

PARADIGMS, DISCIPLINES, AND HUMAN UNDERSTANDING

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Introduction

How is the development of our knowledge and understanding of the natural and human world to be conceptualized? This is the central question confronting anyone who presumes to undertake the theory or practice of any organized rational activity, be it natural science, philosophy, or any of the anthropological sciences. And this question is to be answered, not once, but time and again for each and from each of these three perspectives.

*When he published **The Structure of Scientific Revolutions** in 1962, T.S. Kuhn proposed a controversial role for sociology in the analysis of changes within the project of science. Using notions developed by R.G. Collingwood in the 1940's, Kuhn emphasized the social construction of reality within the scientific group which determines, not only what counts as a "scientific" problem or solution, but also what is accepted by the scientific community as a "fact." Science, in Kuhn's account, was characterized by both synchronic changes which he called the process of "normal science" and by diachronic "revolutionary" episodes in which one "paradigm" (model of scientific practice) was replaced by another. In Kuhn's analysis of paradigm changes one standard of reason is replaced by another incompatible standard. But, because no standard of reason can be given for the replacement of one paradigm with another, Kuhn's critics have labeled the concept of scientific revolutions "relativist."*

*One of Kuhn's most sympathetic critics, Stephen Toulmin, wrote an extended critique of the notion of scientific revolutions in volume one of **Human Understanding** (1972). Unlike some of Kuhn's other critics who adhere to the traditional view that science develops by the accretion of theory-independent "facts," Toulmin recognized a central role for sociology in understanding the scientific enterprise. In his "ecological" account Toulmin sought to provide a standard of reason located within the scientific discipline, thus relegating the most profound and abrupt changes to synchronic status*

and doing away with the necessity for a notion of "scientific revolutions."

Using the example of Vasco Ronchi, an Italian optician, I will exhibit some of the failings of Toulmin's disciplinary account of scientific change. In Ronchi's case a breakthrough was achieved, not within a scientific discipline but in spite of the disciplinary structure of modern science. Instead of the disciplinary matrix facilitating human understanding of the phenomena of vision, the existence of a psychology of perception distinct from optical physics interfered with the process of science. Thus, by playing Ronchi against Toulmin, it is possible to undertake a sympathetic criticism of Toulmin's "intellectual ecology." First I will discuss R.G. Collingwood's explanation of "constellations of presuppositions" and T.S. Kuhn's concept of scientific revolutions; this will be followed by an exposition of S.E. Toulmin's counter-perspective called "intellectual ecology." Using "Vasco Ronchi's Revolution in Optics" as a paradigm for scientific revolutions, I will argue the inadequacies of Toulmin's position. In conclusion, it is argued that while disciplines serve to organize the scientific enterprise and have aided in the development of an impressive technology, the disciplinary structure also leads to a conception of scientific change being beyond human control.

In his slightly antagonistic review of the history of revolutionary perspectives on the development of science, Stephen Toulmin pointed out that R.G. Collingwood, writing in the 1940's, laid the groundwork for Kuhn's analysis of paradigmatic organization within the sciences (Toulmin, 1972: 73-74).

1. The intellectual content of a discipline, at any given stage in its development, comprises a system of concepts and principles operating on different levels of generality.
2. Our acceptance of concepts and propositions on the lower levels of generality is "relative to" those on the higher levels, and such lower level concepts and propositions are presupposed only "relatively" to those on the more general levels.
3. When we reach the most general level of all, our reasons for accepting concepts and principles cannot be explained in terms "relative to" any more general considerations so that those upper-level concepts and propositions are presupposed not relatively, but "absolutely."

3. At any given stage in the development of a discipline, different propositions and concepts can be rationally compared, to the extent that they are both operative "relative to" the same constellation of absolute presuppositions.
5. But no common, agreed principles or procedures of judgment are available for comparing propositions or concepts "relative to" different constellations of absolute presuppositions, or for comparing different constellations of absolute presuppositions in their entirety.
6. So propositions and concepts can be rationally appraised only "relative to" one particular constellation of absolute presuppositions, viz. that within which they are operative and once we leave the scope of one particular framework, we also leave the scope of rational comparison and judgment.

From this perspective, any attempt to answer the questions raised in the introduction involves the investigator in "metaphysics." Metaphysics, according to Collingwood, is the investigation and description of these absolute presuppositions. More importantly, the task of metaphysics is to "find out on what occasions and by what processes one (constellation of absolute presuppositions) has turned into another" (Collingwood, 1940:73). Unfortunately, Collingwood can provide no answer to the crucial issue of whether such changes in constellations of absolute presuppositions are "reasoned" or "caused." He recognized that a change in absolute presuppositions "is the most radical change a man can undergo, and entails the abandonment of his most firmly held habits and standards of thought and action." These changes take place because, "The absolute presuppositions of any given society, at any phase in its history, form a structure which is subject to strains of greater or less intensity, which are "taken up" in various ways, but never annihilated. If the strains are too great, the structure collapses and is replaced by another, which will be a modification of the old with the destructive strain removed" (Collingwood, 1940:48n). However, in Collingwood's description of this process these modifications are not devised consciously but created by "unconscious" processes of thought.

Clearly, Collingwood provided no answer to the central question raised by Stephen Toulmin in *Human Understanding* (1972:7):

How far are our concepts—even the most basic ones—derived from our sense experience? Must our claims to knowledge be backed in particular by sensory evidence? Or do our concepts and categories, rather, predetermine our abilities to perceive and recognize? Do people with different concepts and languages even see the world differently? *Either way, how can we compare the merits of rival concepts?* (Emphasis added.)

Because of this failing, the inability to provide an account of why one set of absolute presuppositions should be rejected in favor of another, Toulmin brands Collingwood's position as "relativist."

Kuhn's Work Is Center of Controversy

T.S. Kuhn's work on *The Structure of Scientific Revolutions* has been the center of controversy since its publication in 1962. Kuhn's analysis shares a common emphasis on the social construction of scientific reality with that of R.G. Collingwood; however, the two accounts of the scientific enterprise are different. Where Collingwood addressed himself to science conceived of as a "constellation of absolute presuppositions" held together by logical relationships, Kuhn shifts ground to speak of "paradigms" and "paradigm communities."

I use the summary of Kuhn appearing in a critical review of Kuhn's work [the second edition of *The Structure of Scientific Revolutions* (1970) and his essays in *Criticism and the Growth of Knowledge* (1970)]¹. Professor Dudley Shapere's review (*Science*, Vol. 172, May 14, 1971, pp. 706ff.) not only provides a good summary of Kuhn's work but assists in formulating the issues it raises. Shapere first summarizes the salient arguments of the first edition:

The thesis of the original edition was that "particular coherent traditions of scientific research" (1962:10), which Kuhn called "normal science," are unified by and emerge from "paradigms." Paradigms are "universally recognizable scientific achievements that for a time provide model problems and solutions to a community of practitioners" (1962:X). Kuhn conceived of a paradigm as not identifiable with any body of theory, being more

"global" (1962:43) and generally incapable of complete formulation. He held it to include "law, theory, application, and instrumentation together" (1962:10). Consisting of a "strong network of commitments, conceptual, theoretical, instrumental, and methodological" (1962:42), and even "quasi-metaphysical" (1962:41); it is, he claimed, "the source of the methods, problem-field, and standards of solution accepted by any mature scientific community at any given time" (1962:102), permitting "selection, evaluation, and criticism" (1962:17). "Normal science" consists of working within and in the light of the paradigm, making it more and more explicit and precise, actualizing its initial promise "by extending the knowledge of those facts that the paradigm displays as particularly revealing, by increasing the match between those facts and the paradigm's predictions, and by further articulation of the paradigm itself" (1962:24). In the course of such articulation, however, "anomalies" arise which, after repeated efforts to resolve them have failed, give birth to the kind of situation in which a scientific revolution can take place: "Confronted with anomaly or with crisis, scientists take a different attitude toward existing paradigms, and the nature of their research changes accordingly. The proliferation of competing articulations, the willingness to try anything, the expression of explicit discontent, the recourse to philosophy and to debate over fundamentals, all these are symptoms of a transition from normal to extraordinary research. . . . Scientific revolutions are inaugurated by a growing sense ... that an existing paradigm has ceased to function adequately in the exploration of an aspect of nature to which that paradigm has itself had previously led the way" (1962:90-91).

New candidates for fundamental paradigm are introduced; ultimately one may become accepted, often necessitating "a redefinition of the corresponding science" (1962:102). Kuhn emphasized that scientific revolutions are "non-cumulative developmental episodes in which an older paradigm is replaced in whole or in part by an incompatible new one" (1962:91).

Kuhn's views diverge radically from those dominant since Mach and Ostwald, developed in the outlook of the Vienna Circle

and its intellectual associates, and paralleled in the views of Bridgman and Frank and a host of more recent thinkers. Whereas those views tended, at least in their heydays, to separate sharply "fact" (or "observation" or "operation") from "interpretation"—thus claiming to preserve the "objectivity" of science—Kuhn emphasizes the dependence of what counts as a "fact," a "problem," and a "solution of a problem" on presuppositions, theoretical or otherwise, explicit or implicit. Likewise he attacks traditional "development-by-accumulation" views of science—views according to which science progresses linearly by accumulation of theory-independent facts, older theories giving way successively to wider, more inclusive ones.

As Professor Shapere and many others have observed, Kuhn, at least in his first edition, comes off as a "relativist":

In emphasizing the determinate role of background paradigms, and attacking the notion of theory- (or paradigm-) independent "facts" (or any such independent factors or standards whatever), Kuhn appears to have denied the possibility of reasonable judgment, on objective grounds, in paradigm choice; there can be no good reason for accepting a new paradigm, for the very notion of a "good reason" has been made paradigm-dependent. And certainly, though in some passages Kuhn denied this implication of his view, in most he gloried in it: "the competition between paradigms is not the sort of battle that can be resolved by proofs" (1962:174), but is more like a "conversion experience" (1962:150); "what occurred (in a paradigm change) was neither a decline nor a raising of standards, but simply a change demanded by the adoption of a new paradigm" (1962:107); "in these matters neither proof nor error is at issue" (1962:150); "We may ... have to relinquish the notion, explicit or implicit, that changes of paradigm carry scientists and those who learn from them closer and closer to the truth" (1962:169). Objectivity and progress, the pride of traditional interpretations of science, have been abandoned. Indeed, Kuhn's relativism did not stop here: for not only is there no means of rationally assessing two competing paradigms; there is no way of comparing them at all, so different is the world as seen through them (or—in an alterna-

formulation that is in many ways more consonant with Kuhn's general thesis—so different are the worlds they define). "The normal scientific tradition that emerges from a scientific revolution is not only incompatible but often actually incommensurable with that which has gone before" (1962:102). Kuhn carried this view to the point of holding that if the same terms continue to be used after a scientific revolution (like "mass" after the replacement of the Newtonian by the Einsteinian "paradigm") those terms have different meanings.

Shapere further points out that in the second edition (1970) Kuhn, in response to criticism of the obscurity of his notion of paradigms, adopts two definitions of "paradigm." Kuhn now wishes to distinguish two different senses of the term:

On the one hand, it stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community. On the other, it denotes one sort of element in that constellation, the concrete puzzle-solutions which, employed as models or example, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science (1970: 173).

For the former, broader sense Kuhn suggests the name "disciplinary matrix," distinguishing four components of such matrices (1970:182-186): "symbolic generalizations," "meta-physical paradigms," "values," and "exemplars," the "concrete puzzle-solutions" referred to above. All these elements were lumped together in the first edition; "they are, however, no longer to be discussed as though they were all of a piece" (1970:182). This distinction, however, is of little help to those who found the earlier concept of "paradigm" obscure. Contrary to Kuhn's complaint, few critics failed to see that the *primary* sense of "paradigm" had to do with the "concrete puzzle-solution." The difficulty was, rather, that Kuhn never adequately clarified how the remaining factors covered by that term were related to (embodied in) the concrete examples in such a way that the whole outlook ("paradigm" in the broader sense) of the tradition would be conveyed to students through such examples. Nor did he clarify

the ways in which, through the concrete examples, this general paradigm determined the course of scientific research and judgment. Yet it was precisely this unity, and the controlling status, of paradigms that constituted the appeal and the challenge of Kuhn's original view: the contention that there was a coherent unified viewpoint, a single overarching *Weltanschauung*, a disciplinary *Zeitgeist*, that determined the way the scientists of a given tradition viewed and dealt with the world, that determined what they would consider to be a legitimate problem, a piece of evidence, a good reason, an acceptable solution, and so on. (The affinities of Kuhn's view with 19th-Century Idealism run deep.)

The affinities with Nineteenth-Century Idealism and its successors are part of what I wish to explore in this paper. Shapere also notes that in the second edition Kuhn is less explicitly "relativistic" and admits the existence of some criteria as to theoretical validity:

Now, what counts as a scientific problem is not determined, at least completely, by the paradigm: "Most of the puzzles of normal science are directly presented by nature, and all involve nature indirectly" (Lakatos and Musgrave, 1970:263); there is, apparently, a paradigm-independent objective world (nature) which presents problems that a paradigm must solve.¹ Further, paradigms no longer, apparently, determine, at least completely, what counts as a good reason: "It should be easy to design a list of criteria that would enable an uncommitted observer to distinguish the earlier from the most recent theory time after time. Among the most useful would be: accuracy of prediction, particularly of quantitative prediction; the balance between esoteric and everyday matter; and the number of different problems solved.... Those lists are not yet the ones required, but I have no doubt that they can be completed. If they can, then scientific development is, like biological, a unidirectional and irreversible process. Later scientific theories are better than earlier ones for

¹Professor Shapere is quoting from *Criticism and the Growth of Knowledge* edited by Imre Lakatos and Alan Musgrave (New York: Cambridge University Press, 1970).

solving puzzles in the often quite different environments to which they are applied. This is not a relativist's position, and it displays the sense in which I am a convinced believer in scientific progress" (Kuhn, 1970:206).

Whether T.S. Kuhn is himself a "relativist" or a "believer in scientific progress" is not at issue here, and one must forgive Professor Shapere and Kuhn's other critics for their overemphasis on his weak spot. Kuhn is not, after all, a Nineteenth Century Idealist, and just as clearly he never denied the existence of the material world. On the other hand, Kuhn's reply to his critics will also be shown to be unsatisfactory.

Finally, Shapere criticized Kuhn for locating rationality far too largely in the scientific "group" and the construction of reality it makes, sanctions, and can sustain "politically":

The fundamental question is, "Do scientists (at least sometimes, even in "revolutionary" episodes) proceed as they do because there are objective reasons for doing so, or do we call these procedures "reasonable" merely because a certain group sanctions them?" Despite the ambiguities and inconsistencies of many of his remarks, Kuhn's tendency is clearly toward the latter alternative. Though occasionally tentative ("some of the principles deployed in my explanation of science are irreducibly sociological, at least at this time") (Lakatos and Musgrave, 1970:237)—in most passages he asserts his view categorically: "The explanation (of scientific progress) must, in the final analysis, be psychological or sociological. . . . I doubt that there is another sort of answer to be found" (Lakatos and Musgrave, 1970:21). "Whatever scientific progress may be, we must account for it by examining the nature of the scientific group, discovering what it values, what it tolerates, and what it disdains. That position is intrinsically sociological" (Lakatos and Musgrave, 1970:238). We must study scientific communities not as one of several steps in clarifying the nature of science (in attempting, say, to separate the irrational from the rational components as a prelude to analyzing the latter); it is the *only* step. What the community says is rational, scientific, is so; beyond this, there is no answer to be found. An alternative to this view is to think of sociology as able

to bring to our attention kinds of biases which scientists should try to avoid, as interferences, hindrances to good scientific judgment. For Kuhn, however, such biases are an integral, and indeed the central, aspect of science. The point I have tried to make is . . . that Kuhn's is a view which denies the objectivity and rationality of the scientific enterprise...

These, then, are the problems raised by Kuhn's work:

1. How are paradigms created and sustained in natural science, philosophy, or the social sciences?
2. Are there, indeed, scientific revolutions?
3. Are the criteria for judging them "objective" or "subjective," "individual" or "social," "disinterested" or "political?"

Collingwood and Kuhn Differ From Establishment

Both Collingwood and Kuhn express a position which remains different from the establishment perspective in emphasizing the group construction of reality. Both emphasize a role for the "sociology of science" which is more central and determinate than in previous accounts of the scientific enterprise. This, and not the concept of "paradigms" or the notion of "scientific revolutions," was Kuhn's contribution to the critical comprehension of the scientific project. And, I suspect, this is the aspect of the theory which calls forth Kuhn's critics. Previous accounts of science, which made use of formal logical relations of induction, deduction, falsification, etc., viewed science as an enterprise outside and separate from the rest of social life, and with some justification, ignored the impact of social beliefs and values. Kuhn, in *The Structure of Scientific Revolutions*, raised problems of historical relevance that the classical approach to science was unable to answer, even though Kuhn's own solutions of "relativism" (the early Kuhn) or "belief in inevitable progress" (the later Kuhn) were considered by critics to be unsatisfactory.

In 1972, Stephen Toulmin published *Human Understanding*, which is an extended effort to critique Kuhn's position from a perspective different from Shapere's. Where Shapere does not give the "sociology of science"

much credence except in informing scientists as to their "unscientific" biases, Toulmin at least recognizes a central role for sociology in understanding what science is all about. Toulmin's position is that what is fundamental to the development of "human understanding" are not the "paradigms" subject to radical transformation in "crisis" times, but rather the "disciplines"—that is, the assumptions of the continuing scientific community as to how knowledge is to be gained (i.e., physiological processes are to be explained in chemical terms). Thus, Toulmin concentrates on how groups of men and women develop the "procedures" for understanding, change those procedures, evaluate their efficacy and the understanding gathered from them. Toulmin and Kuhn are treating fundamentally different processes: Kuhn, the "axioms-systems" or ways of relating "parts" to "wholes" of meaning-systems which, in turn, become grand paradigms for interpreting physical or other "reality"; Toulmin, the procedures for investigating and critiquing the "systems" which seem to unite groups of investigators into a single "discipline." Yet, Toulmin, at least, would insist that the phenomena which he treats are related to, in some sense logically prior to, those which Kuhn treats.

Kuhn's analysis, in Toulmin's view, derives from Collingwood and also, like Collingwood's, leaves the reader with no sense of why one paradigm is rejected and another accepted. It is as if one *government* were overthrown by another *government* without there existing the possibility of appeal to a higher principle which justified or did not justify the "overthrow":

Political revolutions aim to change political institutions in ways that those institutions themselves prohibit. Their success therefore necessitates the partial relinquishment of one set of institutions in favor of another, and in the interim, society is not governed by institutions at all (Kuhn, 1970:93).

Like the choice between competing political institutions, that between competing paradigms proves to be a choice between incompatible modes of community life (Kuhn, 1970:94).

Toulmin wants to avoid this way of looking at conceptual change by showing what the "authority" is—the principle, set of principles, or set of principles and people—to which we appeal when we throw out one paradigm (e.g., Newtonian explanations of "mass") and substitute another (e.g., Einsteinian "mass").

Toulmin's own account of how this relativism may be avoided is paraphrased as follows:

1. The historical and cultural diversity of men's ideas underlies the need for an impartial standpoint of rational judgment; and it appears, at first sight, that this can be provided only in abstract logical terms, claiming absolute and universal rational authority over the concepts and judgments of all milieus.
2. The moment we go beyond pure mathematics and formal logic, however, the attempt to identify such an absolute standpoint runs into difficulties of historical relevance, leaving us with no evident alternative except to abandon our search, and lapse into historical and cultural relativism.
3. But both the absolutist and relativist positions turn out to rest on the common erroneous assumption, that "rationality" must be an attribute of some particular logical or conceptual "system"; they differ only in locating our rational standpoint, in the one case, in an idealized, abstract system, and in the other case in some actual but arbitrarily chosen system.
4. We must begin, therefore, by recognizing that rationality is an attribute, not of logical or conceptual systems as such, but of the human activities or enterprises of which particular sets of concepts are the temporary cross-sections: specifically, of the procedures by which concepts, judgments, and formal systems accepted in those enterprises are criticized and changed.
5. When we turn to consider the process of conceptual change, however, a similar dilemma reappears on the historical level: now, we seem forced to choose between a uniformitarian account, which assumes the universal relevance of a single set of rational methods, and a revolutionary account, which treats conceptual change as a sequence of radical switches between rationally incommensurable positions.

6. But this second dilemma can also be avoided, provided that we recognize two further distinctions: (i) between the theoretical concepts and principles of a discipline—which can, and may, change discontinuously—and the disciplinary concepts and principles which are for the time being constitutive of the discipline, and change more gradually; (ii) between the specific theories of a discipline—each with its own particular family and/or system of concepts—and the intellectual content of an entire subject, which comprises a changing "population" of concepts, and families of concepts, that are in general logically independent of one another (Toulmin, 1972:133-134).

Toulmin's argument for 6 above is as follows:

1. Within any particular culture and epoch, man's intellectual enterprises do not form an unordered continuum. Instead, they fall into more or less separate and well-defined "disciplines," each characterized by its own body of concepts, methods, and fundamental aims. Considered over a long enough period, the intellectual content of a discipline can change quite drastically; and so also, though more slowly, may its intellectual methods and aims. Yet each discipline, though mutable, normally displays a recognizable continuity, particularly in the selective factors that govern changes in its content. An evolutionary account of conceptual development accordingly has two separate features to explain: the coherence and continuity by which we identify disciplines as distinct, and, the profound long-term changes by which they are transformed or superseded.
2. These continuities and changes both involve the same dual process. In any live discipline, intellectual novelties are always entering the current pool of ideas and techniques up for discussion, but only a few of these novelties win an established place in the relevant discipline, and are transmitted to the next generation of workers. The continuing emergence of intellectual innovations is thus balanced against a continuing process of critical selection. Some

conceptual variants are picked out for incorporation, others are weeded out or ignored; yet, in suitable circumstances, this same process can account either for the continued stability of a well-defined discipline, or for its rapid transformation into something new and different.

3. This dual process can produce a marked conceptual change, only given certain further conditions. We assume that enough men of natural inventiveness and curiosity exist to maintain a flow of intellectual innovations or "variants" . . . there must (also) exist suitable "forums of competition," within which intellectual novelties can survive for long enough to show their merits or defects; but in which they are also criticized and weeded out with enough severity to maintain the coherence of the discipline.
4. Finally, an evolutionary analysis of intellectual development once again involves a set of interdependent notions, which between them define the "intellectual ecology" of any particular historical and cultural situation. In any problem situation, the disciplinary selection process picks out for "accreditation" those of the "competing" novelties which meet the specific "demands" of the local "intellectual environment." These "demands" comprise both the immediate issues that each conceptual variant is designed to deal with, and also the other *entrenched concepts* with which it must co-exist (Toulmin, 1972:140).

Thus, what seemed in Kuhn's analysis "revolutionary"—that is, sudden paradigm change as a consequence of movement from one theory to another dependent on the substitution of one set of "upper level" concepts and pre-suppositions for another (e.g., Einstein's for Newton's)—is seen as conducted within the parameters of a conservative "ecology" provided by the discipline. However, Toulmin's "ecological" metaphor is subject to challenge, as is his supposition that basic or "revolutionary" conceptual change takes place within the parameters, procedural assumptions, social group and so forth, of a single discipline. To display the limitations of these two ideas, I want to look at a paradigm change in optics—that developed by Vasco Ronchi. But, before that, I need to summarize, further, some of Toulmin's observations about Kuhn.

History of Science Must Also Be Sociology

Toulmin acknowledged the contribution made by Kuhn, namely that a history of science must also be a sociology:

... A comprehensive account of conceptual development must not merely consider concepts in the abstract, and in isolation from the men who conceive and use them, but also relate the history of ideas to the history of people, so placing the development of our conceptual traditions within the evolution of the activities by which these traditions are carried (1972:116).

However, he does not follow through and discuss the real relations between *social life* and *scientific paradigms*; instead he uses an inept scheme of a "planless" Darwinism, and ends up treating scientific achievement as, in a sense, beyond human control. Science becomes the product of many single selections which are for the most part logical "go, no go" decisions on novel, but synchronic, conceptual variants. When "major" conceptual variants are to be decided upon, say between paradigms, this is done by the process of "survival of the fittest" before a *disciplinary* "court of reason."

Toulmin aims to provide an account of conceptual change which can deal with changes of profundity, not simply a "revolutionary" scheme which describes how entire conceptual systems succeed and replace one another. However, he does this within the institutional framework provided by disciplines. As I have mentioned, in his analysis, he distinguished between concepts and principles of two different kinds within any actual science: the basic "theoretical" principles, e.g., the principle of universal gravitation; and "disciplinary" principles, e.g., all physiological processes are to be explained in chemical terms. The disciplinary principles define the basic intellectual goals of a science and give it recognizable unity and continuity.

Unfortunately, this distinction raises more questions about the nature of the scientific enterprise than it answers, and raises them precisely in the area where Toulmin makes his argument. Toulmin supports his assertion with a fairly intensive discussion of two approaches to the theory of vision: On the one hand, Newton's *Optiks* which is presented as a "physical" theory; on the other, Goethe's *Farbenlehre*, which he interprets as a "psychological" theory of perception. In his view, then, Goethe's difference with Newton

was, thus, not a straightforward disagreement within a single common discipline, "since the interests of the two men overlapped only at the margin" (Toulmin, 1972:125). While Goethe's paradigm looks like an attempt at a scientific revolution, different from Copernicus *only* in that it failed, Toulmin claims a closer look reveals that the cases are not parallel. The Copernican "revolution" took place within a discipline characterized by common aims, whereas Goethe's paradigm did not share either common theoretical or disciplinary principles with Newton's (Toulmin, 1972:125ff.). For Toulmin, even radical changes within a discipline are not revolutionary because of the continuity provided by the common "disciplinary principles"; and there is no possibility for a revolution between disciplines because of the mutual "unintelligibility" created by different explanatory aims.

If one accepts the phenomena of vision as what both men are trying to explain, the central question appears to have been skirted, not only by Toulmin but by *all* positivists, as is clear from the researches of Vasco Ronchi. Going back to Kepler's original paradigm, the theory of light has three analytically distinct parts: (1) a physical theory treating light in geometrical terms, (2) a physiological process determined by the action of light rays on the retina, and (3) a psychic process by which we situate at a distance the object whose image lies on our retina, a process also treated by Kepler in geometrical terms.

Thus, Kepler unconsciously laid a real trap for succeeding generations. . . . In fact he proposed a double-edged theory. Its experimental setup is based on a dualism between objective radiation and psychic representation. Yet the mathematical-geometric treatment of the theory is neutral, since both physical and psychic elements are analyzed by a method allowing their behavior and measurement to be treated in the same way, as though there were no qualitative difference between them. The historical consequences of this ambivalence have been, first of all, that the brilliant geometric solution has made us forget that the similarity of terms in the geometric analysis did not imply a similarity in their nature, and secondly, that the whole of Kepler's theory, inasmuch as it was not an abstract geometric hypothesis but an explanation of the mechanism of vision, was really founded on a distinction between physical and psychic elements. The occlusion of the subjective psychic element was accomplished without replacing Kepler's theory of vision with another theory based only on physical elements. Instead, it has continued to be an indis-

pensable, fundamental presupposition of all further research, notwithstanding the fact that either it must be accepted in all its physical and psychic aspects or else *it loses all meaning since it ceases to be a plausible theory of vision* (Trogu, 1971:7).

The phenomena of vision is neither explicable as *a purely physical event* nor as *a purely psychological event*. That is, the procedures neither of physics nor of psychology are adequate to it. It is both and neither. And if anything like a coherent explanation is to be given, these two conceptions must be operational.

Toulmin's answer, that the two *explanations come from different disciplines, is deficient* since it is that "overlap" which contains the only adequate description of the phenomena. In effect he is making the Cartesian mind-body, subject-object distinction which has been carefully and systematically attacked, not only by positivists who ultimately set it aside as Toulmin himself does, but by phenomenologists:

The positivist solution postulates a theory of human experience based on physical, physiological, and psychological elements, *all of which are material* (emphasis added). Yet it accepts the initially experienced data not as the results of interaction between the external world and the human organism but rather as if they were *original data*. If the result more or less investigated in the operations that allow a personal experience of it can be one and only one, then the problem disappears of itself. If analysis of the interaction between the external world and the human body leads us only to realize that the experienced world cannot be other than what it really is, then science can very well disregard the matter as irrelevant and consider experience as undeniable data upon which to base its operations and scientific investigations. If instead the study of human operations which produce data leads us to consider the data as *something different, upon which we can perform otherwise inconceivable experimental operations*, then the question demands serious consideration. . . . In fact, history shows that science has made genuine leaps forward, just as it has in our day with Ronchi, whenever it has examined the initial data understood as a result and has reconsidered the data as something substantially different from what they were initially believed to be (Trogu, 1971:16).

The fragmentation of the phenomenal world into disciplines is itself the result of a positivism which accepts as initial data those perceptions already mediated by the human organism. Vasco Ronchi became aware of the need to reevaluate the theory of vision in exactly the way that Kuhn indicates a scientific revolution begins. That is, he noticed a widening discrepancy between the geometry-of-vision's theoretical predictions, and the actual measurements made with, and the action of, optical devices. Once aware of these problems, Ronchi began the most profoundly revolutionary journey, which led him to consider the absolute presuppositions of all the sciences. If science is taken as the human apprehension of reality, the artificial fragmentation of that reality into "hard data" and "perceptions" stands in the way of that apprehension. By synthetically merging these two aspects of science, Ronchi is equipped to reconsider optics. Here no justification for the separation of the phenomena of vision into a physical account and a psychological one can be tolerated. And Ronchi's revolution in optics shows that this disciplinary distinction precludes the understanding of vision. Thus, Ronchi's accomplishment is not even describable as *a synthesis between a physical and a perceptive theory of vision*; instead he shows that the disciplines themselves were not independent in the first place.

I submit that Vasco Ronchi has presented us with a modern, accessible case of a real scientific revolution, one not simply in the field of optics, and certainly not just a "radical" change within a discipline. His revolution cannot be described as a conceptual variant, as a new discipline, or as a synthesis between existing disciplines: if at all describable in Toulmin's terms, what Ronchi has done is to overthrow the "court of reason" itself. This revolution in optics was a fundamental perceptual one, with the most profound consequences for every branch of science in which initial data has been obtained by optical instruments, because he revealed a blind spot in the theory (Trogu, 1971:19). He demonstrated that the existing science of optics was in "total and irreconcilable opposition to experience and that all the grand successes of optics were actually due to a technology developed independently of, and often in conflict with, the theoretical formulations of the science of optics" (Trogu, 1971:4). He did this by bracketing his (and everyone else's) absolute presuppositions at the most basic level—an earthshaking and very political thing to do. As Trogu (1971:19-20) observes in the end, this revolution involves a transcendental project:

History teaches that *every* cosmogonic conception comes to

be generally lived as the only possible one by the society that believes in it. It also teaches, however, that over a period of time, man has lived by believing in very different cosmogonic conceptions. Then, even the cosmogonic conception underlying the horizon of scientific operations ought to be considered one of several possible ones. It has provided the solution to a great number of problems and it has changed the face of the earth and humanity's way of life. Yet it cannot be considered definitive, because it leaves unsolved, and by definition irresolvable, some of the problems that interest man the most. Thus it is the task of philosophical investigation to determine whether historical circumstances permit an attempt at delineating another cosmogonic conception, *different from the common one as well as the scientific one*, which would be more responsive to man's present needs.

The presupposition of positivism that the physiological, physical and psychological elements of experience should be described in neutral (interpretation-free) terms, is the basis accepted by Stephen Toulmin, which created the separate disciplines in established science as definitive of reality. And while he pays lip service to the variable nature of human rationality, he is unwilling to deal with changes outside the positivistic framework of his paradigm. Thus, the discipline of optics was for hundreds of years considered to exist as an entity separate and distinct from psychological theories of perception, because the presuppositions of positivism *define* the optical phenomena as existing apart from the perceiving subject, in the same way that Newtonian mechanics consider the movement of inanimate bodies apart from their other qualities such as price, value, or the intention of the experimenter who originally imparts motion to them. As Vasco Ronchi has shown, no further progress in optical science could be made as long as this distinction held. This distinction had to be ignored by treating the cosmogonic conception of positivism as a "factor" instead of a "fact" of existence, a process which Toulmin does not seem to be able to account for.

Paradigm Revolutions Illustrated By Ronchi's Example

Ronchi's paradigm revolution may be taken as an example of paradigm revolutions. It questions basic postulates (first principles). It takes place in

an interdisciplinary context where many disciplines exist and none are sufficient within themselves. It emphasizes the extent to which man is part of nature and, at the same time, the extent to which the nature which man knows and can to some degree alter is a construct of man, a product of his inferential or concept-making powers.

Man, as a species, is the product of the evolution of species, and in this respect he is subject to the processes of variation and selection in the same way that every other species is. Yet, the processes of evolution have selected out, for better or worse, a collection of traits which distinguishes man from the other species of which we have knowledge. Man is a producer, and it is the process of production which allows man to alter his environment, to interfere in the previously external process of evolution, and to internalize and ultimately subject this process to human control. Without man, unless one takes the theistic position, there is no teleology outside the random process of variation and selection; nonetheless, with the development of man came purpose, and with purpose, the possibility of teleological concern and the intentional construction of reality.

Man is the only species that is both the object of the process of evolution and its increasingly conscious subject. The Marxian distinction accounts for man making his own history, and is behind Marx's perception that man reproduces the whole of nature while animals reproduce only themselves—a perception evident in Ronchi's experiment. Through knowledge and production, man creates his own conscious ends; he works according to conscious plan and is possessed of intentionality. This *alone* makes the development of man's knowledge different from the process of biological evolution, and makes Toulmin's "ecological" account seem suspect.

The evolution of concepts, seen as a process of natural selection from among a pool of "random" variants, pictures a runaway "ideal" world where man develops but does not and cannot control things. The realm of science begins to resemble the marketplace where any novelty which "sells" is seen as progressive.

Toulmin explicitly rejects the Marxian concept of evolution as "unilinear," suggesting instead his own "populational" version, but still rejects man's ability to project the future and to control his development within the accords of his own design, thus subjecting humanity to a tyrannical

technological determinism.

Within the whole German tradition of historicism—both its Hegelian or idealist, and its Marxist or Materialist forms—great stress was also laid on the Rationality of that Historical Process; and the idea of "progression" was linked with the idea that history advanced in accordance with an internal "dialectic" (Toulmin, 1972:329).

Toulmin is missing the point of dialectical materialism, as the dialectic does not imply "progress" in the sense implied here. The movement of the dialectic is certainly processual: yet process is not always "progress." As Marx pointed out in what is probably his most widely read pamphlet, *The Communist Manifesto*:

The history of all hitherto existing society is the history of class struggles. Freeman and slave, patrician and plebeian, lord and serf, guildmaster and journeyman, in a word, oppressor and oppressed, stood in constant opposition to one another, carried on an uninterrupted, now hidden, now open fight, a fight that each time ended, either in a revolutionary re-constitution of society at large, or *in the common ruin of the contending classes* (Section 1, pp. 1 and 2; emphasis added).

Marx is simply optimistic in his analysis of history and society, elucidating what he sees as the potential for the development of socialism out of capitalism. Nowhere in his scholarly works, however, is the "progress" or potential for socialism described as inevitable; the spectre of "common ruin" haunts human history well into any foreseeable future. What Toulmin sees as a teleological and unilineal concept of evolution, then, is but a *plan of action* for the development of man's potential; "progress" depends on man's ability to constitute reality on the basis of a vision, not of "inevitable progress" either in the history of classes or the transformation of paradigms. In both cases, of course, the transformation takes place within a concrete and real historical situation which limits and describes the possibilities of change.

Laing and Cooper (1971:99) in their synopsis of Sartre's *Critique of Dialectical Reason*, point out that while a dialectic of nature is possible, it is not necessary to the discussion of human history. Thus while a scientific

paradigm may itself be dialectical or not, as an aspect of the human construction of reality, it takes place within the material dialectic of human history. The project of history, that is, man's discovery of his natural condition, of his subjection to those conditions, and of *a praxis* to overcome those conditions, has resulted in the scientific project. And this project requires a science and technology in service to man's intentionality, not an independently evolving "reified" institution in which movement is confused with progress, and inertia with movement. For Toulmin, every novelty selected for perpetuation by the "court of reason" is *ipso facto* progressive. He leaves the question of *ends up* to a cosmic force (be it God, Nature, or Evolution), while reserving for man the simpler little decisions like the "adequacy" or "adaptiveness" of a single concept vis-a-vis current problems. Thus he continues in the same vein:

In practice, of course, we can judge the "rational adequacy" of socio-political changes to the needs of particular problem-situations only with an eye to specific cases, as they occur; ideological principles of a totally general or cosmic kind give us no precise criteria of judgment. Whether in physical science or industrial organization, it is the particular changes by which men seek to deal with their current problems that are more or less "rational," not some overall teleology or "direction" implicit throughout history-as-a-whole (1972:329).

Nowhere is Toulmin's relativism and acceptance of the status quo more obvious than in this selection, and nowhere is his conservative ideology more apparent. In this viewpoint, shared by many functionalists, behaviorists, systems theorists, and social evolutionists, paradigm changes and the development of disciplines, science and technology, and their accompanying ideologies *are the product of incremental decisions* made "rationally" to deal with current problems. This position begs the question raised by Collingwood and Kuhn: How is the recognition of "a problem" or of "problem solutions" possible without general ideological principles or absolute presuppositions? To argue this position is not to claim an "overall teleology" throughout "history-as-a-whole"; it is to state simply that both man's dreams and his theories, as well as the world which he creates out of them, are more than a response to current ecological demands. After all, if an act can be defined as "rational" because it is a correct response to a given stimulus, then planaria have as much claim to rationality as homo sapiens.

'Ecological,' 'Salesmanship' Metaphors May Conflict

While Toulmin aims to provide *a populational* account of the evolution of concepts, he is not above mixing his metaphors. The "ecological metaphor," borrowed from biology, provides one description of the selection; however, this is confused by the simultaneous use of another metaphor, borrowed from the marketplace, of "salesmanship." This analogy between scientific change and market phenomena is equally troublesome in that it is both inconsistent with the "ecological metaphor" and because it fails to account for elegance and parsimony in scientific explanations. As Toulmin puts it, in any problem situation, the disciplinary selection process picks out for "accreditation" those of the "competing" novelties which best meet the specific "demands" of the local "intellectual environment" (1972:14). (Later Toulmin uses such terms as "competition," "merits," and "demands" to describe how an idea succeeds.) Toulmin appears to elaborate the notion that the existence of a discipline and its collateral disciplinary societies, professions and professional societies, does not depend on intellectual considerations alone, but on what society in general is willing to approve as "ethical" or "respectable":

To cite an extreme case: one could, in theory, consider the development of chemical and bacteriological weapons as a legitimate, self-contained technology, entitled to its own disciplinary ambitions and professional organizations. In the abstract, then, we might have to concede the same professional status to an Institution of Toxological Engineers as are granted to the existing Institutions of Civil, Mechanical, and Electrical Engineers. (By the same token, the Royal College of Nursing could—in point of abstract theory—be paralleled by an equally professional Royal College of Prostitution.) If our minds rebel at such a suggestion, this only confirms that, in conceding an intellectual or practical enterprise disciplinary standing, we take its positive value for granted. Mechanical engineering or nursing are accepted as legitimate concerns of a discipline or profession, only because the goals of better nursing, or better mechanical engineering are themselves accepted without dispute; an ethical repugnance to nerve gases and prostitution alone disqualifies them from the same status. So the existence of an autonomous "discipline," and of its associated "profession," depends both on a preliminary

intellectual abstraction—viz., our discovery that the goals in question can be effectively pursued in isolation from other goals—and also on our approval of those particular goals (Toulmin, 1972: 405).

For Toulmin, then, the "search for truth" and the creation and actions of discipline societies doing the search depends upon "selling the truth" to the members of the disciplines (1972:140) (no matter how abstract or theoretical the quest), *and also depends* on selling the notion that the sort of quest is allowable (ethical, respectable) to society at large.

This notion of "salesmanship" does not sufficiently specify the canons according to which "new paradigms" are judged, on the one hand. On the other hand, it expresses too benign a notion of what society will tolerate, in the way of "professional" societies. There *are* groups which have all the characteristics of a discipline which Toulmin describes (save open publication of results) which are publicly supported, criticize one another's findings, are organized into paradigms, etc., but which do not possess universally approved goals. Military research groups are somewhat of this character; the "atomic scientists" groups originally came into existence largely to develop the atomic bomb. The "Old Crows" group, a professional society, is concerned only with electronic warfare. The existence of such professional groups does not indicate either widespread or elite approval (consider, for example, popular opinions of lawyers and the Newman Committee recommendations on the American Bar Association). Finally, the discipline society itself may engage in activities sufficiently questionable to evoke social rage (for example, the recommendation that "discipline" societies that endeavor to accredit "departments" be subject to anti-trust suits). What is important is that Toulmin recognized that the creation and dissemination of paradigms and the existence of disciplines is *a political* decision.

It may be possible to show that science is the product of a certain mode of production in much the same way that a legal system is, and that it is directed toward the productive ends of that mode; if anything, it is as much constitutive of that reality as it is an "unraveling of the laws of the universe." Thus, the analogies which Toulmin draws between concept-development and "salesmanship" are appropriate, but "salesmanship" contains no assurance that the transaction will be governed by ethical considerations, or, that what discipline societies are allowed to exist will not be the

consequence of the same sorts of elite decisions as are other decisions in a politically hierarchical, elitist society.

How Can Education Relate to Political Decisions?

If decisions about what is to be studied and how they are to be studied are political decisions, how can education or the process of science be transformed so as to serve a less hierarchical, more classless and "open" notion of the "social"?

Man creates his own intellectual and material environments, both in Ronchi's research and in the sense that he creates his culture through the way he sees and uses the realm of "material necessity"; for this reason concepts judged as adaptive only to the current ecological demands of a particular environmental niche are ahistorical, or in Marcuse's terminology, "one-dimensional." It is precisely the current "ecological demands" which Toulmin apotheosizes. Mankind's transcendent rationality holds the potential for disregarding particular conceptual variants which are "adaptive" to a particular reality, but maladaptive to a longer view of human history. The development of the wisdom necessary to reject "bad" concepts which "work" is a necessary part of the development of *Human Understanding*; unfortunately, this dimension is missing from Toulmin's account.

Decisions like these are not only scientific-rational decisions, but are also political-rational decisions: the recent decision in America not to invest social labor in the "feasible" supersonic jet transport is an example of this type of wisdom on a small scale. However, aside from this type of technological decision, Western society defines scientific "progress" as valuable for its own sake, without really evaluating its human consequences. Since we do not see science as reflecting class interests, but instead as a "universal" achievement, we do not evaluate the social consequences now seen as inevitable.

In Kuhn's example, the political process is equated with recourse to an institutional matrix, and is seen as the process of "normal science" and rationality. Change made outside this institutional matrix is viewed as "irrational" since it appeals to "techniques of mass persuasion and force." Toulmin agrees with this analysis, but disapproves of it. He suggests that the proper institution to consider in the history of science is the "discipline," not

the "paradigm," and disciplines are not subject to discontinuous change, thus denying the need for a term like "revolutionary." I offer the example of Vasco Ronchi to show the failure of this disciplinary account. Similarly, Kuhn's political analogy must also be subject to criticism. Decisions made with recourse to mass persuasion and force are *political* in nature; they are not irrational (except perhaps to those adherents to the old paradigm who feel threatened), and they are not unintelligible or discontinuous:

Revolutions in society are of the same character as the violent changes in nature. They do not suddenly "fall from the sky." They are prepared by the entire preceding course of development, as the boiling of water is prepared by the preceding process of heating or the explosion of a steam-boiler is prepared by the increasing pressure of the steam against its walls . . . in society, as in nature, evolution (gradual development) leads to revolution (sudden change): "The violent changes presuppose a preceding evolution, and the gradual changes lead to violent changes. These are two necessary factors in a single process" (Bukharin, 1969: 82, quoting Plekhanov).

To explore our analogies: if evolution and revolution are seen as different aspects of the same process, what is there about an occurrence which would lead one to label it one or the other? For example, presumably a change occurred on the genetic level at a point in history which allowed a previously aquatic creature to survive unsubmerged. This occurrence, however, was not simple and probably never really occurred. Actually, a succession of beings within a species probably began to be able to endure longer and longer periods out of the water, and their ability to tolerate drier and drier climates increased. In other words, the change from creatures able to live only in water to creatures that could spend more and more time on land was *evolutionary*. However, this does not mean that the change from aquatic to terrestrial life was not revolutionary, or that the term "revolutionary" is a descriptive term devoid of explanatory power. The event is described by humans as "revolutionary" because that term explains a small change with monumental consequences. This same term, then, can be applied to many other evolutionary occurrences: the change from stone age to bronze age, from nomadic culture to agriculture, from capitalism to communism, or from Ptolemy to Copernicus. In each case, the change can be literally described as evolutionary; yet in historical perspective, in its consequences for mankind,

in the very relationship between that change and the present world—the change was *revolutionary*.

In my view it is not the precipitous nature of the change which marks it as revolutionary, still less is it any underlying irrationality, incomprehensibility, or illegality; it is the real historical and human consequences which lead people to define a specific occurrence as "revolutionary." Now, the evaluation of a particular conceptual change must be done from 'a historic vantage point which makes "on the spot" analyses uncertain at best. Thus to describe the invention of, e.g., television as revolutionary is premature, and subject not only to discussion but also to the historic process itself. The real question to be dealt with is the degree to which a particular conceptual change, invention, or genetic alteration redirected a previous trend or direction of evolution. Simple biological variations, being selected on the basis of adaptation to existing circumstances, can be judged as revolutionary if, for instance, they open up a new environmental niche. In this process the biological variants, phenotypes, create a world qualitatively different from the previous one. Human conceptual changes, likewise, can be judged as revolutionary, not merely because they are better "adaptive" to the established reality, but because they constitute a reality hitherto unimaginable.

Thus, the notion of a sun-centered solar system opened up a previously unrealized conception of the nature of the physical world (with enormous ramifications in the human social world), a process analogous to the exploitation by a species of a new environmental niche.

This account still leaves open the manner in which people, at the time, can choose between conceptual variants and still be making a "rational" choice. "Rational," in this context, means conscious, not infallible. An account of the decision-making process must take into account the totality of possible arguments for and against the adoption of particular new concepts: a process which by definition cannot be relegated to an elite "discipline" or self-appointed experts, or indeed to any *single* form of "rationality" or "clear-headedness." In this respect, theological, mystical, political, cultural and psychological arguments must enter the process on the same level with empirical or positivistic criteria. The ensuing debate, then, will take place in the transcendent dimension of *potential* human consequences. Only by this political process which appeals to "mass persuasion" will science be unpacked of its reified and mystifying aspects, so as to be recognizable as a

variety of production with its own ideology; only then will science "serve the people" instead of existing as an external and alien force pulling us into a future of endless novelty and shock, and forcing man to alter his social existence to keep up with its technological demands. The alienated science of class society has not proceeded "rationally," i.e., in the light of all relevant knowledge; it has proceeded irrationally by limiting the admissible types of knowledge to a single dimension of what is "rational." Toulmin, in denying the possibility of a conceptual or social revolution, denies the possibility of uniting, in a praxis, the two dimensions of wisdom: the dimension of "what is" (implied in the title *Human Understanding*), and the dimension of "what is to be."

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