THE NORMATIVE ISSUE IN NATURALISTIC PHILOSOPHY OF SCIENCE

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1. Introduction

Philosophy of science, after the immanent criticism of historicists like Hanson, Kuhn and Feyerabend and the postmodernist challenge (Lyotard, Derrida, Rorty) of its very *raison d'être*, is in disarray. The specter of Relativism, which must destroy any methodology worth the name, looms large. While the larger field of science (and technology) studies is being consolidated as a new interdiscipline, philosophers of science are increasingly seeking solace by 'turning naturalistic'. The most ambitious naturalists promise to recover the normative (which in the post-positivist move "from methodological prescription to socio-historical description" got lost[1]), the experimental (which, *pace* Kuhn, only came to the fore after sociologists and historians entered the contemporary or historical lab[2]), and, of course, the social (whose importance the positivist, 'received view' of science tended to play down, in spite of the original Vienna Circle's sensitivity to it)[3].

In this paper, the conflicting agendas of naturalism and postmodernism are first briefly compared (section 2). Next, after some varieties of naturalistic epistemology and philosophy of science are distinguished (section 3), some of the basic building blocks of a workable naturalistic model of science are outlined (section 4). Against this background, the prospects for normative naturalism (or "hypothetical normativism" -Donald T. Campbell) are investigated (section 5). I conclude by listing some relevant functions naturalistic philosophy can have in the near future (section 6).

2. Naturalism and Postmodernism: Conflicting Agendas

It looks as if the philosophy of science had to come a long way to get basically nowhere. Only twenty-five years ago, someone irritated by "the traditional procession of 'great systems'" in other areas of philosophy (phenomenology, existentialism, Thomism, etc.), could praise the then dominant logical-positivist philosophy of science for "hav[ing] reached a considerable degree of stability, productiveness, and almost universal consensus"[4]. Noticing that "a tendency toward 'Philosophy *of* Something' is visible", this author endeavored to present philosophy of science as "a model for all philosophy" and proposed that subjects such as ethics, metaphysics and perhaps even the theory of knowledge be abandoned to reappear as "anthropology, psychology, and other specific disciplines", thus forestalling the

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current revival of naturalism[5]. Significantly, he also identified *dialectical materialists* as potential allies, whom he sought to rally... by invoking a quote from Friedrich Engels: "Was von den ganzen bisherigen Philosophie dann noch selbständig bestehen bleibt, ist die Lehre vom Denken und seinen Gesetzen -die formelle Logik und die Dialektik. Alles andre geht auf in die positive Wiseenschaft von Natur und Geschichte."[6]

Today, after Kuhn and Feyerabend sacrificed the objectivity and rationality of science on the altar of Relativism[7], the "peaceful unanimity" (W. Wimsatt) which inspired Bushkovich's confident vision of the future of philosophy of science has given way to an acute sense of crisis and a paralyzing lack of direction. Postmodernists, who take all knowledge to be power in disguise, have come to dominate the wider philosophical scene[8]. A recent proposal to dismantle departments of philosophy of science and encourage faculties of social and political studies instead should not come as too big a surprise, then[9].

From Comte to Carnap, the positivists were also *scienticists* in that science was for them not just one among other institutions within modern society, but the very embodiment of Enlightenment ideals and as such the prime lever for societal improvement[10]. Somewhere along the way from logical positivism to our present condition in science studies, a radical -postmodern- commitment to emancipation postulating, among other things, that "[t]here is no such thing as an inner logic [of scientific development] inaccessible to the non-expert"[11] has come to overshadow, if not displace, the older ('modern') concern about the perpetuation of "that precious and precarious social system of science" (Donald T. Campbell)[12], which both Polanyi-the-liberal and Bernal-the-Marxist saw endangered, although for opposite reasons[13]. This commitment, which I find sympathetic and praiseworthy *per se*, is 'postmodern' because, not unlike the 'Political Correctness' movement which is currently gaining momentum[14], it is prone to *wishful thinking*. It is one thing to devote oneself to a more democratic science, another to decree (without being in power!) that it exists here and now[15]. By camouflaging the hard realities of science (its *Eigendynamik*[16], its ever-increasing specialization[17] and differentiation[18], its "internalization" of environmental features[19] to the point of "cognitive self-sufficiency", including the fact that it is a "self-justificatory system"[20]) underneath its 'utopian' representation, postmodern critique of science is in danger of imprisoning itself in a self-created illusion. Reinterpreting the world wins over changing it, *wie gewesen*.

Naturalistic students of science, who are committed to the belief that matters of fact are as relevant to philosophy as they are to science, face the more difficult task of articulating their 'cognitive utopias' while constantly keeping in mind the host of cognitive, social, economic, political... factors that shape and constrain scientific evolution ('ought implies can'; cf. section 5). Naturalists have been criticized for not setting their own agenda and to rest content with their role as "underlaborers for science" (John Locke)[21]. I take this challenge seriously, agree that naturalistic philosophers of science should keep an eye on the whole (STS) picture, yet beg to insist with Ronald Giere that "questions about goals become more interesting when asked at a somewhat less general level" than, say, the fundamental

goals and values of science[22], which seem "unfathomable" anyway[23]. The outsider position that, for instance, Parusnikova vindicates for her "philosopher-ironist"[24], on the other hand, allows to circumvent the naturalist's 'expertise' problem. One "beauty" of making the philosophers of science redundant, she points out, would be that "people doing these social and political analyses [cf. above] would no longer suffer from the disrespect of the scientific philosophers of science ('either you know physics or you dare not touch science')"[25]. Here the agenda seems clear enough!

How ought a naturalized philosopher of science, who, with Robert Brandon, is convinced that "[w]e philosophers of science are doing very good work now", without knowing "what the hell we are doing!"[26] to respond to such postmodernist 'irritation'? After all, qua philosopher, (s)he is still expected to be knowledgeable about such things as the *irony* of postmodernist discourse![27] The easy answer, which is probably most to the point, is that *(s)he ought not*, as life is too short to care too much about postmodernists, who are but the latest brand of 'global skeptics' -read: incurable[28]- and that (s)he better get on with the job of developing a workable model of science in the hope that it be profitable to both science and society. The more polite answer will be given at the beginning of section 5.

3. Some Varieties and Aspects of Naturalistic Philosophy of Science

Most generally speaking, naturalism is, according to *The Encyclopedia of Philosophy*, "a species of philosophical monism according to which whatever exists or happens is *natural* in the sense of being susceptible to explanation through methods which, although paradigmatically exemplified in the natural sciences, are continuous from domain to domain of objects and events."[29] It repudiates the view that there exists or could exist any entities or events which lie, in principle, beyond the scope of scientific *explanation* (which many naturalists take to be the principal goal of scientific activity, although this turns out quite tricky to ascertain empirically[30]). But in other respects, naturalism is "ontologically neutral" in that "it does not prescribe what specific kinds of entities there must be in the universe or how many distinct kinds of events we must suppose to take place."[31] Naturalism, then -it should be emphasized- is a methodological rather than an ontological monism, which should not be confused with materialism and is even compatible with a variety of anti-reductionistic positions[32].

In the philosophy of science, naturalism amounts to "the view that theories come to be accepted (or not) through a natural process involving both individual judgment and social interaction"[33]. As to naturalistic epistemology, it is useful to distinguish between (at least) three varieties.

(1) The most radical variety seeks to *replace* the traditional, 'foundationalist' program in epistemology in the vein of Descartes or the British empiricists -which it takes to be a blind alley[34]- with the thesis that "while we certainly have scientific knowledge[35], and whatever norms (...) appropriate for the successful conduct of natural science, we have no philosophical theory of knowledge sitting in

judgment over the claims of natural science to determine whether they live up to a philosophically congenial analysis of justification or knowledge"[36]. The best known representative of this position is Quine, for whom naturalized epistemology "is simply to fall into place as a chapter (...) of natural science"[37]. It is the variety of naturalism which is being advocated in my book, *Taking the Naturalistic Turn* (1993), and which underlies the model of science outlined in the next section.

(2) The second, more moderate variety of naturalism wants to *transform* traditional epistemology by connecting it with the approaches and results of (evolutionary) biology, psychology, cognitive science, and the social sciences, which inspire new epistemic principles without, however, exhausting the contents of epistemology[38]. Unlike the first form of naturalism, the second one is 'rationalist' in that it still allows traditional epistemology to sit in judgment on the claims of natural science -the judgments must now be made by the practitioners of the sciences using the methods of their sciences.

(3) The third variety, in line with the general definition of naturalism given above, simply insists that the methods of the natural sciences are the only methods for acquiring a proper understanding of the world -"natural science, and all that it implies, is the most epistemically privileged activity for understanding the nature of the physical world"[39]. It is this conviction that led the positivists to adopt the position of *methodological naturalism* (or 'naturalistic methodological monism') in the *Methodenstreit* concerning the *Natur-* and *Geisteswissenschaften*: the thesis that ths social sciences and even the humanities basically have the same aims and methods as the natural sciences -advocated today by analytic social theorists such as Jon Elster[40].

The second and third varieties of naturalism do not pose a special challenge to the philosopher who insists on the quintessential importance of being able to phrase normative questions about the scientific enterprise. The first position seems to result inevitably in the dissolution of normative philosophy of science, as it replaces the traditional *justification* of knowledge by *explanation* on the one hand (which can best be handled by the natural sciences and psychology) and -to the extent that the social dimension of scientific knowledge production is fully taken into account- to *legitimation* on the other (which, at the 'object' level, is decided by the conventions of a particular culture ('interests'), and at the metalevel is best handled by the humanistic disciplines)[41]. It is this problem that advocates of 'normative naturalism' try to tackle. But before looking into their suggestions, some additional background information about the radically naturalized philosophy I have in mind is required.

4. Some Basic Ingredients of a Naturalistic Model of Science

A workable model of science and its dynamics, as a host of naturalistic philosophers have come to see it, will invoke *evolutionary considerations* to explain certain basic cognitive and perceptual competences, and will be most plausible if married to a scientific *realism* (roughly, the idea that -even the

'invisibles' in- our competent theories *refer*) which, however, may be of the constructive type (cf. infra). Since work in this area by authors such as Donald T. Campbell, Ronald Giere, Clifford Hooker, Gerhard Vollmer and Bill Wimsatt is readily available in the literature, I will refrain from presenting it systematically and in any detail here[42]. Instead I will point out some desirable features of such a model that are often neglected.

(1) *The units of analysis.* Whereas the unit of analysis in the 'received view' of science was invariably the (finished) scientific theory, the relevant unit for today's students of science must be dynamic and must be able to capture the social and material aspects of scientific work in addition to the conceptual and representational aspects. Concepts such as that of a 'scientific field' (Darden and Maull) or a 'scientific practice' (Kitcher) go a long way in the right direction, although they may still not be 'socialized' enough. A *scientific field* is being defined as an area of science consisting of: a central *problem*; a *domain* consisting of items taken to be *facts* related to that problem; general *explanatory factors and goals* providing *expectations* as to how the problem is to be solved; *techniques and methods*; and sometimes, but not always, concepts, laws and theories which are related to the problem and which attempt to realize the explanatory goals[43]. In a somewhat more conservative vein, the components of a *scientific practice* are taken to be: "language, theoretical principles, examples of experimental and theoretical work which are deemed worthy of emulation [cf. Kuhnian paradigms as exemplars], approved methods of reasoning, problem-solving techniques, appraisals of the importance of questions, meta-scientific views about the nature of the enterprise, and so forth"[44]. By incorporating these or similar notions into their model of science, naturalists can compensate the individualist bias in much of the older work in naturalized epistemology (say, Quine's) and cognitive science.

(2) *Developmental constraints.* Evolutionary epistemologists have paid lip service to Piagetian genetic epistemology, but until know they have usually failed to incorporate the 'developmental' dimension (*ontogeny*) in their models of the evolution (*phylogeny*) of science (I am presupposing here that it is plausible and possible to introduce a genotype/phenotype equivalent for science[45]). The main reason why this is important is that it allows to introduce the (quantifiable) notion of *generative entrenchment* into discussions of scientific change. Intuitively, some feature *A* (say, an axiom in an axiomatized theory) of a developing entity is more deeply generatively entrenched than another feature *B* (say, a theorem of the same theory requiring multiple derivation steps) if and only if a change ('mutation') in *A* has more 'downstream' consequences than a corresponding 'mutation' in *B*. Wimsatt's *developmental lock model* formalizes this intuition[46]. The technical relevance of this idea for epistemological notions such as syntactic character, generality, abstraction, and universality, (un)falsifiability, meaning (non-empirical) relations and the quasi-definitional status of terms is currently being explored. More generally, the ubiquity of phenomena pointing to generative entrenchment corroborates the idea that evolution, including scientific evolution,

does not and cannot proceed in arbitrary directions (and cannot therefore be 'finalized' *ad libitum*), but is often severely constrained.

(3) It has often been emphasized that on the evolutionary-naturalist view, all human knowledge is *fallible*, but this insight has not always been followed by a systematic exploration of the booming literature on the *cognitive limitations* (Herbert Simon and others) of human actors. Vis-à-vis the skeptic, it should equally be stressed that fallible as it is, our knowledge is *corrigible* as well. Thence the quintessential importance of *heuristics* for evolutionary epistemology: (As a 'net' thrown out in order to catch some aspect of (experienced) reality, any heuristic can be made to fail. One can construct classes of problems for which a certain heuristic, which works well otherwise, will systematically fail. The property of systematically producing wrong answers is called the 'bias' of the heuristic.) It often turns out that evolution has provided our perceptual and cognitive apparatus with *biases* that compensate for our perceptual and cognitive limitations. While this general insight has guided much research in 'biological' and 'psychological' evolutionary epistemology[47], the role of heuristics and biases in scientific research strategies (for instance, reductionistic modeling strategies) has not hitherto received the proper attention due to it[48]. The central problem for an evolutionary-naturalistic approach to science, as correctly identified by Campbell, is the problem of moving from the evolutionary justification of everyday perception and cognition of the "mesocosmos" (Vollmer), where 'natural selection' can replace divine Providence as warrant ("God/natural selection would not have given us eyes that regularly deceived us"), to the evolutionary justification of the competence of scientific, i.e. quintessentially *social* cognition, where, say, sociobiology teaches us that there is no reason to assume that "God/natural selection would have given us only trustworthy fellow scientists" because of the existence of sexual selection[49].

(4) The feature of naturalistic models of science which has probably intrigued most observers of the field is that its *metatheory must be reflexive* (presupposing, of course, that one does not want to rule out all metadiscourse from one's model, which would be, for instance, Henri Atlan's strategy[50]). One way of conceiving of reflexivity is by allowing the *products* of scientific activity to be fed back into the *process* that produces them[51]. If the project of an extra- or pre-scientific foundation of science (the traditional epistemological project) fails, *'virtuous' circularity* (Vollmer) would seem to provide the only way out[52]. Much of the 'traditionalist' resistance to the naturalistic project (Almeder, Ginev...) seems to be inspired by a fundamental mistrust in circularity. However, this mistrust, which stems from the fact that many of us are still under the spell of the linear, Euclidean model of justification (Nickles), is misguided. Even a cursory look at, for instance, the practice of human or 'natural' engineers (e.g., bees) should suffice to reveal that instances of *mutual support* or even *self-support* abound in natural and artificial systems! Once the feasibility of 'nonlinear' justification is granted, it becomes possible to identify, in retrospect, the 'ruse of history' that had

the logical positivists (most notably, Neurath) already adopt (mostly unwittingly) the reflexive stance[53].

(5) Finally, I should like to stress that an *evolutionary-constructivist* epistemological position, as envisaged by most of the naturalists referred to in this section, does not preclude one from also being (or attempting to be) a *scientific realist* (which, regardless of all other merits and disadvantages of the realist stance, is a more risky, but potentially also more promising, philosophical research strategy than, say, the conventionalist or the empiricist's strategy[54]). The irreconcilable opposition that is often suggested to exist between 'constructivism' and 'realism', especially by social constructivists in science studies, is mainly the product of a misconception of the status of contemporary realism. Giere, Hooker, and the 'grand old man' of evolutionary epistemology himself, Donald Campbell, have worked out varieties of the *constructive realism* envisaged here.

5. The Prospect for Normative Naturalism, i.e. Policy-relevant Philosophy of Science

In the introduction, I referred to the widespread idea that under the influence of the history and the sociology of science, science studies have shifted from the 'methodological prescriptivism' of philosophy of science to 'socio-historical description'. (One way to illustrate this contrast is to point out that while for traditional epistemologists, only *justified true beliefs* will count as genuine knowledge -and hence, as knowledge t*out court*- the sociologist of (scientific) knowledge will, following Peter Berger, consider as knowledge what in a certain community *passes for* knowledge[55].) In this section on the normative relevance of naturalistic philosophy of science -"If philosophers of science can't issue normative statements about science and scientific evolution any more, what role is left for them?- I should qualify this statement by straightening the historical record.

Firstly, with respect to logical positivism (whose view of science long dominated the scene), it should be noticed that it never (or only unwittingly) *aimed* at being prescriptive. Its 'rational reconstructions' of science aimed at *grounding the rationality of science* (cf. also Lakatos, a generation later), which is an altogether different endeavor than formulating normative advice (say, science policy advice) and does not easily and certainly not automatically translate into it[56]. If the 'received view' was indirectly normative, it was by the roundabout of the logical positivists' emulation of physics as the 'most successful' among the sciences (cf. also the cult of operationism). Popperian falsificationism (which, by the way, if literally put into practice, would immediately kill science!) has been more ambiguous on this score.

Secondly, in the sociology of science, the original thrust of debunking the 'ideological' aspects of knowledge claims which motivated sociologists of knowledge from Marx to Mannheim, has been replaced by a neutral, non-participationist stance in the current 'Strong Programme' (Edinburgh, Bath) schools in SSK. In

this sense, recent calls to "recover the normative" aim to revive the original 'normative' tradition[57].

The current calls for a re-edition of the Baconian "meliorative project" (Kitcher), then, are more appropriately viewed as attempts -by philosophers and sociologists alike- to adjust to the new needs of post-war, regulated ('Big) science, and cannot be simply dismissed as mere 'survival strategies' of philosophers of science on the verge of extinction, as some sociologists of science (e.g., Bruno Latour[58]) would have it[59].

The *feasibility* of the project of 'recovering the normative' seems conditioned, among many other things, on the possibility of 'finalizing', i.e. operating external interventions in the ongoing scientific process (in the lingo of Wimsatt's "developmental lock model" and evolutionary biology, this could be likened to 'directed mutations' in the gene pool of the relevant population). Cast in relatively conservative epistemological terms, this possibility is provided by the circumstance -highlighted by the historical school in the philosophy of science- that *scientific theory tends to be underdetermined by the empirical evidence.* This allows for all sorts of 'nonepistemic' (e.g., cultural, political, moral, religious, social) values to 'slip in' and to impinge on scientific development (the level of the simplest individuated units, say, the representations held by individual scientists) and, hence, on scientific evolution (the population level where the individual representations are being confronted)[60]. On this view, methodological (microlevel) advice and science policy (macro level) advice are about the regulation and control of this 'external input', taking into the developmental constraints on the process[61].

The abandonment of the normative ambition of epistemology/philosophy of science advocated by Quine, Rorty, and, in science studies, by a philosopher-historian like Larry Laudan, is thus made *optional*, and normative naturalism (or Campbellian "hypothetical normativism") can thrive[62]. *In principle*, that is, because it is altogether unclear right now how much generality can be achieved in matters methodological[63]. As to the more global domain of science policy advice, Ronald Giere has pointed out that if the evolutionary model of science is taken seriously, then "science policy analysis is more like applied ecology than management science. The task is not to exploit the relevant social scientific laws by devising and enforcing rules that scientists should follow, but to design an environment conducive to optimal evolutionary development given the normal range of cognitive abilities found in the typical scientific community."[64] Some samples of such advice have been provided by Campbell, and could probably be specified in much more detail provided sufficient 'context' is provided[65]. At any rate, taking evolution (biological and cultural) seriously should remind us that 'global optimization' of the cognitive enterprise, as envisaged by Kitcher (and possibly Fuller), if it is to be had at all, will be little more than a (misleading) name, given the multidimensionality of the optimization problem, the many unknowns and *imponderabilia*[66].

6. Conclusion: Relevant Roles for Naturalized Philosophers of Science

To round off this paper, I want to list five roles which, in the light of the preceding discussion, philosophers of science can (continue to) play with some benefit in my opinion:

(1) a *productive* role: they can contribute substantially to (theoretical) science. On the naturalistic view, the border between 'science' and 'philosophy' is fluid and should remain so (Hull). The 'danger' that "philosophers of science would (...) become professionals participating in the internal scientific debates concerning the problems *scientists* encounter"[67] exists only in the eyes of essentialist demarcationists;

(2) a *parasitic* role, consisting in conceptual and logical clarification, 'gutting' arguments, and methodological criticism. The label 'parasitic' for this role, which philosophers of science have of course been playing all along, has first been vindicated, as far as I know, by the Belgian social philosopher Philippe Van Parijs. Parusnikova, our postmodernist, suggests that "appear[ing] as parasites on science, as a scientific froth"[68] is not correct, somehow; but the evolutionary epistemologist can point out that nature abounds with cases of parasitism and mutualism in 'evolutionary stable equilibrium'! Granted, invoking "the productive interaction which goes on between scientists and philosophers of science", which strikes her as "a quintessentially modernist objection", will not impress her ("its assumptions and approach are simply rejected outright by postmodernism")[69], but then who cares? We can decide at any time to stop playing her game, for instance when we get bored!

(3) a *therapeutic* role with respect to scientific strategy. Here, the philosopher of science rationally reconstructs some scientific episode with a view to drawing methodological morals from it. 'Reconstruction' is to be understood here in the sense of rational decision theory (Wimsatt);

(4) a *unificatory* role, viz. as 'umbrella' for the various other disciplines which together constitute science studies. Again, such an undertaking is anathema to the postmodernists. But, as the discussion between Latour, Kitcher and Giere shows, sociologists act as unifiers too, even if they are ill prepared to admit it[70];

(5) finally, philosophers of science can go on playing a *consumptive* role (Goldman) by investigating various philosophical uses to which scientific results might be put. Evolutionary biology or cognitive science would be prime examples. If I understand them well, this function actually comes close to what postmodernists have in mind when talking about "provid[ing] new, fresh, and challenging insights into [various sophisticated scientific and cultural practices], which scientists or artists are not capable of making on their own, being too deeply immersed in their narrow field of specialization ('games')"[71].

So, after the smoke screen has been lifted, the prospect for collaboration between some representatives of the 'two cultures' may not look so dim after all.

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Notes

1 Pollak 1983.

2 E.g., Franklin 1986; Clarke and Fujimura 1992.

3 Fuller 1992; cf. Kitcher 1993. Steve Fuller likes to suggest that the distance between Kitcher's position ("conservative naturalism") and his own "social epistemology" is immense (Fuller 1994a, 1994b). Yet to my knowledge, Fuller *and* Kitcher (maybe together with Alvin Goldman) are the most obvious emulators of the Baconian "meliorative project" for epistemology -methodological reflection aiming to improve human cognitive performance- one is likely to find in science studies today.

4 Bushkovitch 1970, 307.

5 Elsewhere, I have documented the development from logical positivism to historicism to (neo)naturalism in some detail (Callebaut 1993, ch. 2); cf. Kitcher 1992.

6 Engels 1962, 207, quoted in Bushkovitch, ibid. There are intriguing parallels between logical positivism and Marxism (e.g., Uebel 1991), which, one hopes, would finally be systematically elaborated, now that Marx is becoming *salonfähig* again (Droit 1994).

7 See, for example, Duerr 1980-1981 and Munévar 1991. Officially, relativism in Science and Technology Studies (STS) and in the sociology of scientific knowledge (SSK) is anti-authoritarian in that it "reject[s] any possibility of defending the universal 'validity' of scientific discourse itself, and its supremacy over other kinds of discourse" (Parusnikova 1992, 22). In practice it goes hand in hand with a lot of *science bashing* (cf. Segersträle 1994), even if this goes counter to the professed self-image of many relativists (references in Lynch 1994, 198) and contradicts postmodernism's holy principle of *tolerance*.

8 See, for example, Forum für Philosophie Bad Homburg 1987, or Lawson and Appignanesi (1988). As George Levine points out, literary critics and theorists nowadays share a certain discourse "so comfortably" that they do no longer question or maybe even recognize the *peculiarity* of their assumptions, viz. that (i) all we have is representation (which always implies a politics that is disguised and "naturalized"); (ii) all knowledge is culturally constructed (even so-called "facts" are ideologically loaded); hence (iii) all knowledge is a play for power not for truth, although often in the name of truth (Levine 1981, 365). Viewed, say, from the perspective of the average natural scientist, these are peculiar assumptions indeed. Regardless of the well-foundedness of these assumptions, there is a problem of communication between "the two cultures" (C.P. Snow) here which no naturalist can disregard.

9 Parusnikova 1992, 31. It is rather ironic that this author should acknowledge the Karl-Popper-Stiftung for research support!

10 Duerr 1980-1981; Callebaut 1994.

11 The specific wording is taken from the document, "Science & Technology Studies in the University of Oviedo" (Unit for Science & Technology Studies, Autumn 1993, 3), but I take it to be representative of the field.

12 In Callebaut 1993, 5.

13 Michael Polanyi was committed to a belief in"the highest possible coordination of individual scientific efforts by a process of self-coordination", which external interference ('science policy') could only harm (Polanyi 1962, 56). According to John Desmond Bernal, science was being abused by "destructive" military and economic applications and beleaguered by "mysticism and abandonment of rational thought"; to "clear" science from justified accusations, society had to reorient it toward more "constructive" ends (Bernal 1939, 1-3).

14 Duerr 1994.

15 Schwarz and Thompson 1990; Funtowicz and Ravetz 1993.

16 It seems generally accepted today that scientific evolution is not deterministic in that it could have moved along alternative evolutionary trajectories. Yet this does not contradict the evidence that the evolution of science is highly constrained (or, what amounts to the same thing, the alternatives are not equiprobable). Cf. section 4 on "developmental constraints".

17 Stichweh 1984.

18 Luhmann 1990; Stichweh 1984.

19 Shapere 1984.

20 Ginev 1993, 215-217.

21 Fuller 1994b, 962.

22 Giere 1989, 383.

23 Callebaut 1993, 216-217.

24 Parusnikova 1992, 26f.

25 Parusnikova 1992, 31.

26 Brandon in Callebaut 1993, 446.

27 See most notably Rorty 1989. For Fredric Jameson, irony is "the supreme theoretical concept and value of traditional modernism and the very locus of the notion of self-consciousness and the reflexive"; its survival in postmodernism is, then, "[t]he ultimate irony" (Jameson 1991, 258-259).

28 Viz., global skepticism can neither be proved nor disproved; worse, for all purposes, it makes no difference in our lives (see, e.g., Callebaut 1993, 186, 213, 301).

29 Danto 1972, 448.

30 Callebaut 1995.

31 Danto, ibid.

32 Callebaut 1993, ch. 4 and 8; Callebaut 1995.

33 Giere 1988, 7.

34 See, e.g., Giere 1988. Quine, in particular, has emphasized the sterility of the foundationalist program.

35 This assumption goes counter to the 'global skepticism' alluded to above.

36 Almeder 1990, 263.

37 Quine 1969, quoted in Callebaut 1993, 203.

38 See most notably Goldman 1986; 1992; cf. Ginev 1993.

39 Almeder, ibid.

40 Callebaut 1993, 202f.

41 Fuller 1988, after Quine and Rorty. As Kuhn put it, "as in political revolution, so in paradigm choice -there is no standard higher than the assent of the relevant community. To discover how scientific revolutions are effected, we shall therefore have to examine not only *the impact of nature and of logic*, but also *the techniques of persuasive argumentation* effective within the quite special groups that constitute the community of scientists". (Kuhn 1970, 102; italics mine.)

42 See in particular Hooker 1987, Giere 1988, and Griesemer and Wimsatt 1989; additional references in Callebaut 1993, ch. 7 and passim. In the Spanish literature, 'mainstream' evolutionary epistemology à la Konrad Lorenz and the Popperian brand of an evolutionary theory of science are critically discussed in Ursua (1993).

43 Darden and Maull 1977, after Shapere and Toulmin; additional references in Callebaut 1993.

44 Kitcher 1983, 163.

45 See Griesemer and Wimsatt 1989, and Callebaut 1993, ch. 7 and 9.

46 See the discussion in Callebaut 1993, sections 7.4 and 9.1.

47 See, for instance, the work reviewed by Goldman 1986, 1992.

48 Lindley Darden (Darden 1991), Thomas Nickles, and, again, Wimsatt (see Nickles and Wimsatt in Callebaut 1993) ought to be mentioned as the happy exceptions here.

49 Campbell in Callebaut 1993, 295.

50 Atlan 1986.

51 Luhmann 1990.

52 Giere 1988.

53 "In their pervasive admiration for the achievements of science at its best, logical positivists had already made the crucial assumption of the efficacy of induction as practiced by science." (Campbell 1985, 15).

54 Hooker 1987.

55 Lynch and Fuhrman 1991, 234.

56 Cf. Campbell 1985.

57 Lynch and Fuhrman 1991; Fuller 1992; Lynch 1994.

58 See, for instance, his contribution to Callebaut 1993.

59 For an earlier attempt to recast theories of knowledge so as to make them relevant to policy issues, see Callebaut et al. 1979-1980.

60 Cf. McMullin 1983.

61 Note that on Wimsatt's view, 'innate' features, in the sense of deeply generatively entrenched features, do not have to be located inside the developing system; pervasive causal influence can be exerted from the outside as well.

62 Campbell 1985; Callebaut 1993, 227-230. In addition, there is also "room and need for a rather base philosophical activity from which science could profit tremendously: reconstructing and clarifying scientific debates and controversies through the application of elementary methodological principles" (Sloep 1993, 246-247), as Peter Sloep has shown with respect to the uses of Popperian falsificationism in community ecology.

63 See, for instance, the conflicting positions of Giere, Nickles, Sober and Wimsatt as exposed in Callebaut 1993.

64 Giere 1991, 522, quoted in Callebaut 1993, 171.

65 (1) "For currently important problems, never let a single laboratory have the sole funding. Deliberately fund competitors." (2) "Be wary of research being done by scholars in fields where no other scholar will challenge their conclusions." (Social status relations should be such that scientists are free to disagree with each other without economic jeopardy.) (3) "Fund most those problems were scientists have the

means for changing each other's beliefs, that is, for convincing a fellow scientist that he or she was wrong." (Note that the required *variation* is always at the expense of the also required *retention*.) (Campbell 1985, 17).

66 Cf. also Murphy 1992.

67 Parusnikova 1992, 29.

68 Parusnikova 1992, 30.

69 Parusnikova 1992, 28.

70 In Callebaut 1993, sections 3.6 and 7.2.

71 Parusnikova 1992, 27-28.

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