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The Poverty of Postmodernism

In the general culture, "postmodernism" is the ideology of a "new class" of symbol-workers who specialize in self-referential techniques for manipulating signs, images, and multiple layers of representation. Much of this symbolic work is now computer-driven and, in this sense, "artificial," or removed from the comparatively direct and unmediated material resistances of more traditional industrial labor. Resulting from, and expressing this remoteness, postmodernism is the ideology of a seemingly self-contained and free-floating mode of cultural production in which signs loosely point at other signs, never to "reality." In the general culture, postmodernism may be useful in capturing this fast media economy of replaceable images and alternating signs.

Not so in the academic culture. Here, "postmodernism" refers to a very loose and weakly policed network of antifoundationalist and antirepresentationalist criticisms of Enlightenment rationalism (Fuchs and Ward, 1994). Since science is usually seen as the dominant and driving institution of Western rationalism, many postmodern critics have targetted science's alleged instrumental reason and will to power. These critics, though diverse and multivocal, can be labelled the current "anti-science" movement in the academy.

In the academic culture, postmodernism is unequally distributed. It is concentrated in textual and conversational fields with weak organizations, few machines, and permeable boundaries around weakly professionalized groups. Such fields predominate in some social sciences and many humanities (Whitley, 1984). Internally, postmodern fields are very loosely coupled and fragmented into diverse and multicultural schools and perspectives. In addition, academic postmodernism is strongest where it overlaps with external social movements and ideological interest groups. As a result, postmodernism is virtually absent from the hard and mature sciences.

Inspired by Heidegger, Nietzsche, and Foucault, the "anti-science" movement in postmodernism1 – composed mostly of standpoint feminists, multicultural relativists, and deconstructionists – reheats some old and hostile romantic sentiments against science: Science is cold and uncaring, driven by a technological and phallogocentric

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interest in controlling Nature, and excluding marginalized Others (Fuchs, 1996a). For the anti-science movement, science follows some inherent and one-dimensional logic of instrumental control and submission. Due to its alliance with the evil powers that be, science cannot really be reformed and improved; rather, its entire style and logic of inquiry are suspect and potentially dangerous.

Not surprisingly, the anti-science movement has recently provoked an equally hostile counter-attack from some practicing scientists (e.g., Gross and Levitt, 1994). For a long time, science perceived post-modernism as little more but irritating noise from the periphery, or even outside, the academic field. However, since funds have dried up and science education has turned into a battlefield, indifference has been replaced by ideological skirmishes. The postmodern anti-science movement has, in turn, reacted with much hurt and consternation, but little understanding, of the vigor of science's counter-attack.[2]

This vigor is, in part, due to stratification (Fuchs, 1996b). Postmodernism is an attempt at upward social and epistemic control; it reduces a strong and authoritative mode of knowing, science, to a weak and contested mode of knowing, STS. Plausible chains of reduction, however, always move from the top to the bottom in the hierarchy of the sciences, say from particle physics and their TOEs (Theories of Everything) and GUTs (Grand Unified Theories) to "less fundamental" fields. Reduction becomes implausible when it cuts against the grain of that social and cognitive hierarchy. Therefore, science reacts with disbelief, outrage, and aggressive ridicule to its feminist, critical, and deconstructivist observers.

This mutual hostility and suspicion between science and its observers could be alleviated, if not overcome, if a distinction was drawn between several levels of observation (Luhmann, 1992). On a first level, science observes reality, without being aware of the implicit criteria it uses for observing. First-level observing is always "naive": It observes *what* it observes, but not *how* it manages to do so. At this primordial level, observations are trusted to reflect the world, and are attributed to that world, not to the observer.

On a second level, observers are being observed, but still within and by the science that is observing its niche of the world. This is when issues of truth and error can be raised and communicated. On this second level, observations may lose their naivete and become problematic. Their truth is then no longer simply assumed, but must be argued for against criticism and disbelief. Errors are now attributed to observers, to their inexperience, incompetence, fatigue, or blindness. The important point is that truth belongs to second-order observing.

Finally, on a third level of observing, science can be observed by sociologists, historians, anthropologists, and post-modernists. On this level, an observer can see what cannot be seen on the first two levels: the *hows* of observation and communication. Now, one can investigate possible ideological biases in science, constraints on the selection of themes and research topics, even "interests" and "power struggles." But the crucial point is that all this *cannot* be done on the first two levels, and that this inability is *not* simply a failure or unwillingness on the part of science to take postmodern STS more seriously. On the first two levels, science must explain itself as following methods, standards of rationality, and the constraints of reality itself. It cannot do otherwise; it cannot observe how it manages to observe at the same time that it observes what it observes. When one sees something, one sees some thing, but not the seeing itself. Seeing the seeing must wait for a switch to another level of observing.

This becomes more evident when we realize that, in a sense, the third level of observing science must *itself* be naive about its *own* observations and truth claims. That is, sociological and historical observations of science as, say, interest-driven power struggles cannot observe themselves as

interest-driven power struggles. They must observe themselves as more or less accurate representations of their own object, science. They cannot but assume that, on their own level, truth matters and explains the outcomes of controversies.

This remains true even if the controversies *studied by* STS are seen as under- or undetermined by evidence and Reason, political affairs, quasi-military networking, infinite regresses, or conflicts over experimental vs. rationalist forms of life. In regard to its *own* controversies, STS must assume that they are settled by evidence and arguments, not by sheer force or contingent circumstances. One can analyze the victory of the air-pump over Leviathan as something of an historical accident, but this analysis itself cannot be seen, by itself and on its own level, as nothing but an accident, not supported by reasons and evidence.

This reflexive move takes us back to levels one and two, and to the insight that science cannot observe *there* what it cannot observe there; i.e., the impact of interests, accidents, power struggles, or sexism on its knowledge. Science can explain itself only in terms of truth and rationality; it has no other mode or code of communicating about itself.

This is why the scientists react so vigorously and emotionally to STS, and especially the ideological critiques of postmodern anti-science rhetoric: Science cannot consider the possibility that it is altogether unrelated to arguments and evidence. For science, truth and objectivity are Durkheimian sacred objects – taboos that cannot simply be debunked, dropped, and replaced by something else. To be sure, on a third level one may "deconstruct" taboos, as Durkheim did in *The Elementary Forms*. That is, constructivists may question that truth and rationality are really self-explanatory, but this can itself only be done with good reasons and true statements. If not, it is not part of science.

Again, this inevitability of objectivity, or of the belief in the role of reasons and evidence in science, does not imply that one cannot investigate the impact of social forces or sexist biases on some science. However, if such analyses are to be part of science, they must themselves be done scientifically and objectively. In other words, the finding that some science is sexist cannot be sexist itself – it must, rather, be objective and supported by reasons and evidence.

What is more, if some science is indeed sexist, this can only be taken, by those practicing that science, as cause for alarm, and incentive to remove bias and do better science. Science cannot celebrate the "situatedness" of all knowledge, as standpoint epistemologies and some laboratory constructivists do when they observe some other science, not themselves. For science, a piece of knowledge is either subjective or objective; if it is subjective, it must be dropped from science, or criticized, corrected, and made objective. There is no third option; science cannot operate with distinctions such as true/false/politically incorrect, or subjective/objective/feminist.

Therefore, the claim that *all* of science, its "essential logic" or method, is sexist (or racist or logocentric) is blatantly absurd, for there is no place *within* science from which this observation could be made. If all of science is sexist, *this* observation is as well. Then, it should overcome its own bias. If it is not sexist itself, it is more objective than the rest of science. In that case, science should overcome bias. Either way, the recognition of bias can only be taken as a call to overcome bias, not as a cause for celebration of the essential "situatedness" or "context-dependence" or "localism" of all knowledge.

If bias is nevertheless seen as part of the "essential logic" of science, this can only be perceived by science as an ideological attack from the outside. When this happens, trust breaks down, and is replaced by mutual suspicions about ulterior motives. Then, the distinctions between true and false or objective and subjective are replaced by political or ideological distinctions, such as left/right or male/female or white/black. Pervasive mutual distrust about ideological or self-interested motives destroys science – this much can be seen even in fraud

trials, which localize and personalize distrust (Fuchs and Westervelt, 1996). The anti-science movement will do even more damage to science than fraud trials, for it couples distrust to *ascriptive* characteristics, such as gender or some privileged standpoint that some occupy but not others.

Postmodernism, then, rests on a confusion of levels of observation. With this fourth-level observation, we can now turn to some of its other deficits. Most of these are due to a failure to allow for variations in science. Ironically, postmodernism repeats the cardinal error of epistemology: It assumes that all of science is of one piece, that it follows some uniform "logic" of control, bias, and exclusion. Despite its trendy rhetoric, postmodernism is still squarely stuck in the idiom and tradition of epistemology. This will eventually make it obsolete: As with epistemology, one cannot do any research if nothing is allowed to vary, and everything is compressed into one "essential" logic.

In what follows, I shall "sociologize" postmodernism, and try to show that sociology can provide testable solutions to the irresolvable conundrums postmodernism shares with its direct kin, epistemology. Since I have documented the relevant evidence elsewhere, I apologize here for the numerous self-references. My basic assumption is that a sociology of science is possible, but only as a social science of science, not as postmodernism and ideological critique. Of course, postmodernism and ideological critique are still options, but in, say art or literature or morality, not in science.[3]

Is Epistemology Always Suspect?

Much of STS has been about debunking epistemology and scientific method as "not really" guiding the actual behavior of science. Laboratory ethnographers have observed, hopefully according some method, that real scientists do not actually follow textbook methods in their daily work, and that they are mundane sense-makers rather than rule-followers.

However, this debunking does not allow for enough variation. Some areas of Kuhnian normal science, where routinization is high and not many exceptions and surprises are encountered during work, actually come closer to the standard model of epistemic rule-following (Fuchs, 1992a). Here, teachers look over the shoulders of their students to see if they are doing things right. Philosophers rationalize a practice that has been repeated over and over again, that has become settled over time, and entrenched in the tools, skills, and material means of mental production. Epistemology expresses what is known in organization science as the "formal structure." The formal structure codifies parameters of practice that have been sedimented and institutionalized as the official rules and regulations of (scientific) organizations.

This formal structure surfaces mostly on the frontstages of scientific action, when novices are being initiated, when audiences are to be enrolled by official ceremonies and displays, and when historical progress is being celebrated in rational reconstructions. All organizations do this; they present their formal and official structures on frontstages for audiences and initiates (Meyer and Rowan, 1977; Fuchs, 1993a). Epistemology is part of this formal structure as "myth and ceremony." It is *not* deception or fraud, to be debunked and then replaced by some "more realistic," say constructivist, account of science. Rather, as part of organizational routines and frontstage rationalizations, epistemology is science turning outward, accounting for itself on public frontstages, and narrating its Whiggish stories of progress toward truth to its novices.

In this sense, it is not correct to say that epistemology misses actual scientific practice, or that it is a front or "fraud" concocted by scientists who want to hide the messier aspects of their work. The formal structure is just as real as the informal system; it is the organization acting in public settings, on official occasions, and perpetuating itself in socializing new generations and cohorts.

This implies that epistemology is *not* a good description of the informal system, or of science-in-the making. Research front science is too innovative and controversial for any "method of science" to prescribe neat routines of research. Here, behavior looks indeed more like the playful tinkering and pragmatic trial-and-error searching described in lab ethnographies. In other words, it is useless to ask whether epistemology is a correct or misleading portrait of scientific practice, as if there was only one such practice. Rather, one should ask under what conditions work looks more like Garfinkelian sense-making and indexical reasoning, and under what conditions it approximates Parsonsian rule-following. Once the question is rephrased in this way, it becomes also possible to connect science studies to organization science.

Once we allow for variation in science, we can explain epistemology as a rationalization of frontstage routines, better suited to some areas and some activities of science than others.

Once epistemology turns into a dependent variable, one can explain various epistemologies as the "ideologies" of intellectual employees, working in different parts of the overall structure, handling various materials, and participating in diverse social groups and settings (Fuchs, 1993b).

Is Realism or Relativism the Better Account of Science?

This is another bad question, because it cannot be answered in the form it is posed. It is like asking: Is Buddhism or Christianity the better religion? As we have seen above, *all* science must be realistic on its first two levels of observation. Here, scientists, including constructivists, must assume that their observations are attributed to the world, not the observers. If one studies the impact of interests on science, one must assume that there are such things as "interests" and "science," and that one could convince a critic with good reasons and evidence that the former impact on the latter. If one thinks that all science is forging alliances and managing coalitions, one cannot really view one's own efforts as *nothing but* establishing centers of translation and control.

That is, "relativism" can become an observational option only at higher levels of reflexivity. Simply put, one is a realist about one's own work, and a relativist about others's. No *practicing* science can be relativistic about itself *while* it is being practiced, including the sociology or history of science, and including actor-network models. One cannot practice relativism scientifically; one can only use relativistic descriptions of *another* science to demonstrate its historical contingency, social constructedness, sexual bias, or whatever.

A more promising strategy is, again, to turn realism and relativism into dependent variables. Epistemologies are the ideologies of intellectual workers in various sciences, settings, and times. This makes it possible to explain the differential distribution of realism and relativism across the academic and intellectual field. Roughly and briefly, intellectual workers become realists when their networks are wrapped around the core institutions of the formal structure, such as in teaching and textbook writing. Realism also takes time; beliefs must consolidate and condense around stable *eigenvalues* for realist confidence to emerge. Realism is supported by working pieces of routine equipment and machinery, whereas new machines typically breed more uncertainty and "constructivism."

In contrast, relativism will develop when scientific or intellectual networks are fragmented and extremely controversial. Then, the subjectivity and constructedness of all knowledge become more visible (Fuchs, 1991; 1992b). The selectivity of scientific constructs becomes more visible when there is institutionalized dissensus over foundations and basic concepts, as is the case in STS or social theory, and many other soft yet edifying fields. Relativism indicates a lack of social solidarity and cohesion; it is the philosophy of choice in multicultural and

-paradigmatic fields with little internal closure and much outside interference from social movements and interest group politics. In short, weak organizations produce relativism; strong organizations produce realism at their institutional cores, and pragmatism at their frontiers.

This move does not solve, *but dissolve*, the "problem of representation;" it turns epistemology into sociology, where philosophical puzzles can be naturalized and solved by the methods of empirical science.

Are Different Paradigms and Forms of Life Incommensurable?

Ever since Kuhn, incommensurability has fascinated STS, and is taken by language game and form-of-life postmodernists as evidence for the absence of universal standards of evidence and rationality. When viewed sociologically, however, "incommensurability" is an extremely rare and extremely improbable result of network breakdowns and organizational failures. By no means is incommensurability the normal state of affairs. It should not be used to illustrate "basic points" about science and rationality. Incommensurability results when the network ties linking scientists or intellectuals across time and space are dissolved, or when they lose their temporal continuity (Fuchs, 1993c). This is an extremely unlikely event, for without social and cognitive networks, there is no science. Without networks organizing the flow of reputational and symbolic capital, no science can be done, no reputations can be gained, and no collaborations can succeed. Incommensurability is bad for science, because it turns it into irresolvable ideological conflict between polarized and antagonistic camps. Therefore, one may assume that scientists will do anything to avoid it; it is only relativist multiculturalists who, for some reason, seem to rejoice in mutual unintelligibility.

As long as interaction and communication persist, then, incommensurability will be avoided. It is a result, not cause, of network failures and extended "structural holes" (Burt, 1992; Fuchs, 1995). This does not, of course, exclude controversies and conflicts, but in the vast majority of cases, these will not erupt in violent break-ups of the social and intellectual networks. True revolutions are as rare in science as elsewhere. As other revolutions, scientific revolutions happen as disruptions within the elites, not as mass movements from below. However, as long as teacher-student ties organize continuity over time, and as long as citations and other indicators represent ongoing communicative exchanges, incommensurability will not occur.

Is Science Rhetoric?

Not at all, but rhetoric *is* part of the frontstage work performed by all sciences in accounting for themselves. Strong sciences, however, are much more than just texts and rhetoric; they can rely on machines, powerful organizations, strong professions, numbers and statistics, inscription devices, and much more. The view that science is "nothing but" rhetoric and a form of text emerges in fields that are much weaker, in that they do only have words to convince those who utter other words. In a sense, weak fields *are* indeed little more than rhetoric; these are conversational or discursive fields that often conflate the word and the world.

Once we allow for variation, it becomes possible to explain differences in cognitive styles across various fields and areas. Textual fields worship their classics because the sacred texts are paradigm-substitutes; they allow for a modicum of solidarity (Fuchs, 1993d). A viable classics industry emerges, complete with gatekeepers and guardians of authentic meaning and true authorial intention. Since the classics are in the past, textual fields are wary of the future; they are skeptical of progress and cumulation. For them, much of culture generally is declining. Textual and hermeneutic fields merge their history and systematics, and reward "good

writing." They are often likened to literature, and separated from the sciences by a "double hermeneutic."

Textual fields pile commentaries upon commentaries, and soon think that the whole world is a text. Textual fields organize competition as deconstruction, not innovation and discovery. They are very concerned about foundations and metaphysics. Since they are loosely coupled and multiparadigmatic, their constructive operations are more visible and identifiable. "Authors" play a more prominent role here than in more scientific fields, which anonymize their producers. Pictures of authors often appear on the jackets of their books. Entire histories are written around famous authors; the early Alexander writes on the middle Marx.

Is Science Special?

A very common position in STS is that there is nothing special happening in science, since laboratory ethnographies have revealed that scientists are ordinary people doing ordinary work in ordinary social settings. There is nothing that could "demarcate" science from other forms of knowing. Science is just another form of life, on a par with the messier rest of culture.

This conclusion is premature. The finding that scientists do not normally follow some special rules of method does *not* imply that there are no sociological differences between various institutions and organizations. When compared to, say, markets and hierarchies, science is very special indeed: It generalizes the suspension of suspicions about motives, and trusts in the truth of certain procedures, such as peer review, random sampling, and reputation. In markets and hierarchies, one more routinely and more generally expects self-interest and deception (Williamson, 1975). Markets respond to opportunism with contracts and legal liability or insurance; hierarchies try to contain self-interest and deception by supervision and internal audits.

In contrast, science assumes that all errors are honest mistakes, not intentional lies, or due to an inherent and irreparable inability to know better. This separates science from ideology. In ideology, the assumption is that the opponent (the ruling class, patriarchical males, logocentric Whites) is *unable* to know better because of his ascriptive characteristics. This inability to know better is coupled to an unwillingness to learn. The only way for one's opponent to see the light is to switch standpoints, to come over to the other side.

In science, the mutual assumption of incapability and unwillingness to learn would lead to universal distrust, and an immediate breakdown of communication. It could not even be communicated – unless as a scientific communication, which would then also have to rely on generalized trust. This generalized trust, the suspension of distrust in motives and insurmountable ideological deception, is perfectly unique to science. Nowhere else does trust extend as far to strangers and procedures. This makes science very special indeed, just not in the grandiose sense that some philosophers might have had in mind.

To be sure, this generalized trust does not lead to the absence of conflict, or organized skepticism. Rather, generalized trust makes it possible to restrict conflict to genuine disagreements, and to prevent it from extending and expanding into all-out ideological suspicion. What must be avoided in science is *der totale Ideologieverdacht*.

Does Science Have an Inherent Interest in Control?

The argument that science embodies instrumental reason has a rather strange history, starting with German romanticism, through the antimodernism of Heidegger and Nietzsche, all the way to the New Social Movements and their academic wings, such as postmodern feminism and radical Green ecology. The core assumption here is that all of science is of one piece, following an

inherent "logic" or drive for technological control and manipulation. Often, this belief is coupled to rather pathetic notions of an inherent "femininity" of Nature, which is being "penetrated" by a cold and uncaring male Reason (Bauer, 1996). Who would have thought that feminism would return to "inherent femininity" and, on top of that, have the nerve to celebrate this return as critical and emancipatory?

The biggest problem for the critique of science as instrumental reason is, again, not allowing for variation. If STS has shown anything of value, it is that science has many methods and logics, tailored to specific problems, instruments, and contexts. While a drive toward control and manipulation may, to some extent, be present in some routine areas of normal and applied science, innovative areas are much too uncertain and controversial to allow for much control and intervention. Here, work is more playful, experimental, and unorthodox. Research front scientists often describe their work as following "noses" and their "hunches," not textbook methodology. A "will to beauty" is more pronounced here than a will to power; "personal knowledge" is a far cry from instrumental reason, and from the cold and uncaring apparatus of "disembodied" male minds.

To conclude, I think that postmodernism and the anti-science movement have run their course, in part because they do not ask any interesting questions, i.e., questions that can be answered by empirical research that allows for variation. Instead, they make sweeping ideological generalizations and occupy political standpoints. Science is unlikely to pay much attention to any of this and, if it does, will react with righteous consternation and angry counterpolemic. The result is ideological battles, not science.

NOTES

[1] Obviously, the anti-science movement is only a small, though noisy, part of STS. I exclude from it all those who study science scientifically.

[2] See Vol 25(2) of *Social Studies of Science* for an exchange between science and STS.

[3] This explains the often observed affinity between postmodernism and literature, especially New Literary Forms.

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