SYSTEMIC KNOWLEDGE:

Toward an Integrated Theory of Science[1]

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> It has been put to me that one should in fact distinguish carefully between Science as a body of knowledge, Science as what scientists do and Science as a social institution. This is precisely the sort of distinction that one must *not* make . . . By assigning the intellectual aspects of Science to the professional philosophers we make of it an arid exercise in logic; by allowing the psychologists to take possession of the personal dimension we overemphasize the mysteries of "creativity" at the expense of rationality and the critical power of well-ordered argument; if the social aspects are handed over to the sociologists, we get a description of research as an N-person game, with prestige points for stakes and priority claims as trumps. *The problem has been to discover a unifying principle for Science in all its aspects* [emphasis added] . . . Before one can distinguish separately the philosophical, psychological, or sociological dimension of Science, one must somehow have succeeded in characterizing it as a whole.
>
> John Ziman[2]

1. Introduction

In recent years, there has been a growing criticism of the separation between the philosophy and the sociology of science.[3] In brief, the criticism is that as comforting as the separation has been to both philosophers and sociologists alike in that it relieved each of them of the burden of having to understand and to account for each other's knowledge and concerns, one cannot properly study (and hence ultimately understand) the social-institutional structure of science independently of its cognitive-intellectual structure and vice versa. As among those who have contributed to this criticism, we are obviously in agreement with this line of argument, indeed so much so that we seek to broaden it in this paper.

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The argument of this paper is that the entire field of science studies is in need of revolution and revision, not just the philosophy and the sociology of science. In a word, this paper is critical of *all* the various distinctions and divisions which have served to separate the history, philosophy, psychology, and sociology of science from one another. This paper argues that anything less than a "genuine" systems approach will fail to capture and to do justice to the phenomenon of science.[4]

The procedure of the paper is as follows: We present a brief survey of some of the major concerns, variables, and distinctions which have served (a) to characterize the history, philosophy, psychology, and sociology of science and (b) to divide them from one another. We show by means of reference to recent empirical studies and theoretical criticisms that the separate variables and concerns actually presuppose and depend on one another. If the subject matters of the various fields which pretend to study science are opposed to one another, the nature of the opposition is more of the character of a dialectic than of anything else. That is, the nature of the variables of one field (p) take on their meaning as much as by what they exclude and are opposed to (not-p) than by what they are in harmony with and thus include (i.e., p).[5]

We also present a systems model for the study, understanding, and portrayal of science. The model which is presented in the form of a schematic diagram attempts to incorporate as many of the variables which are relevant to science as possible. The model includes two of the most important sub-systems of science as sub-models (components): (a) a model of the epistemic structure of science, and (b) a model of the social, political, and organizational structure of science. The overall model attempts to show that these two sub-models continually interact and presuppose one another.

The outcome of the effort is a map and an evaluation of what has been studied as well as what has *not* been studied in the field of science studies. It is also a reappraisal of the various notions of rationality which enter into the component parts and the total system of science. The standards of rationality which are applicable to the parts are not necessarily applicable to the whole and vice versa. The eventual goal of the effort (currently beyond the scope of this investigation) is the construction of a working computer simulation model of the whole of the scientific enterprise so that the effect of various theoretic decision rules and models could be investigated for their effect upon the parts and the whole of science.

2. A Selective Map of Science Studies

TABLE 1: Major fields of science studies classified by investigators, major substantive ideas and/or critical distinctions introduced

Philosophy of Science

Reichenbach: introduction of critical disjunction between contexts of discovery and testing.[6]

Feigl: elaboration of the orthodox or "received" view of scientific theories (primitive terms, uninterpreted observational base, formal theoretical language, deduction of consequences); disjunction between theoretical and observational entities.[7]

Hempel: studies in the logic of explanation; covering-law model of scientific theories; deductive character of scientific explanation.[8]

Nagel: logical character of scientific laws.[9]

Popper: emphasis on falsification as the distinctive character of scientific knowledge; strong demarcation between the social-psychology and logic of science on basis of emphasis on distinction between contexts of discovery and testing; strong assertion of superiority of logic of science over social psychology, emphasis on asymmetry between verification and falsification; critique of psychologism and sociologism.[10]

Scheffler: strong critique of Kuhn; re-emphasis on non-relativistic, neutral character of scientific data as impartial arbiters of theories.[11]

Caws: discovery is no less "logical" in character than testing.[12]

Polyani: the personal character of scientific knowledge, the tacit dimension.[13]

History of Science

Duhem: emphasis on inconclusive nature of scientific experiments; impossibility of a crucial falsifying experiment in science.[14]

Hanson: emphasis on virtual inseparability of all observations and theory; discovery is patterned and theory-laden.[15]

Kuhn: Normal and Revolutionary science; scientific paradigms; interaction between theory and data; emphasis on social-historical character of science; bad side-effects of the pedagogy of science (the textbook).[16]

Feyerabend: severe critique of the logical, rule-patterned accounts of science as a "rational" process; emphasis on the irrational, anarchistic, subjective, idiosyncratic features; emphasis on theory-ladenness of all observation; need for incommensurable theories.[17]

Holton: role of conflicting themata in science; interaction between "public" and "private" science; tolerance of ambiguity and conflicting themata as a prime characteristic of the great scientists; case studies of Einstein.[18]

Westfall: case studies of Newton's "fudge factoring" subjective behavior.[19]

Price: growth of scientific journals, societies; demographic studies of science.[20]

King: historical critique of the Mertonian norms of science.[21]

Ravetz: the social problems and context of scientific knowledge.[22]

Lakatos: critique of Popperian naive falsificationism; theories have to be protected from falsificationism: theories have to be protected from premature testing.[23]

Toulmin: critique of "logicism"; critique of externalist-internalist distinction.[24]

Sociology of Science

Merton: studies of 17th century science, societies; formulation of the norms of science; analysis of priority races; the ambivalence of scientists; sociology of knowledge.[25]

Zuckerman: reward system of science; study of Nobel prize winners.[26]

Crane: study of the gate-keeping function in science; invisible colleges.[27]

Hagstrom: social structure of different fields of science.[28]

Cole: citation structure.[29]

Mullins: structure of different scientific specialties; theory groups in sociology.[30]

Barnes & Dolby: critique of Mertonian norms of science.[31]

Mulkay: critique of Mertonian norms; scientific theories function as norms.[32]

Lodahl & Gordon: structure of different scientific fields; levels of paradigmatic development.[33]

Psychology of Science

Roe: psychological portraits of scientists in different disciplines via traditional clinical instruments (projective) background data, origins of, childhood interests, religious interests.[34]

Eiduson: in-depth interviews with scientists, family backgrounds.[35]

Kubie: psychoanalytic analysis of the problems of the scientific career role.[36]

McClelland: in-depth summary of work of Roe; studies of the imagery of scientists.[37]

Hudson: studies of differences between arts and science students, diverger/converger distinction; spouses of scientists.[38]

Maslow: analysis of the conflicting psychological elements inherent in the scientific method, rigidity, compulsiveness–fear of the unstructured and unknown vs. openness–playfulness; ability to suspend structure and welcome the unknown.[39]

Mitroff: psychological analysis of different scientific roles; projective images of physical objects; extreme aggressiveness of science; spouses of scientists; characterization of working epistemologies and norms-in-use.[40]

Garvey: structure of the communication system of science.[41]

Simon: analyses of science as a complex problem-solving activity; justification for the heuristic character of a "logic" of discovery.[42]

Table 1 presents a selective map of what are, in the opinion of the authors, *some* of the major contemporary works in the philosophy, history, sociology and psychology of science, respectively. By preparing such a table, we certainly do not mean to imply that it is exhaustive in any sense. We also do not mean to imply that every author fits neatly under a single disciplinary label. Indeed, there are a number of important thinkers whose work cuts across more than one category. In placing a person in a particular category, the major criterion we used was the intellectual tradition or discipline out of which the work seemed to us to emanate.

While the table is thus not meant to be exhaustive, we do believe that it is representative of the character of the work taking place (as well as having taken place) under each tradition. We do not believe that it is an artifact of the table (i.e., the classification) that certain conclusions can be drawn from it. For example, with the primary, if not almost sole, exception of some like Polanyi, the tradition represented by the category "philosophy of science" is almost solely identified with the logic of science. That is, the primary emphases are on: (a) the logical character and representation of scientific theories; (b) a rigid distinction and barrier between the supposedly soft and unorderly processes of the discovery of a theory and the supposedly hard and orderly processes of testing; (c) a clear and unambiguous separation between observational and theoretical entities—the former (observations) supposedly being unproblematic because of their "public" character, the latter (theories) being problematic because of their hypothetical and inferential character; and (d) not only a clear and rigid line of demarcation between the areas of application of the social psychology of science (to discovery) and the logic of science (to testing) but also the assertion, if not the smug assumption, of the general superiority of the logic of science with an accompanying "put-down" and demeaning of the status of the social psychology of science.[43]

Everything that the philosophy (logic) of science has asserted as characteristic of science, the tradition represented by the history of science has either denied or contradicted by asserting a counter characteristic. Little wonder why the tension between these two ways of viewing science has been so great. For instance, if the logic of science has placed great stress on the logical character of science, then the history of science has been able to show (and seemed to delight in doing so) the alogical and illogical character of actual scientific practice. It does little good to say that the logic of science is concerned with studying the characteristics of science as an ideal system of knowledge whereas the history of science has been concerned with studying science as it is. The rejoinder to this has been that our formulations for an ideal method must bear *some* relation to what is humanly attainable or else we may not just be pursuing an ideal world, but a fantasy world at that.[44]

In a similar vein, historians have been critical of nearly all the other distinctions that logicians have presupposed. Thus, when one examines actual practice there is no clear line of division between the discovery and testing phases of scientific inquiry. There is a continual crossing over and interaction between the two. Further, powerful arguments have been advancd as to whether, even ideally, there should be a clear distinction between the two.[45] By the same token, the lack of a clear separation between observational and theoretical entities has also been stressed.[46] In fact, just the reverse has been emphasized, i.e. that observations are decidedly not neutral. If anything, they are theory-laden. What this means is that not only can we not collect scientific observations without presupposing some theory (not necessarily unique) with regard to the phenomenon under study, but that the presumption of different theories is likely to give rise to different observations.

In terms of the few issues we've been discussing, if historians lack consensus, it is with regard to the status of the sociology and psychology of science. Kuhn, for one, is not only sympathetic to both psychology and sociology but has relied heavily upon them in his attempt to fashion a theory of science. Likewise, Feyerabend and Hanson have heavily utilized psychology in their own approaches. Toulmin, on the other hand, evidences a mixed position. He argues that science cannot be understood in terms of a set of fixed rational rules but must instead be understood in terms of cultural processes that are akin to biological (evolutionary) processes. That is, while espousing a cultural point of view, Toulmin adopts a biological explanatory base, not a sociological or psychological one.

If the relationship between the logic and the history of science has been one of a dialectical opposition, i.e., both positions depend on one another for what they affirm as well as deny, then the relationship of the psychology of science to all of the traditions represented in Table 1 has been one of relative independence. With the notable exception of Herbert Simon's essay on the "heuristic lawlike" character of discovery (and not just testing), the psychology of science has stood relatively apart from the issues that have occupied logicians and historians of science—the character of scientific knowledge. The emphasis instead has largely been on the character (the personalities, the family origins, religious backgrounds) of scientists. While it is not completely accurate to say that the psychology of science has studied scientists as it would any other interest group, it is not entirely off the mark either. Again, with the relative exception of the work of Maslow, Mitroff and Simon, there has been little study from a psychological point of view of science as a special kind of subject matter. This may in large part be due to the fact that as a formal field of study the psychology of science is the least developed and institutionalized of all the research traditions listed in Table 1.

TABLE 2: A matrix of some possible studies of science formed by pairwise interaction effects of disciplinary traditions

	PHILOSOPHY	HISTORY
PHILOSOPHY		PHILOSOPHICAL-HISTORY OF SCIENCE Is it possible to build a model of science that would satisfy both the philosopher and the historian of science? What would a dialectical treatment of scientific history look like?
HISTORY	HISTORICO-PHILOSOPHY OF SCIENCE What would a logical reconstruction of science look like that was grounded in the realities of historical practice? Is a logical account possible that incorporates the irrationality and nonrationality of science, as well as changing concepts of rationality? Is it necessary to presuppose timeless standards of rationality?	
PSYCHOLOGY	PSYCHOLOGY OF SCIENTIFIC KNOWLEDGE: Do different psychological types have different concepts of rationality, objectivity, logics of discovery and testing, methodologies of science; of science itself? Is a psycho-logic of science possible? Do all types equally accept the distinctions between discovery and testing, observations and theory? Can we build a simulation model of the inner workings of science? What is the role of different types in scientific knowledge? Is a healthy science possible? Does the growth of science necessitate neuroticism?	PSYCHO-HISTORY OF SCIENCE: What would a definition of a paradigm be that did not presuppose consensus? In what sense are the physical sciences in a pre-paradigmatic stage? Can we use clinical methods to form a psychological portrait of scientists from historical documents? Can we study the psychological forces behind the origin and growth of modern science? How does psychology help us in understanding the historical context of science? Is the historical context a response to the personalities of scientists or vice versa?
SOCIOLOGY	SOCIOLOGY OF SCIENTIFIC KNOWLEDGE: Do different social groups of scientists have different concepts of rationality, objectivity, etc.? Do groups differ in the amount of protection time they are willing to give to a new theory before subjecting it to crucial tests? What are the functional/dysfunctional consequences of having different group concepts of science? Are groups more severe in testing the claims of their competitors than those of their friends?	SOCIO-HISTORY OF SCIENCE: Can historical documents be read as social surveys which give us a sociological analysis of the times? To what extent can sociometric techniques be used to understand the group structure of scientific societies? Can the attitudes of elite versus nonelites be discerned from past records? Can the origin of paradigms be inferred by social techniques?

PSYCHOLOGY	SOCIOLOGY	
PHILOSOPHICAL-PSYCHOLOGY OF SCIENCE What is the evidential status for the existences of different types of scientists? How does the existence of different types bear on the creation and validation of scientific knowledge? What would an appropriate logic be for adjudicating the conflicting claims of different scientists?	PHILOSOPHICAL-SOCIOLOGY OF SCIENCE Are the norms of science necessary, sufficient? How do social norms bear on scientific knowledge? Is there a disjunction between social norms and scientific theories? Can theories function as norms? Are norms organized into opposing dialectical sets? What would an appropriate logic be for adjudicating conflicting norms? Should one be more severe in testing one's opponents?	PHILOSOPHY
HISTORICO-PSYCHOLOGY OF SCIENCE Does history suggest a richer and broader set of labels and concepts for classifying different types of scientists? How does the historical context help us in understanding individual scientific personalities? Is there a constancy of different types through different periods? Is personality a response to the period, or vice versa?	HISTORICO-SOCIOLOGY OF SCIENCE Are norms invariant? Do they change over time? Has the interaction between science and the larger society always been the same? Can we use history to give us better categories for contemporary surveys of scientists?	HISTORY
	PSYCHO-SOCIOLOGY OF SCIENCE Do different types have different norms? Does a particular psychology stand behind different norms? Do different types exhibit a differential degree of migration into and out of various disciplines? Is there a particular psychological orientation that is similar to elite scientists? Are there personality differences between fields?	PSYCHOLOGY
SOCIAL-PSYCHOLOGY OF SCIENCE To what extent do institutional forces shape different personality types? To what extent do the norms of science shape types? What would an appropriate set of projective tests look like for getting at the unconscious institutional features of science?		SOCIOLOGY

The relationship between the history and the sociology of science may be more asymmetrical in its character than anything else. That is, the history of science seems more willing to adopt a sociological perspective in its approach to historical analysis than does the sociology of science seem willing to borrow concepts from history. This is certainly reflected in the contemporary sociology of science with its primary emphasis upon: (a) science as a social institution governed by the same norms which supposedly govern all social systems and for all times and settings[47]; (b) the reward and allocation system of science which favors some (elite) scientists more than others; and (c) the social structure of different sub-groups of scientists. The primary emphasis, in other words, is on the social structure of science independently of how the social structure impacts on the generation and assessment of the very thing for which the structure supposedly exists in the first place, i.e., the generation of scientific knowledge. With the almost sole exception of persons like Mulkay, there has been little mention, and even less empirical study, of how the substantive subject matter of science can give rise to the social norms of science, and vice versa.

As much as Table 1 is significant for what it shows–namely what aspects of science have been studied by which of the major traditions–it is even more significant for what it does *not* show, i.e., what has *not* been studied in depth. In the language of the analysis of variance, each of the major traditions represented in Table 1 have tended to study main effects; what is lacking is an equally in-depth and comprehensive study of interaction effects.

An example of what a study of some possible interaction effects and questions might look like is given in Table 2. Table 2 is an attempt on the part of the authors to portray two things: (1) what each field potentially has to contribute to the others by making possible the study of a unique interaction effect, and (2) the fact that the traditional approaches to science studies have largely confined themselves to a study of the diagonal cells in Table 2. What we have identified as the history of science tradition is best represented by the intersection of the row and column labeled history. Table 1 contains the diagonal cells of Table 2. Table 1 thereby shows what a small proportion of possible studies it represents.

Following Lazarsfeld, we adopt the following convention for reading the table.[48] We consider each discipline a potential donor as well as recipient of the substantive knowledge, concerns, and methods of each of the others. Thus, every discipline is considered both as a potential donor and potential recipient. It should be recognized that Table 2 only considers pairwise or two-by-two interaction effects. It does not consider the fields taken three at a

time in order to get at the four possibly three-way interaction effects. Nor does it consider all of the fields taken at once in order to get at the single four-way interaction effect. Reflection on the nature of the four-way terms shows why science is such a difficult phenomenon to understand. As the Ziman quote to the introduction of this paper attests, the understanding of science as a total system demands that we understand how the historical philosophical, psychological, and sociological elements of science all exist as well as act in simultaneous conjunction with one another, not in isolation.

In order to avoid possible misunderstandings it should be emphasized that we are not saying that there has been no study at all of the two-way effects. For instance, whatever history finally makes of Frank Manuel's psychoanalytic portrait of Newton,[49] it stands as an excellent example of the psycho-history approach. Likewise, the cells labeled "historico-philosophy" and "philosophical-history" represent the kinds of questions being confronted by the newly emerging discipline of the History and Philosophy of Science. In fact, interestingly enough, the tradition we have labeled history of science in Table 1 exhibits, upon closer inspection, the boldest excursion into the region of interaction effects. Thus, the work of Thomas Kuhn represents the prime example of the importation of sociological analysis and reasoning into the field of history. The work of Norwood Russell Hanson likewise stands out as a premier example of the sophisticated use of psychological concepts of observation transported into history.[50]

To summarize, what we are contending is that the interaction terms (questions) have not been as consistently and self-consciously studied as the main effect questions. We see, for example, little current demand for such hybrid disciplines as "the psychology of scientific knowledge." To put the point in its strongest terms, the larger the number of interactions, the less systematic study it has received. Thus, for example, the number of simultaneously combined studies in the philosophy, psychology, and sociology of science is virtually nil.[51]

We turn now to an elaboration of two sub-models of a larger model of science. The sub-models will not only allow us to tie together some of the preceding points, but also allow us to demonstrate in a more effective manner some of the higher order interaction effects that were merely alluded to above.

3. Toward a Whole Systems Model of Science

In previous papers, we have discussed some of the properties of a deceptively simple sub-model of the epistemic or problem solving structure of science.[52] The present discussion of the model differs from previous ones in that the emphasis is on higher-order interaction effects.

Figure 1 portrays the main qualitative features of the model. For the moment, we suppose that every scientific inquiry "starts" with the extreme left-hand circle, the "felt existence" or recognition of a Problem Situation—what a naive realist would be inclined to call Reality. From the point of view of systems thinking, there *are no* simple starting or ending points to the process of inquiry. One can begin as well as end the process at any point in the model. Indeed, where one "starts" and "ends" is a complicated function of the paradigmatic development of one's field of science (history), the social organization of the discipline (sociology), one's preferred methodology (philosophy), and finally one's personality type (psychology). The point is that even so seemingly simple a feature as where one starts and ends an inquiry is able to give a complicated four-way interaction effect. This feature is not only a potential candidate for explication by all four research traditions but it is doubtful whether it could be entirely explained by any of the four major

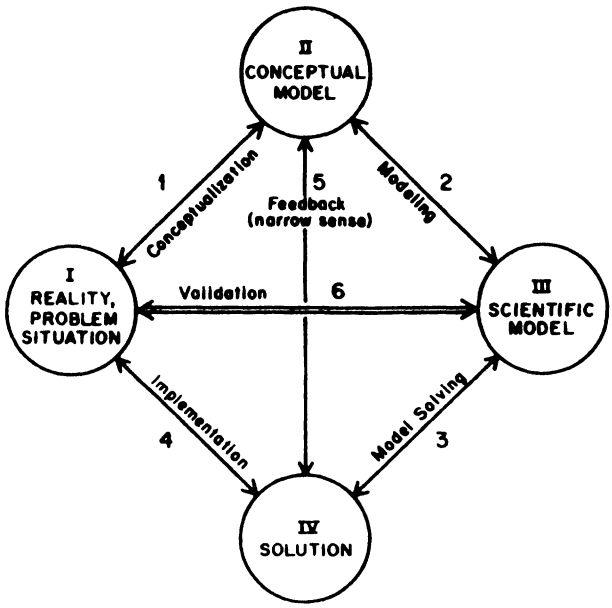

Figure 1. A Systems View of Problem-Solving

fields of science studies acting in isolation.

The path from the circle labeled Problem Situation to that labeled Conceptual Model is meant to indicate that if one enters the system at the level of the Problem Situation then the (relative) "first phase" of problem solving consists in formulating a Conceptual Model of the problem. The Conceptual Model defines the problem in the most basic and broadest of possible terms. For example, the Conceptual Model specifies whether the problem is one of physics, chemistry, economics, psychology, etc. If, for example, the problem is one of physical mechanics, then the question is whether it is to be considered a problem in classical mechanics or relativistic mechanics. The Conceptual Model, in other words, corresponds to the choice of a "paradigm" in the most metaphysical meaning of the term. The choice of a Conceptual Model is akin to the choice of a world-view. It is indicative of a deep commitment to view reality as structured in a particular way. Since different fields and traditions rarely share the same basic Conceptual Models, the disagreement at this level can be especially severe. Indeed, it is this level that we believe Kuhn and Hanson had in mind when they argued that the proponents of radically different traditions literally "saw different realities."

Once a Conceptual Model has been chosen by whatever process, consciously or unconsciously, a Scientific or Formal Model can be formed and a Solution derived from it. If the Scientific Model is a mathematical one, then the Solution, if one is possible, will be a formally derived one. If, on the other hand, the Scientific Model is an empirical one, then the Solution will take the form of an empirically testable hypothesis. If the Solution is then fed back to the initial Problem Situation for purpose of taking action on it (to remove it), we have the situation of Implementation. In other words, Implementation constitutes the actiontaking phase of problem solving.
To complete the model, the path from the circle labeled Reality to Scientific Model corresponds to the philosophical concept of the "degree of correspondence" between "reality" and a representation or model of reality.[53] The vertical path between the Conceptual Model and the Solution corresponds to the degree of correspondence between a given Conceptual Model and the Solution it suggests, i.e., the degree to which a particular Solution corresponds to or follows from a particular Conceptual Model and vice versa. (For reason that are not pertinent to the present discussion, the vertical path has also been labeled "feedback in the narrow sense."[54]

The model helps to clear up a number of fundamental matters regarding the nature of science and its understanding. Each phase of the model potentially involves different social or institutional norms of science, different standards

of rationality and/or measures of performance,[55] different psychological types, linguistic levels of analysis, and the paradigmatic development of a particular science. Consider for example, the matter of linguistics. The Conceptual Modeling phase obviously involves questions of semantics for the concern at this level is with the basic "meaning" of the Problematic Situation and its representation within an appropriate universe of discourse. The Scientific Modeling and Solution phases, on the other hand, properly involve questions and matters of syntactics. The concern of these phases is with the detailed and valid manipulation of concepts within some formalized language. If the semantic question is one of a choice *between* two or more competing languages for Conceptualizing a problem, then the syntactical concern is one of working correctly *within* a particular chosen language structure. Finally, the Implementation phase involves questions of pragmatics, i.e., Does the proposed theoretical Solution work in practice? Does it make a difference?

Notice that these same questions help to elucidate the various and competing standards of rationality operating throughout the model. Syntactical or technical rationality[56] is concerned with questions such as: Does effect x follow *precisely* and impersonally from antecedent conditions y? Semantic rationality, on the other hand, asks whether a particular problem makes sense (is it of interest[57]) to a particular group or audience of *interested,* partisan on-lookers. In the same vein, pragmatic rationality asks how and whether the *theoretical* Solution holds or measures up in the context of *practice.*[58]

Above all the model helps to make clear why the Popper-Reichenbach distinction between the logic and the social psychology of research fails to hold upon closer inspection. Current research is beginning to show that different psychological types are more adept (i.e., better suited in the sense of performance) for some of the phases of the model than for others.[59] The implication is that the *epistemic* or *inquiry* structure functions effectively only because there is an effective *social* allocation function of the different types to the different phases.[60] The system as a whole can only function effectively if the epistemic structure is in tune with the social psychological structure. The epistemic structure of science is dependent upon, and in this sense, a reflection of the social psychological structure and vice versa.

Needless to say, the preceding model does not exhaust all the relevant features of science considered as a total system. The preceding model is merely intended as a *micro* model of the epistemic structure of science. It is not meant to illustrate how the epistemic structure both influences and is influenced by the broader environment in which it exists. For example, it does not illustrate where one of the most important features of science, the

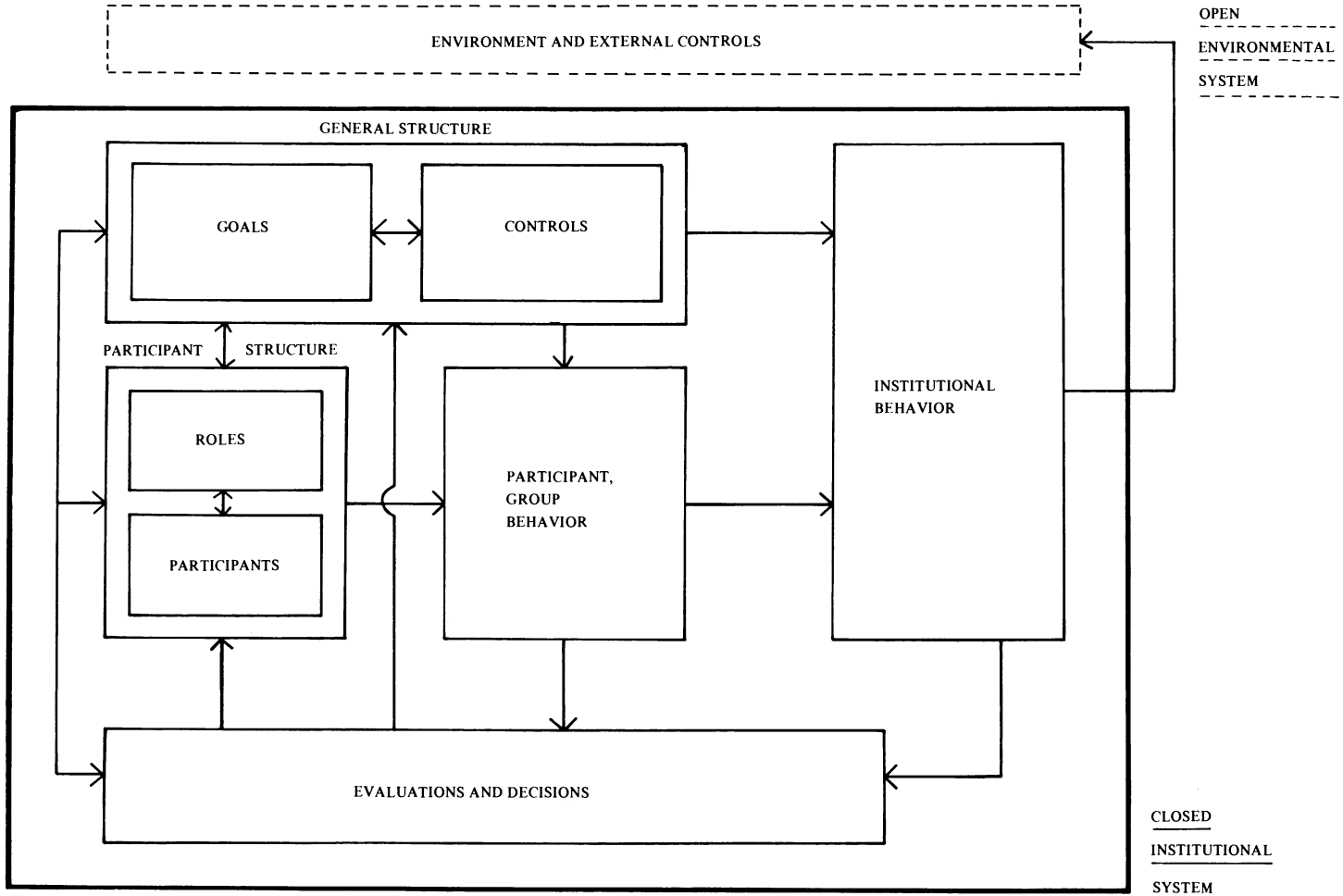

Figure 2: A macro model of science – the analytical model of institutions

publication and review process, enters in. What is needed is a *macro* model which places the micro model in proper perspective.

Figure 2 demonstrates a macro model that derives basically from the field of organizational behavior,[61] but can be equally applied to institutional behavior (i.e., the scientific institution). We label it the Analytical Model of Institutions.[62] It is drawn from the field of organizational behavior for the reason that the epistemic model of science does not function in a political and social vacuum. The broader aspects of science *are* both institutional and political.

The first aspect of the model to be considered is the distinction between an open and a closed organizational (institutional) system. One of the major contributions of management science and organization theory has been the recognition that an organization is highly dependent on its environment for a variety of resources such as information and inputs as well as, ultimately, an outlet for its products or services.[63] Several decades ago most organization theories assumed that the organization was a closed system. Virtually all research concerned the internal functioning of any organization or institution.[64] Such an assumption may not have been seriously in error at that time. The environment of most organizations was fairly stable. There were no major changes in technology, information, and societal needs. In the past few decades, however, the environments of organizations have become even more dynamic and changing than perhaps ever before.[65] The Analytical Model of Institutions takes this into account by drawing a boundary around institutional activity which acknowledges the several points at which the environment needs to be monitored and how the environment influences the functioning of the institution. The actual boundary which defines the institutional system (*vis á vis* its environment) is determined by those concepts (variables) that the organization or institution can control over the short run. Thus, the selection of scientists (participants), the socialization of scientists to norms and procedures as the "scientific method" (roles), the long term and short term goals of science and the mechanism to assure the attainment of these goals (i.e., controls such as reviews for publication and other "reward" systems), can be directly altered by those "inside" the scientific institution. However, several variables can be defined which cannot be immediately, or may never be directly, controlled by the institution and these become classified as environmental concepts. These environmental variables include: the basic economic condition in the nation, the political-legal structure, the availability of certain technologies, and the general culture. At an intermediate level, environmental variables include other institutions (the family, church, funding agencies, industrial organization). Finally, at the micro level of analysis, environmental variables include particular members of society

which may eventually use scientific knowledge. Again, while these environmental variables can be confronted and changed in the "long run" (greater than one year); as, for example, "lobby groups" like the American Association for the Advancement of Science affect political issues, research develops new technologies, education affects culture, the concepts and variables that are generally most attuned to "self-regulation" are those within the institution that are subject to change in the short run.

Tne next component of the model to be discussed is the general structure of the institution which consists of its goals and controls. The goals specify what the institution expects to accomplish: what knowledge or services it provides, which other institutions it seeks to serve, and what general sets of resources it expects will apply to the solution of certain social problems. The controls specify the basic method or design by which the institution will attempt to achieve these goals: the actual disciplinary divisions, selection policies, socialization policies, reward and incentive systems, and other institutional policies to guide decision making on scientific affairs (e.g., ethics). The general structure is thus the blueprint of the institution: what to accomplish and how, even if stated at only the level of broad policy guidelines and specifying only the breakdown of institutional resources into broad design categories (i.e., the basic structure of scientific disciplines).

It should be noted that the environment of the institution has a direct influence on what goals and controls are possible and available for the institution (note the arrow in Figure 2). First, the goals are realistic and functional only if they truly address some problem in the environment and if the institution can command the necessary resources, expertise and design to approach its goals. Second, there are often legal and moral constraints on the institution which preclude the use of certain control mechanisms. In science, for example, there are moral and legal restrictions on the use of human subjects in experiments. The effect of the environment on the institution's general structure becomes perhaps most noticeable when the institution applies its goals and controls to "new" scientific fields (e.g., the social sciences in the twentieth century) in terms of the general structure of traditionally taken-for-granted fields (such as physics) only to find that the latter may not apply directly to the former. The nature of a phenomenon and the "environment" of that phenomenon are different, requiring a different general structure for treating each.

The general structure by itself, however, is not enough to foster effective institutional behavior. For one thing, more specific guidelines need to be developed so that each sub-division and individual in the institution knows

what is expected; for example, who is to interact with whom and how, who is evaluating performance on what criteria, what specific methodologies and recourses are to be utilized, etc. This requires a participant structure for the institution to operationalize the general structure into specific behaviors which are functional for the institution and its subparts (i.e., disciplines). Each participant in the institution is thus given a role. This role can be fairly specific as in the case of a formal scientific method or can be merely a set of expectations regarding the participant's contributions to the institution. The role has been divided into two components: programmed and discretionary. The former contains the specific prescriptions while the second recognizes that not all aspects of an individual's behavior can be determined beforehand, but the institution must allow for some discretion. When the phenomenon under study is fairly complex and changing, the role naturally cannot be heavily programmed; if the phenomenon is well structured, the role can be relatively preprogrammed.

The concept of participants does not simply designate the person. It includes the various personality dispositions of the person that may be relevant to institutional performance. Also included are the skills, values, experience, and needs which can be conceptualized and assessed for each participant. Similarly, the concept of role can include attribution of the task, required skills, assumed values of scientific behavior, and expected patterns of interaction with participants in other institutional roles. An important concept in itself is the degree of fit between the participant and his role, i.e., to what extent the person's skills are congruent with the skill requirement of the task and to what extent the nature of the work is consistent with the individual's need for self expression.

The environment of the institution also has a direct influence on the participant structure. First, the roles in the institution are only functional if they prescribe expected behavior that can actually be performed by participants. However, in some cases participants with the necessary skills may not be available either inside or outside the institution. There may also be certain norms in society which preclude certain tasks and jobs to be defined within role descriptions. Furthermore, there may be legislation which disallows the institution to define a role as being relevant to only a certain class of people (e.g., white male scientists). Thus, even if such matchups between roles and participants may be desirable from an institution performance point of view (e.g., previous selection and training of male scientists), the environment may require the institution to operate within a broader set of objectives–anti-discrimination.

In the Analytical Model of Institutions, the behavior that is actually observed within the institution is referred to as participant and group behavior. This includes people working on tasks, interacting with one another, in essence, "concrete behavior." This concept of participant and group behavior actually represents the closest thing to unconceptualized behavior. However, if one wants to understand what is causing or determining the observed behavior, one has to hypothesize various forces or influences. The model indicates that participant and group behavior is in fact determined by the general structure and the participant structure of the institution (i.e., goals, controls, roles, participants) as well as influences from the environment. The reason that people in the institution are behaving in certain ways is, at one level, based on the general goals of the institution and the control mechanisms set up to pursue these goals (e.g., disciplinary divisions which create particular sub-goals, incentive systems which motivate behavior, academic training programs which socialize participants to certain roles and norms). In addition, each participant has been selected according to some criteria for skills, values, and other characteristics and is given a role which will specifically guide his behavior in the pursuit of more general scientific goals. In other words, the concepts involved in the general structure and participant structure of the institution are those that have been hypothesized as determining or at least influencing behavior in the institution.

A separate category of behavior is labeled institutional behavior. The distinction between this and participant and group behavior is that the latter is observed vis-á-vis the internal functioning of the institution while the former is observed vis-á-vis the environment of the institution. The reason for the distinction is that often there may be a discrepancy between internal and external perceptions of behavior, which may be a manifestation of certain institutional problems. Institutions, for example, often foster a particular myth or certain public image or perception and this may or may not be in line with what the institution is actually providing or the way in which scientific knowledge is actually being developed. In any event, it should be noted (in Figure 2) that an arrow goes from the participant and group behavior box to the institution behavior box (signifiying that one does influence the other) and then an arrow from the latter to the environment of the institution. This arrow signifies that institutions can and do influence their broader environment (as illustrated by the recent and growing concern of ecology and social responsibility) although the effect is not always as direct and powerful as is the environmental influence on the institution. In general, the environment consists of many more resources than the institution and therefore the environment is more powerful, unless several institutions "combine," either implicitly or explicitly, to have a significant effect on the environment.

A central point of any conceptual model is a component that specifically allows management, change agents, problem solvers, etc., to enter in the model in order to assess the states of the model and then to alter those states to solve or manage a problem situation. This property is contained within the box labeled, "Evaluation and Decision Making." The basic inputs to this concept are participant and group behavior, institutional behavior, and the environment of the institution. These concepts become operationalized by both quantitative and qualitative measures (assessments) of behavior or outcomes of internal and external activities. Usually the institution will have a formal measurement of outcome variables (e.g., new scientific developments and findings). In addition, the institution may have some measures of group behavior (e.g., the accomplishments of various disciplines and sub-disciplines). Most institutions also have some formal assessment of environmental variables: economic trends, technological needs, political developments, etc. Furthermore, institutions develop informal qualitative assessments of various processes and outcomes in the institution which cannot always be represented in a quantitative index or the cost of such assessment may be too high to justify. For example, organizations such as the National Science Foundation have been exploring sophisticated information systems (management information systems) to consider systematically what kinds of assessments and information are necessary in order to make various management decisions.

The model shows that the general structure and the participant structure of the institution are the prime determinants of participant, group, and institutional behavior. Consequently, any discrepancy between goals and performance or a lack of discrepancy (i.e., institutional problems) can be conceptually traced to these basic concepts; for the source of problems is rooted in the goals, controls, roles, participants and their interactions within the institution and between the institution and its environment. Furthermore, to solve or manage some Problem Situations (as discussed in the preceding section) thus requires a change in the goals, controls, roles, participants and their interactions.

More specifically, the reason for a discrepancy between goals and performance (however measured) may be that goals were set too high. It may also be that the institution's overall control mechanisms have not been effective (e.g., the design of the disciplines is not conducive to high performance, the reward system does not appropriately guide members) or that the interaction between goals and controls is inconsistent and mutually constraining. For example, scientific goals may emphasize the greater utilization of scientific knowledge, yet the reward systems may foster mostly new theoretical research.

The source of the discrepancies between goals and performances may also be defined vis-á-vis the participants in the institution: they may lack the expertise, training, and motivation to perform effectively. Alternatively, it may be that the roles are ill-defined and do not correctly specify the work that has to be done and what resources should be utilized in performing tasks. The interaction between roles and participants, however, may be the source of the problem. The participants in the institution may have the necessary expertise, roles may be appropriately defined, but the matching between participants and roles may have been ineffective: the wrong people may be working in the wrong roles.

A source of a Problem Situation could also be in the interaction between the general structure and the participant structure of the institution. That is, how well has the institution translated its general, long term plans into its short term activities? The general structure may still reflect the good intentions of the institution but the overall participant structure may no longer be an effective way of operationalizing these intentions. Or the participant structure may be an effective way of performing tasks but the general structure may be out-moded and can no longer provide a useful framework for planning the long term direction for the short term activities.

Finally, the Analytical Model of institutions suggests that the environment itself may be a source of the discrepancy between goals and performance. Particularly, the environment may have changed, thus affecting the possibilities of goal attainment, norms for working, or attitudes which affect the use of scientific knowledge. And while the institution may not be able to directly affect its environment, a change in goals may have the same effect since different segments of the environment become pertinent to the institution as goals are changed.

The foregoing concepts and interactions of concepts (their mutual effects) are all possible sources of the institution's Problem Situation and are therefore likely candidates for the resolutions of problems (either the discrepancy of goals and performance *or* the lack of discrepancy). While it is beyond the scope of this paper to indicate the full range of institutional problems and considerations that the model helps to illuminate, we can at least briefly touch on the following important consideration. In general, it is more costly and difficult to attempt to change any of the qualities of the goals and controls in the general structure than in the participant structure. Changes in goals and controls, because of their centrality to the institution, will require changes in the roles and participants, since the latter operationalize and thus further define the former. However, changes in roles and participants will not

necessarily require any changes in the general structure since the former is more specific while the latter is more general. Changing the core of the institution (general structure) thus sends ripples throughout the rest of the system. An interesting implication of this "depth of change" phenomenon is that it is therefore easier to change the participant structure than the general structure. Basically, the more pervasive (not to mention costly) the change and the number of other variables that are affected by the change, the more institutions tend to resist the change. These resistances develop to protect the status quo and individual status, positions, attitudes, domains and spheres of influence, etc. Thus, the change that can have the greatest effect in the institution (e.g., a change in the general structure of scientific disciplines) is also least likely to be enacted, and the change that has the least effect (or disturbance) on the institution (participant structure) is most apt to be applied (e.g., more stringent selection methods).

Perhaps what the model helps to illuminate most of all is the central theme of this paper–the strong interaction between the so-called separate aspects of science and the separate disciplines which have attempted to study those aspects. Thus, for example, one can approach the goals of science from the perspective of each of the four major traditions of science studies. From the perspective of the philosophy of science, it has been claimed that the sole goal of science is to increase the logical (truth) and empirical content of our theories about the world, i.e., to maximize Scientific Truth.[66] From the standpoint of the history of science, the goal might be to further the evolution of scientific truth and rationality and to broaden what we mean by each of these terms.[67] From the standpoint of the psychology of science, the goal might be to develop non-compulsive, more healthy scientists and a psychologically "healthy" concept of science itself.[68] Finally, from the perspective of the sociology of science, the goal might be to evolve a more rational institutional structure of science that parallels the rationality of the inquiry structure, i.e., the rational allocation and assignment of the most qualified individuals to the social roles to which they are most suited.[69]

A similar set of points could well be made with respect to the so-called norms of science, i.e., that each of the four traditions might define them differently. Thus, from the standpoint of social psychology, there is little doubt that the norms of science are manifested in the concrete and specific roles that scientists assume, and that as a result of occupying these roles, scientists come to internalize the norms of science.[70] Be this as it may, a more interesting and important point follows almost directly from the model itself. As the control mechanisms vary depending on the size, location, and immediacy of the work group that one is a part of, one would expect that the norms of science would

also vary as well. It is not to be expected that one would apply the same social norms of science to one's immediate colleagues as to one's more distant colleagues and competitors.[71] The defect of the traditional Mertonian norms of science is not that they were "wrong" but that they failed to take account of their variability within the institution of science.

Finally, it should also be emphasized that the epistemic model ties into the institutional model at a number of points, depending again on what research tradition we bring to bear. From the standpoint of the sociology and the psychology of science, the epistemic model enters by means of the concrete roles required at each phase of the inquiry structure of science. From the standpoint of the philosophy of science, the epistemic model enters by means of the evaluative criteria necessary to insure adequate performance at each phase of the inquiry process. Finally, from the standpoint of the history of science, the question is: How have the goals, performance criteria, role structure, and control mechanisms as embedded in the epistemic structure of science evolved over time?

4. Concluding Remarks

The central argument of this paper has been that the epistemic structure of science cannot be adequately captured, let alone sufficiently understood, independently of its social, political, and institutional aspects. The argument has also been that for the most part, the major traditions of science studies have tried to isolate and to study the system of science in a reductionistic and piecemeal fashion. This situation will not be corrected by merely developing more new hybrid disciplines, although this too is called for. (Indeed, one can even identify the need for more "pure" traditions such as the anthropology and economics of science, both of which are sadly underdeveloped at best and almost nonexistent at worst.) Neither will this situation be corrected by developing specific and detailed research hypotheses from the models outlined in this paper (although this too is called for). What is called for is a far greater sense of awareness and appreciation of the fact that science is, above all, a holistic phenomenon. Unless one is first aware and appreciative of this, it is unlikely that one will frame the kind of research hypotheses necessary to search for the interaction effects in the first place. It is also unlikely that one will be convinced of their existence by the data uncovered by such research efforts in the second place.

NOTES

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