EVOLUTIONARY EPISTEMOLOGY AND SOCIOLOGY OF SCIENCE

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1. INTRODUCTION

It is uncommon for philosophers to appeal to sociology in support of an epistemological theory, and it is even more uncommon for them to recognize and accept sociological findings when they are unsolicited. Recent sociology and anthropology of knowledge is a case in point: though some of us have on occasion found ourselves embroiled in theoretical controversies with philosophers, we rarely succeeded in getting them to notice our empirical results (1). EE refers to a relatively recent attempt to extend the theory of biological evolution to scientific change. It is the first coherent epistemological program that is empirically grounded (2), and under the leadership of Don Campbell it has shown none of the predeterminations with which philosophers of science tend to fend off sociological inroads upon their territory. In fact, Campbell's challenge to sociology of science to come up with interesting answers to the many problems of scientific change is serious and sincere. But can we meet it? And does not our methodological orientation prevent us from cooperating with the only epistemology that till now has seriously espoused sociology of science? And that, for better or for worse, has felt "unanimously rejected" by the new sociology of science?

I think we can do better than that, even though the benefit for either side may be less than clear. For example, my own methodological orientation has been more phenomenological ("constructivist") and microscopic than evolutionists are generally keen on, and little imagination is necessary for someone to envisage the quarrels which might arise between a constructivist and an evolutionary orientation. On the other hand, as Geertz says (1983: 19f.) there has been an enormous amount of genre mixing in intellectual life in recent years, and one might go as far

as to suggest that this jumbling of varieties of discourse has added to the success of such writers as Kuhn, Feyerabend and Foucault. So, presumably Campbell is on the right track, and in this paper I shall not hesitate to join the jumbler and to compromise on my own preferred discourse orientation.

That the formula of mutation and natural selection translates easily into a number of theoretical vocabularies is hardly deniable. It can apparently accommodate successfully Popperian falsificationism and Merton's normative functionalism (Hull, 1978), interest models (it can be applied to Pickering, 1980), and, if I understand Campbell (1986) correctly, hermeneutic interpretation. Of course, this expansionist success of evolutionary theory can to some degree be explained by the fact that the analogy is drawn on a very general level, and it may profit from the fact that Darwinian concepts have long filtered into lay discourse and social science disciplines. It is disconcerting nonetheless, for it seems that the more phenomena one covers by a trim conceptually parsimonious explanation of the social world, the less one is actually saying about it. And the more easily a conceptual framework lends itself to translation into other vocabularies, the more frequently one might ask what we gain from doing these translations.

In a sense what I want to do in this paper is just that: arguing from my interest in scientific work I shall ask for which parts of the evolutionary imagery the above translations appear fruitful in that they provide access to or appreciation of hitherto little examined aspects of scientific practice and for which they do not. In essence I shall argue that the use of the theory of biological evolution as a model of scientific discovery does not commend itself to the analysis of scientific work. The evolutionary model collapses some rather complex processes which we are only now beginning to understand into singular events (conceptual mutations), which, themselves unexplained and unexplainable within evolutionary theory (they are chance occurrences), are burdened with doing all the telling there is about scientific innovations. On the other hand, the theory of natural selection, I shall argue, sits rather well with some sociologists' of scientific practice beliefs, and might even suggest some interesting challenges to more established science studies concepts. If the notion of a

conceptual mutation, of a "meme" or of other essential components of mutation theory comes as close to being an essentially contested concept as any in the social sciences, the idea of natural selection, properly reformulated, might become essentially uncontested in social studies of science (a few remaining rationalists notwithstanding). Thus I believe that the full weight of the evolutionary analogy can probably not be coherently extended to scientific change on a concrete level, but the natural selection part can, and perhaps even catches its tone and underscores recent sociology of scientific knowledge and sociology of scientific practice results.

2. THE USE OF THE EVOLUTIONARY ANALOGY AS A DISCOVERY MODEL

Most advocates of EE do not propose to extend biological explanation to scientific behavior. Instead they propose to use the theory of biological evolution, or some parts thereof, as a *model* for scientific evolution. The first problem for someone reasoning analogically from an established theory is to identify the analogues in the system to which the model is to be extended. In EE, virtually everyone who writes equates mutations, the ultimate source of variation in biological evolution, with conceptual innovations.

As an example, consider first Toulmin's theory of scientific evolution, best summarized in an article he wrote in the *American Scientist* in 1967. As most evolutionary theorists, Toulmin distinguishes between the generation and the selective survival of ideas, a distinction which mimicks the traditional philosophy of science distinction between the context of discovery and that of consensus formation. Accordingly, at any given time in scientific thought we have a pool of scientific innovations and an ongoing process of rational selection among these innovations. Conceptual variants are the products of individual scientific innovators; their merits are collectively judged and their fate decided by communities of specialists. At any given time, intrinsic (or intellectual) and extrinsic (or social) factors jointly affect conceptual variation. For example, social factors may limit the occasion for scientists to speculate freely, and intellectual factors may help them to focus their theorizing on promising lines of thought. Selection, on the other hand, in Toulmin's model is

ultimately rational. In the common situation where disciplinary problem solving is based upon agreed disciplinary goals and standards, the selective perpetuation of conceptual variants is straightforwardly rational (intellectual). When scientific work pertains to the "rational frontier" (Toulmin 1972, p.241) where standards of selection are themselves up for reappraisal, scientists may have to take "rational bets" (1972, p.246). These may not be arrived at by accumulations of bare facts of Nature, but they are nonetheless "objective" in the sense that they are backed by scientists' collective experience and that they will be judged, in the long run, in the light of their actual practical sequels for our understanding of Nature and not by personal considerations (1972, p.244).

With Toulmin the published but not yet accepted products of scientific work constitute the pool of variations. Campbell's well-known (1974) theory of scientific evolution differs from Toulmin's in a number of respects. Most noteworthy, perhaps, is his much broader perspective on variation and selection which prompts him to include among conceptual innovations the novel not yet tried out ideas of laboratory work and among extrinsic factors the many flaws in human visual perception. He has also chosen to be less specific than Toulmin about selection standards, and wisely so as I shall argue later, despite the critics who attacked him on this lack of specificity (e.g. Corning 1983, p.247). Partly because of his broadening of the discovery model to include - and commend - "wild speculation" and partly because of his preoccupation with the biases of visual perception (and partly because he wants to offend), Campbell insists on calling variation "blind" and "unjustified" (1974, p.147ff.). Where Feyerabend uses the slogan "everything goes" to advocate counter-induction and the proliferation of theories (1970, p.26), Campbell employs the somewhat polemical aphorism of blind variation to refer to the discovery of new scientific theories through 'underjustified' speculation (1974, p.152).

No aspect of theories of sociocultural evolution modelled upon biological evolution has caused more resistance than their specification of innovation as conceptual mutations (e.g. Corning 1983, p.205), and no aspect of Campbell's theory has provoked more criticism than his calling variations "blind" (e.g. Hull, 1982). The arguments against the case for mutation-like innovations in socio-cultural systems, scientific or otherwise, have been

discussed in terms of 'coupled' versus 'uncoupled' evolution, Lamarckism versus Darwinism and human intentionality versus the chance occurrence of variations. These arguments are well-known and need not be repeated here in detail. In essence, most discussions of the problem boil down to the claim that sociocultural innovations, including those of science, are *intentionally designed* to survive the selection process which decides their fate, whereas biological mutations arise in a spontaneous, random manner *unrelated* to the environmental factors which account for their selective perpetuation. Toulmin acknowledges the disanalogy but seems to think it is unimportant (1972, p.337), to the consternation of his critics (Hull, 1982, p.17). Campbell, more belligerent than Toulmin on this point, attempts what I consider a rather successful rebuttal (1977, p.503). No matter how reasonable and intelligent scientific inquiry is, and no matter how persistently scientists strive to come up with conceptual variants that will survive the selection process, innovation cannot be explained by the restraints or guidance provided by existing conceptual systems. Discovery by definition cannot strictly be a matter of producing desired conceptual innovations on demand (Hull 1982, p.37f.). If it were, scientists would not really go *beyond* what they already know, but merely enact a culturally acquired knowledge program. Besides, as Campbell and others have argued (1974, p.147ff., Corning 1983) the mere existence of restraints caused by goal-directedness and previous knowledge on conceptual mutations is not necessarily a disanalogy to biological evolution in which there exist a number of restrictions which limit the range of variations available to a population.

Now I agree with Campbell that the purposiveness of scientists' behavior may not be the key to the disanalogy between biological mutations and scientific innovations; but I also believe that problems remain nonetheless. Intentionality is a notoriously intractable internalistic concept. I shall follow a different path, and begin by looking at the initial recognition of an innovative idea. I want to borrow a term used by Campbell (1974, p.155) to suggest that discovery in science may be better understood as an *editorial process* rather than as a process of conceptual innovation, and hence must be seen as a phenomenon rather unlike biological mutation.

3. DISCOVERY AS AN EDITORIAL PROCESS

As everyone knows, concepts are constructs: they construe the world in a specific (selective) way. But they do so not only with respect to their propositional content. They also come with certain illocution-like pretensions; for example, when they arise in scientific work, with the pretension of being a "good idea". When we refer to a conceptual event as a "good idea" or simply as an "idea", we usually mean it is a fortunate thought potentially valuable in solving the problems at hand or in opening up new problem solutions. We do not mean that the thought was irrelevant, or that it was a nuisance and suggested troubles. Hence with respect to innovations, the point is not only that they are accidents in the sense that they are unforeseeable and cannot be produced at will (and hence are like biological mutations), but also that they are, *unlike* biological mutations, *fortunate* accidents, by which I mean occurrences which scientists perceive and interpret (*preselect*) as felicitous opportunities for success. Thus for an occurrence to enter the selection processes of experimental trial and disciplinary "consensus formation", it must have been *preselected*, first by being recognized as an 'idea' or an opportunity for success, and second by being judged feasible under the circumstances and worth to be tried out. *Primary imputation* (3), as one might call the recognition of something in terms of certain implications, refers to this process of preinterpretation.

The point here is that a linguistically coded occurrence acts as an instruction to react in terms of the coded formulation, and thus has real consequences in practical action. If the thought that crosses my mind comes through as a worry, I shall do what I can to prevent it from becoming true. If it comes through as a promising new idea, I shall want to pursue it, and create the circumstances which will help it to work out. Thus there is no such thing as a pool of unclassified conceptual mutations waiting for later selection processes to take hold. A "bad" or "irrelevant" idea may go unnoticed or stimulate repairs, but it would seem to have no chance to become part of the pool of variants available for (experimental) selection. I have found successful scientists to pride themselves to be good at just that: the recognition of the potential for success of a novel problem, concept or technique. What was wrong with a lot of unsuccessful scientists, one of them told me,

was that "they were not dumb, they just worked on the wrong things" (Knorr Cetina, 1981, p.74). The clues one gets from such scientists refer to their ability to discriminate, which initially means to recognize an occurrence as an opportunity for success. In this connection it is good to remember that scientific 'innovations' have many origins, among which novel thoughts by individual scientists are but one. Another well known source of scientific innovations are unexpected experimental outcomes (anomalies), which may be as prosaic as the smell, the feel, or the look of a substance. Still other sources lie within the local environment, as when someone unrelated to a project makes the right comment at the right time or when an apparatus suggests a particular problem solution. In all these cases, it is clear that what is at stake is a scientists' *ability to recognize* the potential of an occurrence. In other words, the achievement here is one of *discrimination and selection*, and not of unjustified or otherwise qualified variation.

To repeat, the linguistic designation of an occurrence as "innovative" or as an "idea" invariably involves a process of imputation which, within the imagery of evolutionary theory, would seem to be more appropriately rendered by a selection model than by mutation theory. Since there is no such thing as a pure, objective, once-and-for-all scientific innovation irrespective of its designation as an innovation, there is also no such thing as a proper equivalent in scientific work of biological mutations. I have referred to the process of imputation as editorial, and I now want to add two observations which extend its scope. First, interpretativity (selectivity) does not stop with initial designation. Scientific works are construed as novelties through editing practices throughout their laboratory life and in print, not to mention the reconstructions by posterity, such as by historians of science. They are also of course deconstructed as novelties, often by their own originators whose work moves on, and by the uses of these novelties. Users exemplify sophisticated literary skills in highlighting their own originality while de-emphasizing the contribution of those upon whose work they draw. Of course on occasion it may also serve the purpose of a user to 'rediscover' and applaud an earlier original inventor in order to outfight a contemporary who aspires to the honor of being first in a priority struggle - as Brannigan (1979) has

shown with respect to the "rediscovery" of Mendel's laws. These strategies vary, but they would all seem to imply that novelty is the outcome of locally and historically variable - and reversible - imputations of novelty negotiated between participants from within and without science. In essence this is what attribution theory predicts and what has been established in at least one extensive historical case study (Brannigan, 1982). In accord with calling the recognition of an occurrence as a potential innovation primary imputation, we might refer to these later designations as *secondary imputations*.

Yet the discovery process, and this is my second observation, is only partially captured by attribution arguments. Construal as novel or interesting penetrates into all aspects of scientific inquiry: a scientist may choose a technical instrument because it is novel and rarely available, seek out literature not generally known in his or her area, give preference to procedures associated with the frontiers of knowledge, or buy apparatus perceived to give him or her an edge over the competition. Thus construal as novel is not just a matter of simultaneous (primary) and negotiated (secondary) imputation. It is also a matter of *articulating* the product in and through scientific work. This aspect of the editorial process is more akin to the *writing* of a work itself than to a publishers' cosmetic touch-up operations. If the *initial* editing of a thought as an innovative idea is primarily a matter of "*seeing-as*" (primary recognition), and if *secondary* editing involves mainly processes of *negotiated* imputation (attribution), the third aspect of the editorial process I have mentioned might best be described as a matter of *work-articulation*. Recent empirical studies related to the subject imply that discovery is an editorial process in all three senses. I think that an understanding of the editing strategies involved *amounts to* a theory of scientific innovation.

Now to call discovery in science an editorial process invokes the text analogy. It shifts the idiom from intentionalist approaches taking their lead from the psychology of mind to a phenomenalist perspective content with reading the surface of things. Innovations are not seen as unaccountable hot spots, little explosions of spontaneity and creativity, in otherwise drab everyday routines. Rather they are seen what participants "*read*" and "*write*" (articulate) them to be in the discourse and text building of scientific work. Almost automatically, then, the

vehicles of scientific 'innovation' become *situational* and *social processes*, observable from the outside and *prima facie* not so different, except in topic of course, from other processes in everyday life.

Mind-creativity is a phenomenon of seemingly intractable depth. But if we avoid it by describing scientific discovery as an editorial process, do we not lose the phenomenon of "*real*" conceptual innovation? The problem with this question, I think, is that it identifies scientific innovation with certain combinatorial skills participants may display in taking advantage of their environment, experience or education. These skills, I submit, may account for a certain percentage of conceptual variations, but they do not account for "discovery", or scientific "innovation". Why not? Let me expound the thesis by a brief excursion into the *metaphor or analogy theory* of scientific innovation.

As has been argued (Black, 1962), much of what we customarily call scientific innovation involves reasoning from analogy, that is it results from bringing to bear pre-existing and pre-tested concepts from a context in which they have worked in the past upon a less understood problem area. Now the crux of the matter in reasoning from analogy is not that an idea is new but that it is *old*; it is a *pre-tested solution* in a context similar to the one under current investigation. In evolutionary terminology, the *content* of the variant is not novel; what is novel is the *connection* made between hitherto unrelated problem contexts and the *insertion* of a familiar idea into a context to which it had not previously been applied. According to a number of authors, this "displacement of concepts", as it is appropriately called, may be the single most important source of our conceptual extension of knowledge (Hesse, 1970; Schon, 1963; Mulkay, 1974). If this is true, and in my own limited laboratory experience I find reasoning from analogy quite common, then we have here a phenomenon that is eminently based upon cultural learning and transmission and very unlike biological innovations. After all, biological mutations, whatever else they are, are not transfers of pre-existing and pre-tested genes from one context to another (though with the rise of biotechnology, they may become so).

But be that as it may, the point I wish to make is that displacements of concepts based on metaphor and analogy are routine features of scientific (and everyday) reasoning.

It can be shown (Knorr Cetina, 1981, ch.3.6) that they occur as frequently in the case of "blind alleys", "degenerative problem-shifts", or simply failures to make something work as they do in "innovative" successes. Thus, a theory which accounts for scientific innovation solely in terms of metaphor and analogy is at the same time a theory of *scientific failure and mistake*. It does not *discriminate* between the different fates analogy-based ideas encounter in the process of research and in scientific publication. The metaphor and analogy theory of conceptual displacement is at best a theory of the creation of variants. It fails to take into account the editorial processes through which variants become stylized as scientific innovations.

To conclude, I think the example of the metaphor and analogy theory suggests that it might pay to distinguish between cognitive and other mechanisms which account for the creation of variants and processes which explain the generation of "discoveries" and "innovations". If we equate the latter with the former, we not only disregard the larger part of the picture discovery represents in scientific practice, but we also misconstrue a phenomenon as exclusively mental in which mental factors may play only a small and in any case a non-decisive role. Innovations do not lie at the beginning, but rather at the end of processes which include retroactive imputations on the basis of the consequences of previous events. In other words, they are the result of participants' "ethno-epistemology" work, whereas variations are not.

I shall return to this point at the end of this paper. But first I want to stress that it is quite another matter to explain variation than it is to explain mind-creativity and scientific 'innovation'. Consider that scientists do not have to wait for scientific 'mutations'. The scientific literature, and the rest of culture, provides a pool of stored up variations ready to be called forth at any time by those who can make the appropriate connection. Moreover, many variations may come about through utterly uncreative activities, for example through adapting a technique or other finding to local conditions, to the special demands of a new project, or to personal knowledge and expertise. Thus, variations may have more to do with the local requirements of knowledge production and with the variability of discourse communities than with anything akin to individual "creativity" or "innovation".

4. THE USE OF EVOLUTIONARY THEORY AS A MODEL OF SELECTIVE RETENTION

I shall now turn to the use of the theory of biological evolution as a selection model. As Campbell once said, there may be great conceptual gain in moving from teleological causation to natural selection explanations of teleological achievement (1974, p.145). These gains, I think, have not been sufficiently exploited by evolutionary epistemologists. In order to appreciate the potential of evolutionary theory as selection theory, let me first recall what natural selection is not. I shall draw heavily on Corning's summary of Darwinian evolution (1983, p.30ff.).

First natural selection is not a mechanism or a principle or an agent extrinsic to the relationship between organisms and their environment. Rather, "natural selection" is a shorthand way of characterizing a vast array of dynamic processes by a term derived from the "artificial selection" of plant and animal breeders which inspired Darwin's concept. Whereas artificial selection can be seen to be teleological in the sense that it involves external selecting agents who deliberately select, with an eye on the future, the traits for which they are breeding (Nagel, 1979, p.303), natural selection is "a non-teleological process in which the multifarious interactions between organisms and their environment can produce, without external direction, the ... emergence of functional design (or adaptation) in nature". Phrased differently, the term natural selection refers to a variety of factors which in an given context are responsible for causing differential survival and reproduction among (genetically) variant individuals, and for absolute changes in numbers and diversity of different populations over time (Corning 1983, p.30-31). Recall the textbook example of industrial melanism as recently summarized by Corning (1983, p.31). Before the industrial revolution, a light colored strain of the moth (*Biston betularia*) predominated in the English countryside over the darker, "melanic" form. Apparently, when the light moth rested on tree trunks it was almost invisible against the then light trees, while the dark form "stood out" and was preyed upon more frequently by birds. With the industrial revolution, smog from factories blackened the tree trunks in industrial areas, and the relative visibility of the light and the dark form was reversed. Now the darker moth became

the one less subject to predation, and in due course increased in frequency relative to the lighter strain of the species.

Now natural selection is not located in any single factor that led to the reversal; neither in the human purposes responsible for the industrial revolution, nor in the perceptual abilities of the birds that switched from the darker to the lighter variety of moth; neither in the smog that blackened the tree trunks nor in the moths' wing pigmentation. Rather, it refers to a "configuration of factors" that combined, in a specific context, to produce differential survival and reproduction rates. It might be described as a sort of vector sum (Wright, 1934) or, less mathematically speaking, as the upshot of a number of external and internal influences such as the resting behavior of the moth, its polymorphism for melanic wing coloration, factors named only recently such as an augmented fertility in the melanic form and differential susceptibilities to variations in air temperature and sulfur dioxide levels, as well as of course industrial pollution and the feeding habits and visual detection system of the birds. Thus the notion of natural selection must be disaggregated, and if this is done a multitude of interacting local factors can be seen to contribute to it.

Now I am dwelling upon this point in order to remind us that natural selection is not a sculptor but a *historical process* which bears a number of resemblances to historical selection processes in social life. Concretely, the process of natural selection can be characterized as follows:

- (1) It is *local*, in the sense that differential selection among populations or individuals happens within, and relative to particular environments.
- (2) In addition to being local, the process is *contextual* in the sense that the specific constellation of factors involved in a particular selection effect is *historically contingent* and cannot be predicted concretely in advance.
- (3) Furthermore, the process is *open-ended* and *without inherent direction*. It is not propelled forward in one direction by some law-like principle which accounts for evolutionary progression - although retrospectively, of course, certain past trends may be discerned.

To these characteristics on which there seems to exist a broad consensus among evolutionists (see Jacob 1977; Corning 1983, ch.2). I would like to add a fourth, which is that

(4) From the point of view of selection theory, the process needs *variations*, but does not require us to adopt a model which locates the origin of these variations in genetic mutations.

There are other aspects of evolutionary theory, of course, on which there is little consensus among biologists, such as the question whether the process is incremental or includes what Mayr (1975) called "genetic revolutions" (see also Gould & Eldredge, 1977). However, these aspects need not concern us here, for whatever the outcome of the respective controversies among geneticists, it is unlikely to change the basic premises of evolutionary selection theory outlined above. The question for us is rather whether the respective factors can also be seen to characterize selective retention in science and whether the resulting model offers a plausible alternative to currently predominant concepts of scientific consensus formation. I think it does. To argue my point, I will begin with the first premise.

5. SELECTIVE RETENTION AS A LOCAL ACTIVITY

Natural selection is local in the sense that *situation-specific* effects such as the relation between the wing coloration of a moth and the color of tree trunks in the English countryside become the cause of *transgenerational* continuities and changes (Corning 1983, p.32). Phrased differently, causally efficacious effects in biological evolution are inherently bound up with the environment in which they arise. They do not normally hold for all members of a species across different environments, nor - natural disasters notwithstanding -, for all environments irrespective of their inhabitants. Now in cultural evolution environments are not necessarily defined by a shared locale or by physical proximity. Instead, they might be defined in terms of frequency of interaction and accessibility, a social structural phenomenon dependent upon participants' awareness and access strategies. These are not, however, to be confused with outsiders' (say social scientists' or philosophers') similarity classifications regarding social structures. Cultural environments no less than natural environments must be "lived" ("transacted") in order to be effective. To portray them adequately within theory, the analyst has to adopt what Edge once called "a radically participant-centered perspective". I want to

claim that scientific disciplines, considered by Toulmin (1967, 1972) and others as relevant selection environments in the sciences, are based on historically rooted similarity classifications, which are reflected in administrative and educational structures but have little to do with the *working environments* within which technical choices are routinely made. These working environments are not constituted by the content of disciplinary textbooks. Nor are they identical with administrative units divided by disciplines such as the university departments or other organizations which constitute a scientist's institutional affiliation. Furthermore, the concept of a working environment does not refer to an identity question. It has nothing intrinsically to do - though it may at times coincide with - scientists' official disciplinary or professional self-identification. In fact, what constitutes these environments is entirely an empirical question. It cannot be decided by simply adhering to the institutional (e.g. disciplinary) pre-classifications which we encounter in everyday life. For one thing disciplines are clearly too large and encompass too many specialties for their members to be relevantly involved, or even know about, each other's selection decisions (4).

But if the discipline is something of an institutional *artifact* and in any case too large, what about the smaller *specialty communities* which have dominated our thinking about the social and intellectual organization of science, and not only since Kuhn (1962)? The problem with specialties, I think, is that like scientific disciplines they are not properly speaking "*organizations*", goal-oriented co-operative little systems, that is, with explicit systemic goals and superordinate controls (5). They lack the property of being "*wholes*" by virtue of systematic ("*emic*"!)(6)) characteristics such as *organized action in concert* or *political superstructures*. Instead, they are wholistic *constructs*, from the analyst's Petrarcan, "scenic" perspective, from which the fissured landscape of specialty members' local dealings appears like the integrated cosmos of a nature painting. The design of the various parts of the human body is never for long independent of the whole. As someone once said, "When I decide to go to dinner almost everyone of my roughly ten trillion cells is constrained to go with me" (7). But when the member of a scientific specialty publishes a paper, no one else is constrained to take notice, let alone to react.

It is only if some participants decide to react *for their own local reasons* - some of which may have to do with each other - that a "collective" outcome based on the sum or interaction of these actions becomes possible. But like the selective survival of populations in biological evolution, this is an *aggregate effect*, not a purposeful, knowing and organized achievement of contributing participants.

There is, then, *no system* or *superstructure* in a specialty which makes communally organized selections. This lack of a superordinate structure leaves us with the actual working relationships, the arenas of interaction in which scientists are involved, as potential candidates for working environments. Are these the "problem areas", "research networks" or "research areas" in terms of which some sociologists of science attempt to redefine specialty communities (e.g. Mulkey, Gilbert and Woolgar, 1975)? I think not quite. For although these notions have the advantage of bringing the concept of a specialty community down to the size of scientific reference groups *known* to participants, they are nonetheless inadequate in that they latch on to - rather than to render problematic - the culturally presupposed distinction between science and other institutions. In other words, they are built on the assumption that the environments we are looking for must be *internal* to scientific disciplines, or in any case to the scientific community at large.

But is this internalism justified? Recall that evolutionary epistemologists, among others, have been keen to admit that "extrinsic" factors play a role in scientific innovation. For example, Toulmin considers the production of conceptual variation to be affected *jointly* by intellectual ("intrinsic") and social ("extrinsic") factors (1972, ch.3). To these Campbell (1977) has added psycho-physiological and cultural factors such as those associated with the many biases of visual perception. On the other hand, Toulmin (but not Campbell) proposes a rational interpretation of conceptual selection. Considering intellectual disciplines as "organized, rational enterprises" (which I have denied), he presumes that conceptual selections are primarily or "normally" made "in the light of" relevant disciplinary considerations (1972, p.222ff.). Now with this kind of reasoning Toulmin has many philosophers on his side. Until recently, philosophers maintained that questions of discovery must be distinguished from the context of validation, and considered the latter to

be subject to some version of the rational interpretation. In the meantime, however, the distinction has been challenged on a number of grounds from within philosophy and from without (Knorr Cetina, 1981, p.7ff., 1982; Nickles, 1980). One reason for rejecting the distinction refers to *where* selection processes in science are located. For it can be argued, and I have done so elsewhere in detail, that the context of discovery is itself the locus of scientific selection, the place where the products of previous scientific work become singled out and selectively incorporated into new scientific inquiry. It has often been maintained, correctly I think, that the value of scientific results lies in their usefulness for further research, that is in the degree to which other scientists decide to build upon these results in their own research. But where would these research decisions take place if not in the laboratory, that is in the very context of scientific discovery? What is consensus formation if not a process of *selective affirmation* of knowledge claims *in and through* further research? Surely it is not a process of *opinion* formation *disengaged* from actual laboratory decisions. And even if such a process existed, it would count for little, for it is quite clear that disengaged opinions have an oblique, if any, relationship to action. What counts with respect to the final survival of a knowledge claim is plainly *scientific practice*, and this takes place at the various sites of actual research.

Now if, as I am arguing, the process in which conceptual variations are produced is *at the same time* the process in which previous variations are either selectively reproduced or "weeded out", then all factors which admittedly affect the production of variations must also affect their selective retention. Hence we cannot, following Toulmin, determine the "context of discovery" to be impure with respect to the origin of factors which enter participants' discovery considerations and simultaneously keep the selection context 'clean'. In terms of our previous notion, there is only one *working context* of inquiry which encompasses both discovery *and* selection, and which in all likelihood is neither completely 'inside' the scientific community nor external to it. This does not rule out, of course, the possibility that one dimension prevails at a given time to the near exclusion of the other, at least within the general constraints imposed by the historicity of human affairs and by anthropological constants. But it does

rule out wholesale relegation of selection processes to internal specialty environments and "disciplinary" considerations.

Elsewhere I have called these working environments of scientists "transscientific" and "transepistemic": They potentially transcend the scientific community in that they include relations of dependence and interactions between scientists and members of a number of '*neighboring groups*' which are somehow engaged or engageable in the pursuit of relevant inquiries (I have in mind users, administrators, journal editors, funding agents, toolmakers, the industry serving science, and others). I use the notion *epistemic* to refer to considerations presumed to promote the truth-like character of scientific results. Transepistemic arguments traverse and transcend these considerations, and they play, as one can show (Knorr Cetina, 1982, 1984, ch. 5.11) a prominent part in scientist's technical decisions. *Working environments* are potentially larger than problem areas and research networks precisely in the sense that they transcend disciplinary boundaries and "intellectual" considerations. But they are also potentially smaller in that they include only "working" relationships of interaction and influence actively entertained by relevant participants. In other words, the fact that selection decisions may be made by scientists is not relevant - what is relevant is that these decisions are embedded in, and hence influenced by, working environments which encompass participants in different roles and occupations and different discourse provinces of the social world.

6. SELECTIVE RETENTION AS AN OPPORTUNISTIC ACTIVITY

Evolutionary theory predicts that internal and external selection pressures will arise in natural environments, and that those individuals or groups which are equipped to meet the challenge or those that organize effectively in response will be selectively favored over those that do not. It does not predict concretely what selection pressures will arise or what traits and measures will have selective value, for the simple reason that these will all depend on other factors within the historical interaction systems (Wright, 1980) of specific selection environments. What predictions evolutionists might wish to make with respect to specific processes of selection must be based on concrete analyses of local configurations and cannot be

derived from the general theory of evolution. By specifying natural selection as a process in which situation-specific effects cause transgenerational continuities and change, the theory of biological evolution also tells us what it cannot - without case analysis - further specify, and where *historical analysis* must supplement nomothetic theory.

By dubbing selective retention in science "local" I have prepared the ground for considering it as *contextual* in exactly the same sense as natural selection is contextual: What selection factors matter in the choice of one scientific product over another cannot be concretely predicted for the simple reason that it depends on historically idiosyncratic configurations and interpretations which are not governed by any single law-like principle or mechanism. But of course the respective outcomes can be analyzed retrospectively (historically), and educated guesses about some future selections may be possible from contemporary analysis of networks of related working contexts (*transscientific fields*). It might also be noted that from the point of view of scientific work, contextually contingent operations are opportunistic in the sense that scientists do not only take account of and passively react to 'contextual' pressures. They also actively structure their environment in terms of work-opportunities (and impediments). One might even say that through interpretation, rearrangement and active participation, scientists create these environments with whatever initial material they have at hand. Opportunism, of course, is a matter of flexible, ad hoc reaction to perceived opportunities as they present themselves in unpredicted ways. As an inquiry *strategy*, it reinforces and sustains the presumed contextual contingency of particular selection processes. And it is of course the most important reason for insisting that working environments should be outlined from a participant-centered perspective.

Now if I am right in asserting a strong analogy between selection in biological evolution and selective retention in science with regard to the local and contextual properties of these processes, this means that *single factor* selection theories in philosophy and sociology of science such as rationalistic interpretations or interest models are either wrong or tautologous ("interests are whatever prompts scientists to choose a particular theory"). This is a strong assumption, but it has much evidence to its support. My proposal also means that the process of selective

retention is as open-ended in science as it appears to be in biological evolution. Again, this is not a trivial consequence of what seemed to be the innocent proposal that selection in science is local and contextual. To be sure, the assumption of open-endedness gains support from authors such as Kuhn, but evolutionary epistemologists have not embraced Kuhn's conclusions. Nor have their critics. In fact, one of the strongest disanalogies perceived between natural selection and scientific selection is that the latter is seen to be (rationally) organized and goal-directed, whereas the former appears to be blind. The argument runs somewhat like the conjections advanced against the use of evolutionary theory as a model of scientific discovery. For example, Rescher (1977, p.133) describes selective retention in science to be rational, and contrasts "rational selection" with natural selection (8). "Rational selection is a process of fundamentally natural selection - both are simply devices for elimination from transmission. But their actual workings differ, since elimination by rational selection is not telically blind and bio-physical, but rather preferential/teleological and overtly rational."

In a similar argument, Elster (1979, pp.9,87) elaborates the disanalogy between natural and "rational" selection by distinguishing between locally and globally maximizing machines. As Hull (1982, p.33f.) points out, "a system is only locally maximizing if it can react only to immediate conditions". Natural selection "is at best locally maximizing" while, as Elster says, 'the capacity for global maximization is a unique and distinctive feature of man'. Hull seems to agree with these authors in thinking that the key to the difference between biological and sociocultural selection is to be found in human intentionality, but he nonetheless believes that the picture which emerges from reading the primary literature of science is "not very directional" (1982, p.38). I think intentionality is an inherently individualistic concept which has nothing to do with the aggregate outcomes of sociocultural selection. As Weber proposed, intentions refer to the meaning an actor attaches to his or her actions. A society, a state or a scientific discipline do not have intentions. And from the fact that individuals are intentional, perhaps even rational actors, it does not follow that the actions of a number of individuals will, in the aggregate, lead to purposively organized, let alone globally maximizing selections. As most social scientists

will agree, it is difficult to maintain that societies, states and even smaller units such as universities change through processes of globally maximizing rational selection. Why, then, is it more plausible to assert that science, scientific disciplines or specialist communities are governed by such processes? Aggregates can be globally maximizing, or less strongly put, can have organized supra-individual outcomes if they are structured, effectively cooperating systems with superordinate controls. Or if all individuals in the aggregate are altruistic optimizers of the common good and, with respect to their selection strategies, exact copies of each other. But, as I have argued before, scientific disciplines or communities are not cybernetic systems. Nor do we believe, after many years of Parsons-criticism, that individuals in whatever aggregate share enough goals, values, reasoning strategies and information in order for the sum of their independent efforts to result in just what the globally maximizing outcome was supposed to be. In fact, evolutionary theory does not support the notion that individuals seek global outcomes. Instead, it favors a notion of context-embedded circumspection in which self-interest and self-related consideration play no small role.

My argument then is that given human intentionality and purposiveness it makes sense to analyze scientists' selections in terms of local reason, but that in the aggregate scientific *selection is blind*. Global outcomes have the character of retrospectively *adopted* consequences which arise behind the back of agents from non-communal, often inadvertent local choices. However, this formulation still neglects the fact that like discovery, selective retention in science is a double-headed (self-referential) phenomenon. It includes the process of selection-making as well as participants' selective editing of this process through recording and formatting practices. These can be seen at work not only in the textbooks and teachings of science, but also in scientists' biographies, notebooks and laboratory accounts, to the degree to which it is often difficult to distinguish selection from the editing of selection. However, unlike my ethnomethodological colleagues who will not agree with me on this, I still think the distinction is useful, for the following reason: In the last analysis, all selection appears to be edited once it is made reportable, but not all selection appears to be "up-graded" in the process to a summary record. As an

illustration, think of the video-recording of a process. Clearly the camera edits the picture through lighting, camera angle, lens used and other means, but it does not normally give us a summary record of occurrences (its not doing so is precisely the problem of video-analysis). On the other hand, indicators derived from questionnaire answers usually provide a highly edited and aggregated account of the phenomena to which they refer. If we refer to this aggregation as a change in format, we might say that selective retention in science appears to be procedurally open-ended and opportunistic and editorially directional and closed.

7. SELECTION THEORY NEEDS VARIATION, NOT INNOVATION

I want to end these speculations about selective retention by a brief note on variation, which in a way brings me back to the discovery model from which I started. The claim was that from the point of view of selection theory, the process of selective retention needs variations, but does not require us to adopt a model which locates the origin of these variations in some equivalent of genetic mutations. With regard to science this has the advantage of avoiding virtually all of the objections raised against the use of evolutionary theory as a model of scientific discovery, including my own. For one thing, it is quite another matter to account for the origin of variants than to explain scientific innovation in terms of the nearly unaccountable spontaneous generation which characterizes the birth of mutations. A theory of "variations" can accomodate the claim that much of our "creative" extension of knowledge is apparently based upon the selective mobilization of old ideas by means of metaphor and analogy. It can accomodate the claim, which I think is implied by contemporary research and theoretical work, that there exists a tremendous reservoir of variations in science which mainly derive from the fact that scientists are not very interested in replication and much more interested in varying the components of technical work in order to adjust to local circumstances and enhance the probability of technical success. And it can accomodate the claim that in science, the process in which conceptual variations are produced is at the same time the process in which previous selections are either selectively reproduced or "weeded out", whereas this does not hold for biological evolution. Finally, if,

as I have argued, "discovery" in science is better understood as a process of selective editing than as a process of novelty generation, this makes no differences with respect to a theory of scientific variation, whereas it does deal a further blow, I think, to a theory of mutation-like scientific innovation. Variation theory might simply include the point that designated novelties result from various processes of editing variations, and explore the consequences of this phenomenon for the status and perception of variations.

In short, my argument is that whatever selection theory needs to say with regard to scientific 'mutations' should be construed as a theory about scientific variation. Variations can be explained in ways compatible with the objections raised against the use of evolutionary theory as a model of scientific innovation, and accomodate our current logical, psychological and sociological understanding of scientific innovation.

NOTES

1. There are exceptions of course. See for example Hesse (1980), Nickles (1980), and Giere (1984).

2. Although it is not the first attempt to develop an EE. Don Campbell has made me aware of the fact that Simmel, as early as 1895, wrote a paper on the relationship between selection theory and epistemology (Simmel, 1895).

3. Compare Hesse's (1974) use of the term "primary recognition".

4. This has been argued by Whitley (1978). It is fair to say that Toulmin (1967, part 4) sometimes refers to specialities when he talks about "disciplines".

5. For the definition of a cybernetic system, see Corning (1983, p. 210).

6. Ethnoscience distinguishes between "emic" (from phonemic), participant centered, and "etic" (from phonetic) intercultural approaches. See Pike (1967, p. 37ff.).

7. See Corning (1983, pp. 99-100).

8. The following quotes and arguments are cited after Hull (1982).