideology of scientific objectivity needn’t change the rules. A radical critique of science and society, even if it implicates certain ideals, does not require repudiation of those ideals.

## **AT: Essentialism**

### No epistemic grounds for critiques of essentialism – you still have to prove that the empirical claims are false

Antony, 2003 [Louise, Professor of Philosophy at the University of Massachusetts, Amherst, “Quine as Feminist: The Radical Import of Naturalized Epistemology,” ed. Lynne Hankinson Nelson and Jack Nelson, *Feminist Interpretations of W.V.Quine*, pg 136-7]

So what about universalist or essentialist claims concerning human nature? I have argued that there really are no grounds for regarding such claims as antipathetic to feminist aspirations or even to feminist insights regarding the importance of embodiment or the value of human difference. Suggestions that essentialist theories reify aspects of specifically male experience, I argued, involve a serious misunderstanding of the rationalist strategy. But notice that even if such charges were true, the real problem with such theories should be their falseness, rather than their androcentrism. A theory that purports to say what human beings are like essentially must apply to all human beings; if it does not, it is wrong, whatever its origins. In fact, I think there is excellent evidence for the existence of a substantial human nature and virtually no evidence for the alternative, the view that there is no human essence. But what’s really important is to recognize that the latter view is as much a substantive empirical thesis as the Cartesian claim that we are essentially rational language-users. We need to ask ourselves why we ought to believe that human selves are, at the deepest level, “socially constructed”—the output of a conXuence of contingent factors.62

## **AT: Foucault**

### Foucault misunderstands discourse as non-material – only a focus on scientific and material realities can understand the materialization of real world phenomena

Barad, 2003 [Karen, ph.d. in theoretical particle physics, Professor of Feminist Studies, Philosophy, and History of Consciousness at the University of California, Santa Cruz, “Posthumanist Performativity: Toward an Understanding of

How Matter Comes to Matter,” Signs: Journal of Women in Culture and Society 2003, vol. 28, no. 3]

If performativity is linked not only to the formation of the subject but also to the production of the matter of bodies, as Butler’s account of “materialization” and Haraway’s notion of “materialized refiguration” suggest, then it is all the more important that we understand the nature of this production.10 Foucault’s analytic of power links discursive practices to the materiality of the body. However, his account is constrained by several important factors that severely limit the potential of his analysis and Butler’s performative elaboration, thereby forestalling an understanding of precisely how discursive practices produce material bodies. If Foucault, in queering Marx, positions the body as the locus of productive forces, the site where the large-scale organization of power links up with local practices, then it would seem that any robust theory of the materialization of bodies would necessarily take account of how the body’s materiality—for example, its anatomy and physiology—and other material forces actively matter to the processes of materialization. Indeed, as Foucault makes crystal clear in the last chapter of The History of Sexuality (vol. 1), he is not out to deny the relevance of the physical body but, on the contrary, to show how the deployments of power are directly connected to the body—to bodies, functions, physiological processes, sensations, and pleasures; far from the body having to be effaced, what is needed is to make it visible through an analysis in which the biological and the historical are not consecutive to one another . . . but are bound together in an increasingly complex fashion in accordance with the development of the modern technologies of power that take life as their objective. Hence, I do not envision a “history of mentalities” that would take account of bodies only through the manner in which they have been perceived and given meaning and value; but a “history of bodies” and the manner in which what is most material and most vital in them has been invested. (1980a, 151–52) On the other hand, Foucault does not tell us in what way the biological and the historical are “bound together” such that one is not consecutive to the other. What is it about the materiality of bodies that makes it susceptible to the enactment of biological and historical forces simultaneously? To what degree does the matter of bodies have its own historicity? Are social forces the only ones susceptible to change? Are not biological forces in some sense always already historical ones? Could it be that there is some important sense in which historical forces are always already biological? What would it mean to even ask such a question given the strong social constructivist undercurrent in certain interdisciplinary circles in the early twenty-first century? For all Foucault’s emphasis on the political anatomy of disciplinary power, he too fails to offer an account of the body’s historicity in which its very materiality plays an active role in the workings of power. This implicit reinscription of matter’s passivity is a mark of extant elements of representationalism that haunt his largely postrepresentationalist account.11 This deficiency is importantly related to his failure to theorize the relationship between “discursive” and “nondiscursive” practices. As materialist feminist theorist Rosemary Hennessey insists in offering her critique of Foucault, “a rigorous materialist theory of the body cannot stop with the assertion that the body is always discursively constructed. It also needs to explain how the discursive construction of the body is related to nondiscursive practices in ways that vary widely from one social formation to another” (1993, 46).