Philosophica 60 (1997, 2) pp. 57-73

IN THE WAKE OF THE WINNOWER: DONALD T. CAMPBELL AND THE SOCIOLOGY OF OBJECTIVITY

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1. *Introduction*

As a graduate student during the late 1960s, I - like many of you - had Don Campbell as a virtual teacher. His influence was most salient in my methods courses. We read, studied, and were examined on unobtrusive or nonreactive measures, quasi-experimental designs, multiple operationism (better known, perhaps, as [methodological] triangulation), internal and external validity, convergent and disciminant validation, regression-discontinuity design, and more. More generally, much of what made social science (and in particular sociology and social psychology) *science* for my mentors could be traced to the contributions of Don Campbell. But it wasn't clear to them - or to me at that time - that the science *Don* was helping or trying to put into social science had two important features. First, it reflected and was relevant to the diversity and complexities of social life and culture. Second, it reflected a sophisticated post-positivist, anti-foundational understanding of the physical and natural sciences (hereafter, simply science for short) inheritance as complex, dynamic, and processual social and cultural phenomena. For this reason, it may be that Don was partially responsible for putting me on the road to a critical sociology of science and a sociology of objectivity.

It was the sophisticated understanding of science as a human activity reflected in Don's early contributions to methodology that contained the seeds of his later contributions to the philosophy, psychology, and sociology of science. He rejected definitional operationalism and advocated multiple operationism; he struggled to reconcile opposition between behavioral and phenomenological inquiry; he did not merely and simplistically adopt the methods of science, but adapted them to the settings of social and cultural activities and processes. Don defended a critical epistemological relativism and described himself as a fallibilist and anti-foundationalist opposed to direct realism, naive realism, epistemological complacency, and ontological nihilism. And how he relished describing himself as a *critical* realist.

Don's research agendas and career seem in retrospect to have led him inexorably to develop a social science of science. It is here that our agendas overlapped and our careers intersected. And it is here that I want to focus in the following remarks. My objective here is to honor Don's memory not by worshipping but by critically thinking about and engaging his ideas in a continuation of at least one part of his agenda.

2. *Wild characters and selective winnowers*

In a handwritten note Don wrote to me many years ago, he referred to me as a "wildman" and to himself as a "winnower." I suppose Don's characterization of me had something to do with a lecture I'd given, or a paper I'd published. But most likely it was provoked by my "performance" at the 1981 Lake Cazenovia workshop where I was wrongly perceived by Don as leading a constructivist assault on the philosophers of evolutionary epistemology. Whatever the source of his personology, it struck me as at least interesting that he had seen past my tranquil mediterranean exterior through to the Nietzschean turmoil boiling away inside of me. He would have many occasions during the course of our interactions to rehearse this descriptive personology. This personalized his famous slogan, "blind variation and selective retention." But I would later be surprised to find that in characterizing me, Don had put me in company with Hansen, Polanyi, Popper, Toulmin, Kuhn, Feyerabend, and Quine - all, in Don's (Campbell, 1988: 316) view , "wild characters." And yet, in recalling Don's defense of "oddball" methods as part of his triangulation approach, his speculations on the experimenting society, his anti-foundationlism, his epistemological relativism, his humanism, his cautious affection for Paul Feyerabend, we should all be reminded that there was at least a little bit of wildness in Don Campbell and in his theories.

Don was more like the sociologists of science he liked to engage than he sometimes seemed to think. He made much of the fact that he was not an ontological nihilist. But there were no ontological nihilists among the sociologists of science he disputatiously engaged. When he wrote (Campbell, 1988: 290-91) that "if pure or applied social studies are to merit the term scientific, their problem areas will have to be 'colonized' from the successful sciences," and that "Such colonization will be dependent upon a valid theory of the social system of validity enhancing belief change of the successful sciences," he defended a version of a key tenet of Bloor's (1976: 141) strong programme: "Only proceed as the other sciences proceed, and all will be well." In general, Don tried to engage sociologists of science who shared his respect for science. But these sociologists of science were, as Don knew and lamented, absolutely unwilling to seriously consider (or consider at all) issues such as "the experimental society" and enhancing validity. There are a few sociologists of science - I count myself, Daryl Chubin, and Julia Loughlin among them (Chubin and Restivo, 1983; Restivo and Loughlin, 1987) - who have considered the kinds of questions Don wanted them to. But their (our) role in his disputatious community was marginalized by their (our) failure to be awed by or worshipful of modern science. At issue in these differences were questions of the epistemological relevance of the (internalist) sociology of science (ERISS).

3. *ERISS and civilization*

The epistemological relevance of the sociology of science can be established in at least three ways. First, the sociology of science can link sociological and epistemological rationality, and so link the standards of what constitutes "correct" sociological work with the standards of science. This is one way of formulating what Bloor christened the "strong programme in the sociology of knowledge."

The second way in which a sociology of science can be made epistemologically relevant is by focusing attention on the social structural requirements of scientific practice and progress. I take this to be the goal of Don's work on descriptive evolutionary epistemology. Don's approach shares a demarcationist perspective with the strong programme (that is, it distinguishes and separates science from other modes of inquiry), but it is based on a more explicitly dynamic conception of scientific process and change. For this reason, I called this the "mild program in the sociology of science." Don (Campbell, 1988: 503), for example, referred to the "iterative oscillations of theoretical emphases" and to a "continual dialectic that never achieves a stable synthesis;" but he remained, like Merton, Kuhn, Bloor, and others committed to the Grand Paradigm of modern science: "...of all the analytically coherent epistemologies possible, we are interested," Don (Campbell, 1988: 393) wrote, "in those (or that one) compatible with the description of man and of the world provided by contemporary science."

Don (Campbell, 1988: 447) described himself as an "ontological realist, positing and seeking a reality shareable by all knowers, but which can only be known presumptively and indirectly." One of the fundamental queries of the mild program is: "In what kind of world would what kind of procedures lead a knowing community to improve the validity of its model of the world?" (Campbell, 1977: 22). Don (Campbell, 1974: 153, 158) argued that in order to fit theories and the world described (just as in fitting organisms and environments) "a wasteful nonprescient variation (blind variation) and selective retention process is required:"

> The variations are, to be sure, bound to be restricted. But the wider the range of variations, the more likely a novel solution. The recommendation to speculate wildly thus belongs in the guide book to the *strategy* of discovery, if not in the logic.

Of course, selective retention is blind too since there are a variety of levels of, contexts for, and criteria for selection. Don (Campbell, 1988: 399-418, 476) did of course consider the problem of levels, but I'm not sure he stressed this idea sufficiently.

Don (Campbell, 1974: 196) argued that the idea of "stubborn facts that speak for themselves, independently of any scientist's whim [is] is some sense literally untrue;" it is an "ideology." This sounds compatible with constructivism, but Don believed that the ideology of facticity is "an extremely important norm to preserve, and one that has a functional truth." Don thus always had one foot in the Mertonian-Kuhnian camp (in his defense of a functionalist theory of science) and one foot in the Feyerabend camp, the camp of dadaist or anarchist inquiry (in his defense of "wildness" or "unjustified variation"). But Feyerabend, Don complained, "is so in love with *variation* as to totally neglect *selection* and to see *retention* only as variation's enemy." That this is not a fair reading of Feyerabend (1978) is illustrated by a careful reading of *Against Method*, and especially the closing pages; but Don did have a cautious affection for Feyerabend's wildness, and I daresay the wildness of some of his other colleagues.

The third way in which the sociology of science can be epistemologically relevant is by drawing attention to problems of authority and competence in the process of discovering and justifying scientific knowledge (something Derek Phillips pointed out a long time ago). More broadly, it can lead to a critique of science and to consideration of alternative modes of thought and inquiry. This is the basis for a critical sociology of science.

Numerous hidden and uncritically accepted assumptions guide the selection of problems, research methods, explanatory modes, and the legitimation of logics and rationalities in the sociology of science. Research advances clandestinely, supported by assumptions about the nature of reality, the legitimate ways to study reality, and the legitimate ways to explain reality. Methods and theories are developed, utilized, and changed with scarce regard for what they imply about how one comes to know things, what one does with such knowledge, the nature of the researcher, the social relations of research (in the critical, introspective, and unobstructive reflexive sense), and the values and ethics of research. It is a small - although non-obvious- step from the idea of hidden or taken-for-granted assumptions to the idea that methods and modes of explanation, problem-selection criteria, and rationalities and logics are imbedded in world views. This insight is implied in one of the various interpretations Kuhn gives to "paradigm," and in some of the literature in the post-Mertonian/Kuhnian sociology of science.

For example, the "sequence of widening perspectives" sketched in Radnitzky's (1970) call for metascience studies carries us toward a worldview analysis. Hooker (1975) is more explicit in construing philosophies of science (such as empiricism and realism) as world views. Meta-philosophy is then the process of making the world view associated with any given philosophy of science explicit. The central idea in David Bohm's (1976) book on *Fragmentation and Wholeness* is that scientific theories are world views. Bohm conceives of world views as being concerned with all aspects of our lives - nature, ourselves, and our relations to others and to nature. For Bohm, the function of metaphysics is to unveil the world views in theories. Feyerabend's Galilean studies and critique of method are yet another affirmation and exemplar of the meta-analysis of science-as-worldview. Bohm has another way of putting all this: science and scientific theories are "insights;" and insights are neither true nor false. They are clear in some domains (and perhaps in some time frames), and unclear when extended to other domains.

I would like to say a little bit about critical sociology of science, the program I have pursued as an alternative to the various strong programmes, but which at the end of the day has much in common with Campbell's evolutionary epistemology and even more perhaps with Hooker's (1987) evolutionary naturalism. The divergences, however, are not without significant consequences.

4. *Critical sociology of science*

Critical sociology of science is based in part on the nature and implications of the meta-analysis of science, following Radnitzky, Hooker, and Bohm. It changes the focus of the sociology of science from Science (the Grand Paradigm of modern science) to the broader epistemic activity, "inquiry" (and what Nietzsche referred to as "thinking"). And it replaces such terms as "objective statement" and "truth" with the term "insight." It operates under three assumptions: (a) no insight (more broadly, "world view;" more narrowly, "objective fact," or "truth") can ever be final or absolute; (b) no system for arriving at insights can ever be universally valid and unchanging in its foundations; (c) there is always a broader context, or higher level, for establishing an insight than that of any given system of inquiry; and (d) all insights are rooted in locations or standpoints. Since these assumptions are not proposed as "truths," no truth paradoxes arise here.

As a critical sociologist of science, I do not - and perhaps cannot - affirm *a priori* that science is a privileged mode of inquiry. In this respect, CSS is more radical than the marxist program with which it shares many assumptions and theoretical guidelines. And it is compatible with conflict sociology insofar as that program is materialist and constructivist. Critical sociology of science does not focus on science-as-it-is or "speak" of science in the grammar of the ever-present tense. Science is not assumed to be an immanent (let alone transcendent!)

process that can only be facilitated or obstructed by the sociocultural context within which it unfolds. Rather, we are concerned with inquiry and thinking as unfolding human activities with evolutionary and devolutionary tracks; therefore, progress is not left out of our vocabulary, but it is not assumed and it is not universalized. Critical sociology of science is grounded in explicitly non-elitist and liberatory values, reflexivity as a freeing and not an obstructive activity, and dialectical thinking. God tricks are eschewed, and the locations and standpoints we operate in and from are recognized. But this does not mean we cannot and do not adopt god tricks now and then heuristically or strategically to assist us in keeping our inquiries going.

Finally, critical sociology of science is a relativistic program - but in a Protagorean, not a philosophical, sense. Philosophical relativism, as Feyerabend (1978: 82ff.) noted, affirms that "all traditions, theories, ideas are equally true or equally false, or in an even more radical formulation, that any distribution of truth values over traditions is acceptable." Protagorean relativism, again following Feyerabend, "pays attention to the pluralism of traditions and values...it does *not* assume that one's own village and the strange customs it contains are the navel of the world." This political relativism (which I find compatible with Biagioli's [1996] "contingentism") affirms that "all traditions have equal rights (Feyerabend, 1978: 82-83):"

> It is not asserted, for example, that Aristotle is as good as Einstein; it is asserted and argued that "Aristotle is true" is a judgement that presupposes a certain tradtion. It is a *relational* judgement that *may* change when the underlying tradition is changed. There *may* exist a tradition for which Aristotle is as true as Einstein, but there are other traditions for which Einstein is too uninteresting for examination. Value judgements are not "objective" and cannot be used to push aside the "subjective" opinions that emerge from different traditions. I also argue that the appearance of objectivity that is attached to some value judgements comes from the fact that a particular tradition is *used* but not recognized: absence of the impression of subjectivity is not proof of "objectivity" but of an oversight.

The research agenda of critical sociology of science encompasses both traditional and new themes in the sociology of science and knowledge. It is distinguished by a concern for ethical and value issues and problems common to what C. Wright Mills called "the sociological imagination." This is perhaps as good a point as any to remind my readers and listeners that I follow Nietzsche in affirming that the question of values has priority over the question of knowledge certainty. The certainty question only becomes serious once we answered the values question.

Critical sociology of science moves easily and regularly between a broadly interdisciplinary conception of sociology and one that is narrowly focused on the pervasiveness of the social and the causal primacy of social structures. The narrow focus is, occasionally at least, necessary to avoid confusing "social" references or rhetoric with sociological analysis (as occurred in the case of Kuhn's analysis of scientific change), and CSS from those sociologies of science that are conceived as sources of support for the ideologies and myths of science, in particular the physical sciences. Thus, a critical sociology of science treats the social organization of science, scientific change, and patterns of communication and power in science as problems, not givens. It is not based on "awe" of science and scientist (the Sartonian turn) or a worshipful, ritualistic orientation to objectivity, rationality, rigor, and the other "good" aspects of scientific inquiry (the Campbellian turn). It does not assume that the development of science as a human activity with negative and positive consequences for people and their environments (as part of a cultural apparatus) is necessarily progressive. It does not accept "intersubjectivity" as a panacea for the fallibilities and pathologies of individual perception, cognition, motivation, and choice. Intersubjectivity is a social process and thus as vulnerable to fallibilities and pathologies as individuals. Social organization is, in science as elsewhere, a dynamic process with the potential for temporary or permanent pathological transformations (including, for example, goal displacement and manifestations of the "iron law of oligarchy" in professionalization and bureaucratization).

One of the common signs of an uncritical sociology of science is the assumption of "efficacy," "success," and "progress" in science. A critical sociology of science must evaluate efficacy, success, and progress in terms of how science is perceived within the various social classes, institutions, communities, and organizations, and by the individuals who make up "science in/and society." To what extent is it created and controlled by classes etc. and individuals? And how does it affect classes etc. and individuals? The fact is that while modern science is part of the general social process of epistemic activity in modern societies, it is grounded in aggressive, domineering, exploitative relationships between people and their social, physical, and material surroundings. Focusing on the "successes" of science without considering the negative personal, social, and environmental consequences of those "successes" is analagous to focusing on the Gross Domestic Product as a sign of economic prosperity without considering the Gross Domestic Disproduct (for example, waste, pollution, and alienation) that would, if we had such a measure, measure the negative consequences of the production, distribution, and consumption of goods and services. And - to take up one of Don's major interests - any focus on *scientific validity* that is restricted to a concern with prolems of measurement and ignores general problems of truth and objectivity is consistent with the prior analogy.

5. *The question of validity*

Optimizing validity can become a narrow organizational and administrative goal, and not the best way to ensure that we are getting high-quality results in our research. Validity (internal and external) is a concept rooted in a quantitative, measurement-oriented conception of science. In this sense, it can be an especially appealing criterion for legitimating research findings from the point of view of governmental or other administrative and social control agents and agencies. A focus on validity in this narrow sense separates the research community (or research communities) from its audiences, clients, subjects, and funding sources and reinforces the notion of a social system of science that is immune to "external" social forces and values. But the concern for establishing, sustaining, and reinforcing a research community producing knowledge that is "useful" is better served by focusing on the social *relations* of science, and on the problem of generating a sense of the social nature and value of valid knowledge among researchers and users alike. As in the case of objectivity, so in the case of validity, we are talking about social processes and different levels and degrees of social organization.

As informal modes of consensus formation become stablized and institutionalized, they become transformed into "truth tests" that appear to be independent of social and cultural forces. These tests can then be used as official guarantors of the validity of research results. More specifically, professionalization and bureacratization (for example) cause creative and innovative ideas and actions to be devalued. Such ideas and actions are a threat to the internal stability of a profession. They also threaten the established social order that underwrites the profession, and the position of the profession in that social order. This is why Don's "social system of science" approach is so problematic. It is a theory of science that is not merely grounded in the findings of science as a profession, but that is also (and not incidentally) congruent with the conservative requirements of scientific ideology. A critical sociology of science cannot for any reason ignore the fact that epistemic communities develop "self-validating knowledge-use systems," and that the claims, predictions, recommendations, and theories of a prestigious epistemic community can be taken as warnings, become self-fulfilling prophesies, and facilitate social control. Ignoring these social realities can only undermine the sorts of efforts Don supported to apply social science knowledge in ways that are both effective and humane.

It is worthwhile rehearsing some of the results of research in science studies over the last quarter century. This research has brought into question, at least, the uniqueness of the rationalities used in science; it has at least suggested that reliability, validity, truth, and objectivity are achieved in science (as a specific social institution) in the same ways that they are achieved in general epistemic activity in any organization, or culture; and it has shown that rigor is not a *sine qua non* in science: it is part of the cycle of inquiry, and can coexist in the same field of research - and even in the same project or problem domain - with nonrigorous methods and concepts. Standards of rigor and validity are historically and culturally situated. And the loosening of canons of rigor is often a condition for solving intractable problems, developing new approaches to get around obstacles, and generally for "getting things done." *Standards* of rigor, validity, rationality and so forth are generally established by or associated with orthodoxy and authority. And we should not forget the stake scientists have as professionals - as workers - in demarcationist strategies. Admitting that scientists have ideological and professional interests and goals, but ignoring these factors in the interest of some sort of idealistic model of inquiry only veils the complex social realities that link discovery and validation with issues of status, power, and prestige, make cognitive "correctness" context dependent, and link theories, methods, and social organization.

Just as *science* can be a label for the general problem-solving activities of humans and the variety of cultural, organizational, and institutional manifestations of epistemic work, so *enhancing or optimizing validity* (Don's major project in his sociology of science) can be viewed as a label for routines in the realm of argument, demonstration, and proof that are standard features of a culture that is self-sustaining and on some sort of "growth" or "developmental" curve. I do not object to the idea that we can develop the epistemic potential of human beings through the study and application of principles of sociology and social psychology. And while I find Don's focus on optimizing validity too narrow, I do not object to the more general idea that it is possible to enhance or optimize the human capacity for generating objective knowledge. This is the aim of a sociology of objectivity carried out in the arena of an emancipatory epistemology (Restivo, 1994).

Don's program in evolutionary epistemology and the sociology of validity is a conserving program: trust the validity of the great bulk of our beliefs while revising a subset of them; emphasize the *social* message of the rigidity of biological duplication processes; coopt the radical critics by recognizing their actual or potential value for established interests - but not their potential as an *alternative* to those interests. Don's 1/99 variation/retention formula is a biological prescription for a Kuhnian system of scientific change. But the alternative is a fully assimilative science, a science in permanent revolution, an emancipatory science (as conceived in the writings, for example, of David Bohm). Don developed a model of social organization based on the "naturalness" of biological rigidities. But he ignored - or didn't take seriously enough - the possibility that biological rigidities may be the basis of a certain "looseness" at the sociocultural level that has a positive evolutionary or developmental function. "Natural selection" cannot have any feeling or concern for the human condition; neither can "God," which is sometimes used as a synonym for natural selection in these kinds of arguments.

No humane - practical - epistemology can therefore take natural selection seriously as a starting point or principle. An *emancipatory* epistemologist is a valuing, creating, criticizing, epistemic agent. He or she is responsive to encounters with more or less recalcitrant realities such as breaking pencils, and planets without margarine in their cores. But he or she is never under any obligation to be bound by or loyal to the "lessons" of these encounters in any rigid way, and certainly under no obligation to be bound by or loyal to the *authority* of natural selection. Here it may be helpful to consider the words of Umberto Eco's (1983: 491) fourteenth-century Sherlock Holmes, William of Baskerville:

Perhaps the mission of those who love mankind is to make people laugh at the truth, to *make truth laugh*, because the only truth lies in learning to free ourselves from insanepassions for the truth.

This message has been broadcast in many forms. Kafka's (1964: 286) assertion in *The Trial*, "Logic is doubtless unshakeable, but it cannot withstand a man who wants to go on living," would find ready endorsement in Dostoevsky, Nietzsche, and others. Those thinkers and critics held such views not because they were "relativists" but rather because they had an appreciation for the dialectical complexities of social structures, and the pervasiveness of the social. Along with some modern students of science, they were critics of the "Cult of Science," and that Cult's intense "faith in science." In at least one of its forms, relativism is synonymous with "good" inquiry or science. Barnes and Bloor (1982: 47n44) write:

> A plausible hypothesis is that relativism is disliked because so many academics see it as a dampener on their moralizing. A dualist idiom, with its demarcations, contrasts, rankings, and evaluations, is easily adapted to the tasks of political propaganda or self-congratulatory polemic. *This* is the enterprise that relativists threaten, not science.... If relativism has any appeal at all, it will be to those who wish to engage in that eccentric activity called disinterested research.

In order to appreciate Kafka, Dostoevsky, Nietzsche, and Eco - let alone Barnes and Bloor - we must appreciate that when we talk about science, truth, logic and related ideas we are *always* talking about *social relations*. This sensitizes us at once to the progressive *and* regressive aspects of words, concepts, and terms that as social relations can embody inequalities, destroy environments, inhibit individual growth and development, and undermine inquiry.

As epistemic agents, we may and do choose to pursue inquiry under certain limitations because we find the social, personal, or ecological costs of "knowledge for its own sake" unacceptable. When Campbell writes about "fit-increasing processes," the emancipatory epistemologist wants to know "fit for what?" If the answer is "fit to the natural world," it must be remembered that we are always changing the world (intentionally or not). By changing the world, by constructing and reconstructing the world we study, we also affect the object world and therefore the "laws of nature" to which we have access. Finally, it is important to realize that social life is not only the source of concepts of nature and scientific theories, but itself part of the "reality as a whole" we are in fact studying. One of the problems with Don's program is that it promotes interdisciplinary approaches but insists, at the end of the day, on separating social and physical worlds, and social and physical sciences.

Don gave us a "strong programme" of sorts in the sociology of science. The problem here is that sociology is considered an "immature" science, but is called on to give an account of science that encompasses the "mature" natural and physical sciences. Simultaneously, sociology is called on to help promote the maturation of the social sciences, including. pressumably, the sociology or science. Aside from any contradictions implied in this approach, sociology is placed under the banner of modern science (in its prevailing organizational and ideological form) and makes sociology of science a demarcationist junior partner to epistemology. The contrast here is with a sociology of science that draws attention to problems of authority and competence in science as a *central* feature of its agenda.

6. *Conclusion*

Science is a social institution, and scientific activities, representations, and products are intimately implicated in that institution. Therefore, any evaluation or critique of science is an evaluation or critique of social relations, social power and social control, the tensions between conserving and transforming social forces, and values. Any call for an alternative science is a call for an alternative way of organizing for and thinking about the production of knowledge, and alternative ways of, and reasons for, pursuing, producing, distributing, and utilizing knowledge.

Don stressed visual demonstration as a basis for science: "In the paradigm instance, the 'facts' are visually supported beliefs shared by the community and visual *demonstrations* introduced in a persuasive process." But while he gave vision a social dimension, he did not always *stress* - as Patrick Heelan does, for example - that "vision is always highly contextual, and in each context the foreground-background relation is different." Nor did he discuss the social construction processes that create validity out of visual demonstration, as Steve Shapin has. This social construction-social context perspective must be the starting point for any critical sociology of science. Emancipatory epistemology adds a concern for revealing and opposing (1) the fetishims of cognition, representations, and knowledge, especially in the theory and practice of science; (2) the fetishism of such ideas as objectivity, reality, rationality, truth, validity, and science; and (3) the alienation of knowledge specialists (including science workers), and people in general from the processes and products of inquiry. In emancipatory epistemology - or better, emancipatory theories of inquiry - the program for a liberated society and personal liberty is simultaneous with - and in any case never subordinated to - the program for open inquiry.

There is no reason to suppose that we cannot apply sociological theory to the problem of enhancing validity or producing objective knowledge. But once we recognize that validity and objectivity are community products - social constructs - we are obliged to recognize that valid or objective knowledge has qualitative features. Thus, we need to be concerned with the ways in which the intersection of various standards of validity might affect our evaluation of knowledge claims. Cultural content of various kinds and levels pervades valid or objective knowledge. The problem for sociologists of science, then, is to clarify this thoroughly social conception of validity and objectivity and to contribute to developing new standards for evaluating knowledge, including of course their own.

7. *ERISS and civilization revisited*

Don introduced the collection of his papers published by Chicago in 1988 with a sketch of his scholarly career. There were three sections of this sketch I found especially interesting since I think they may be the root of why our sociologies of science diverge. The most significant is the section he titled "Respect for Tradition and Evolutionary Theory." Don affirmed a commitment to creating a thoroughly scientific social psychology but an unwillingness "to jettison traditional wisdom about how to live life and rear children." My experience, by contrast, has led to a commitment first to the naive goal of a scientific sociology and later to a more sophisticated goal of theoretical inquiry combined with a sometimes volatile rejection of traditional ways of life. I found nothing at all to salvage from religious teachings, and while I had very caring and loving parents, I found nothing to imitate in a form of life characterized by deprivation, job dissatisfaction, alienation, and so on. Once I learned to see our family and community situation as a product of traditional ways of doing politics, economics, and religion, my form of anarchism (nourished by Marx and Nietzsche as well as Kropotkin and Bakunin) followed.

At the ERISS (epistemologically relevant internalist sociology of science) conference in June, 1981, Don and co-organizer Alex Rosenberg assembled (and I quote Don here) "ideal groups of naturalistic philosophers of science and relativistic sociologists of science..." In the wake of the conference, Don wrote that "the conference failed utterly to address the agenda I had intended, mainly because the sociologists focused on a well-articulated skepticism, being unready for a speculative comparison of social systems of belief change and belief retention." I was one of the "ideal" participants in that conference (along with David Bloor, Karin Knorr-Cetina, and Steve Woolgar), and I found it unfortunate that Don placed the blame for a key failure so baldly on the sociologists. In fact, the very theme of the conference could not have been satisfactorily imagined without the revolutionary contributions of the assembled sociologists to our understanding of science as a social and cultural phenomenon (the label of "relativism" in this context is a red herring). It was, from where I stood, the philosophers who resisted or were ignorant of the pervasiveness of the social and advocated naive realisms about things in the world and terms that refer and an insulting attitude in the face of sociologists who were transforming our understanding of science in fundamental ways.

Finally, Don wrote that he "read many of Stendhal's works but found Dostoevsky too threatening to complete..." I, by contrast, was never able to finish anything by Stendhal but found Dostoevsky an enlightening companion. I read "Notes From the Underground" as an undergraduate, and realized many years later that this was a treatise on the sociology of mathematics. I think there is a key here to some of our differences, but I have not thought through the relevance of these literary tastes and distates.

7. *Farewell to the winnower*

The agenda the sociologists tried to articulate at the 1981 Lake Cazenovia conference on epistemologically relevant internalist sociology of science fell on deaf and resistant ears. Then, philosophers tried to pound naive realism into us by banging coffee cups on table tops and rehearsing infantile realism at the blackboards. At a 4S meeting not too many years ago, I saw the relativism-realism debate - or debacle - carried on by breaking pencils and threatening a philosopher with a water pitcher. That we are still haunted and hounded by charges of relativism, that the term reductionism is unsheathed at the mention of the word "social," that social constructionism is imported into philosophy and treated as just another philosophical idea amenable to being chopped to pieces using the traditional tools of philosophical surgery either signal we sociologists are totally mad, or that we are the targets of a resistance to discovery Bernard Barber alerted us to so many years ago, and that Mary Douglas could help us understand better than any one. Barnes and Bloor defined relativism as disinterested inquiry many years ago, a definition that has gone virtually unnoticed and unheeded as our critics continue to illustrate that a sacred realm is under siege. Even a wild character like me recognizes that a little selective winnowing would go a long way to cleaning up this field of controversy and confusion.

Don will be missed for his lessons in selective winnowing, and for all of his major contributions to methodology and epistemology in the social sciences. I will miss him - and I should think all of you will miss him too - for the childlike curiousity, even naivete, with which he approached the world, his intense interest in playing with ideas across the disciplines, and his tolerance for wild characters.

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