



Administrative Science as Socially Constructed Truth

Author(s): W. Graham Astley

Source: *Administrative Science Quarterly*, Vol. 30, No. 4 (Dec., 1985), pp. 497-513

Published by: [Johnson Graduate School of Management, Cornell University](#)

Stable URL: <http://www.jstor.org/stable/2392694>

Accessed: 27/09/2011 10:47

Your use of the JSTOR archive indicates your acceptance of the Terms & Conditions of Use, available at

<http://www.jstor.org/page/info/about/policies/terms.jsp>

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact support@jstor.org.



Johnson Graduate School of Management, Cornell University is collaborating with JSTOR to digitize, preserve and extend access to *Administrative Science Quarterly*.

<http://www.jstor.org>

Administrative Science
As Socially Constructed
Truth

W. Graham Astley

This paper argues that the body of knowledge that constitutes administrative science is a socially constructed product. Because empirical observations are inevitably mediated by theoretical preconceptions, our knowledge of organizations is fundamentally shaped by the subjective world views through which we perceive data. Truth is defined in terms of the theoretical constructs and conceptual vocabulary that guide research and mediate access to organizational phenomena. The chief product of research is, consequently, theoretical language, rather than objective data. The knowledge of administrative science is not built from objective truths but is, instead, an artifact — the product of social definition. Institutional mechanisms reinforce these social definitions of truth by investing them with the stamp of scientific authenticity.

In an ambitious proposal for enhancing the state of organization theory, Warriner, Hall, and McKelvey (1981: 173) invited all organizational scholars to participate in formulating "a standard list of operationalized, observable variables for describing organizations." This invitation was designed to overcome problems resulting from the tendency of different investigators to use alternative sets of variables and, consequently, describe organizations of the same type, and even the same organizations, in quite different ways. Thus, Warriner, Hall, and McKelvey (1981: 175) argued that a more accurate representation of organizational phenomena could be achieved by establishing a repository of "pooled data" on operational measures to be used in empirical research.

Underlying this proposal is a conventional model of scientific progress as a cumulative discovery of objective truth. This model, which currently dominates administrative science, assumes that knowledge grows linearly as new data are added to the existing stock of research findings. In this view, the lack of standardized measures and variables for describing empirical reality greatly impedes scientific progress and should be rectified. This paper, in contrast, argues that variety in the language of administrative science is unavoidable, since it reflects deep metatheoretical differences between researchers whose backgrounds, values, and philosophies diverge radically. No theory can simply "describe" empirical reality in neutral linguistic terms; all theoretical perspectives are infused by the biases inhering in particular world views. The search for a standard list of variables is, consequently, based on a misapprehension, since differences in perspective between theoretical approaches cannot be resolved through an appeal to "objective" truth.

In contrast to the conventional model, this paper represents administrative science as a fundamentally subjective enterprise. Because researchers adhere to different world views, they generate a variety of alternative perspectives as they impose different meanings and interpretations upon data. The field's development is characterized not by increasing convergence upon an accepted body of knowledge but by a growing divergence in research perspectives and approaches. Intense competition between rival paradigms prevails as advocates of diverse theoretical positions attempt to persuade others of the

Administrative Science

intrinsic superiority of their favored conceptual vocabulary. Scientific progress, in this view, does not result from the instrumental acquisition of information about objective reality; it is the product of an essentially subjective process in which administrative scientists seek preeminence for their chosen paradigm as an end in itself.

THE SUBJECTIVE CONSTRUCTION OF TRUTH

The body of knowledge that constitutes administrative science is an artifact generated from the a priori constructs of predefined theoretical models. Such constructs do not just describe organizational reality by classifying it into analytical categories, they define its epistemological constitution. Rather than approach organizations as unbiased observers of the facts who passively record events in neutral theoretical descriptions, we already harbor conceptions of what is to be studied; our theories determine what will count as a fact in the first place. "The spectrum of organizational life is filtered through the researcher's preset categories; elements related to the categories are selected, coded as data, and simultaneously given meaning by the categories" (Evered and Louis, 1981: 391). From this perspective, administrative science did not begin with the process of documenting observations. It first required preliminary conceptual distinctions regarding what constituted the phenomenon of organization (Bittner, 1965). As Feyerabend (1975) argued, there are no "bare facts," since the facts that enter our knowledge are already viewed in a certain way and are, therefore, essentially ideational constructs.

As administrative scientists, we formulate knowledge subjectively through biased, selected observations of everyday managerial practice. The world of practice has its own "objective" reality, but since, as scientists, our only recourse to that world is through what we see and do, our knowledge is unavoidably subjective in nature. The "facts" constituting our knowledge are necessarily theory-dependent, since we can perceive nothing except through the knowledge structure in which perception is embedded. Factual truth exists only on this ideational plane. Behavioral observations have no intrinsic truth value; they are blank slates onto which we inscribe factual meaning. Theoretical constructs are connotative as well as denotative; they simultaneously describe and explain. Discovering truth is really a matter of creatively incorporating events into theories to make sense of them. Administrative science, in other words, is essentially an interpretive exercise, a sense-making activity in which truth is defined by the rules of intelligibility embodied in theoretical schemata.

Such considerations led Daft (1983) to describe organizational research as essentially concerned with "storytelling." Organizational research does not just report observations, it tells a story that imputes meaning and significance to those observations. As scientists, we do our job properly only insofar as we are creative in casting phenomena within interpretive frameworks. The interpretive frameworks, not the observations, contribute to our knowledge. Daft went on to argue that objective proof of an idea or theory is not possible: "we cannot obtain knowledge independent of our own judgment and social construction" (Daft, 1983: 543). There is no direct access to reality unmediated by language and preconceptions. Perhaps

this is why we judge the ultimate validity of our measures, their "construct validity," not in terms of their correspondence with objective organizational phenomena "out there" but in terms of whether they plausibly support the theory they are meant to test.

Kuhn's (1970) analysis of paradigms as subjectively generated "world views" reinforces these arguments. Kuhn contended that whether a paradigm is retained or abandoned does not depend primarily on the accumulation of objective evidence. Paradigms are, instead, based on the acceptance of a set of metaphysical presuppositions, so that a paradigm shift "can only be made on faith" (Kuhn, 1970: 157). When new paradigms gain acceptance they do not negate their predecessors. They simply provide an alternative view of what constitutes the scientist's relevant universe. The new theories are not always improvements over the old in terms of predictive power; they primarily represent differing frameworks of understanding. The world is seen anew, but not necessarily with greater accuracy.

To argue that truth is a subjective construction is not, however, to suggest that knowledge is an idiosyncratic product. Knowledge is generated not by fiat in the minds of individuals but through a rigorous examination of ideas in public communication. Though we may reject the "myth" that management science is an objective search for truth (Mitroff, 1972; Albrow, 1973), we must recognize that the discipline is, nonetheless, highly systematic in its formulation of knowledge. Ideas must usually survive an exacting process of intellectual scrutiny by advocates of diverse theoretical positions before they graduate to the status of truth statements. New definitions of truth emerge as products of a socially negotiated consensus between truth makers. Such consensus may encourage the negotiators to view the knowledge so produced as "objective," but it is, in fact, intersubjectively generated.

LANGUAGE AS THE PRODUCT OF RESEARCH

This view of knowledge as a product of social negotiation within a scientific community highlights the importance of theoretical language as the vital medium through which the community's negotiations are effected. Once we relinquish the view that theoretical constructs are direct representations of external reality, language itself becomes the essential subject matter of scientific deliberation. Language is not simply a vehicle for transmitting information. Rather, it is the very embodiment of truth; our knowledge structures are linguistic conventions (Gergen, 1982: 101). Scientific fields are word systems created and maintained through a process of negotiation between adherents to alternative theoretical languages.

Objective reality may have little relevance in determining the composition of these word systems. The relationship of a particular theoretical interpretation to a particular empirical phenomenon is often largely negotiable. The best example of this is dialectical analysis: the simultaneous superimposition of two or more competing analytical interpretations on the same empirical phenomenon. For instance, several divergent explanations can be offered to explain the phenomenon of internalization — the inclusion of activities that were formerly part

Administrative Science

of an organization's environment within the boundaries of the enterprise (Astley and Van de Ven, 1983). Internalization has been variously explained as an absorption of critical contingencies that might potentially impede the efficient functioning of the organization's technical core (Thompson, 1967: 39), an adjustment of portfolios designed to exploit product-market opportunities (Leontiades, 1980), a re-establishment of the economy's efficient allocation of resources under conditions of market failure (Williamson, 1975), and an attempt to eliminate fair market exchange and bring the economy under the political control of an elite of giant corporations (Edwards, 1979).

It is quite possible that any one instance of internalization may simultaneously exhibit all of the empirical elements needed to lend support to each of these respective theories, in which case each theory would capture a different aspect of the phenomenon. This, however, is not the point. Rather, the key issue is that different observers tend to apply favored theoretical perspectives in a more or less exclusive manner. Some authors, for example, interpret virtually all occurrences of internalization as instances of market failure, while others interpret virtually all such occurrences as instances of elite control (Van de Ven and Joyce, 1981: ch. 8). The adherents of different positions typically do not concede that their perspective reveals only part of the whole story or that a theoretical synthesis would be desirable. Such adherence to distinctive, biased, essentially partial views of reality is, furthermore, rewarded within the discipline. Persistence in advocating the merits of a particular research paradigm creates visibility and is a common hallmark of academic success.

In many instances, moreover, objective reality cannot be the final arbiter in deciding which theoretical perspective is most valid. The notion of "loose coupling" (Weick, 1976) provides a good illustration. What appears as loose coupling from one point of view might equally well be viewed as tight coupling from another point of view. If we focus attention on the leeway that organizational subunits have to pursue their own subgoals and on the degrees of autonomy they possess vis à vis the remainder of the organization, we will likely conclude that organizations are loosely coupled systems. But such leeway and autonomy can never be total if the organization is to retain its corporate identity. Furthermore, different subunits possess different amounts of autonomy. Consequently, some degree of interdependence and constraint impinges on all subunits, and the activities of some subunits are more closely governed by functional interdependencies and operating ties with the rest of the organization than are other subunits. Thus, it makes as much sense to emphasize degrees of tight coupling as it does to emphasize degrees of loose coupling. Organizations are always both loosely coupled and tightly coupled, depending on the analytical lens we adopt. As Lincoln (1985: 35) observed, there can be no objective choice between alternative perspectives: the validity of the perspective employed depends on the context of inquiry and on the research concerns that guide investigation. The variety of ways to reconstitute our knowledge of organizational reality is, in this sense, bounded only by theoretical ingenuity in inventing new linguistic constructions.

Linguistic Ambiguity

Not only is language, rather than objective fact, the chief product of research, but ambiguous, empirically imprecise language, dominates theorizing. The maintenance of linguistic ambiguity enhances a theory's conceptual appeal by widening its potential applicability. The very generality of ambiguous constructs assures their widespread importance in the discipline, since a great number and variety of more specific propositions can be included within their overarching frame of reference. The most general and abstract theories, the ones farthest removed from empirical reality, exert a disproportionate influence on the field by virtue of their sheer ubiquity as umbrella concepts to which a multiplicity of more explicit hypotheses can be attached.

The abstract, empirically nonrefutable hypotheses of general systems theory, for example, had substantial influence on administrative science because they provided a way to organize a large body of what might otherwise seem to be unrelated findings (Peery, 1972). Resource dependence theory fulfills a similar role: its popularity as a general orienting framework of analysis is enhanced because virtually any organizational activity can be defined as a resource (Pfeffer and Salancik, 1978: 259). Again, much of the appeal of transaction-costs theory rests on its claim to produce a "genuine synthesis between economics and organization theory" (Williamson and Ouchi, 1981), a claim that can only be upheld because of the very broad connotation of the theory's central concept (Perrow, 1981: 375; Cook and Emerson, 1984: 23; Leblebici, 1985: 100).

The scientific appeal of abstract analytical frameworks lies in the symbolic, sense-making functions they fulfill (Frost and Morgan, 1983). Analytical frameworks offer a satisfying sense of understanding; they "explain" why things are related in a certain manner. Linguistic ambiguity, moreover, is what allows such analytical frameworks to function symbolically, as reference points to which multiple, diverse, sometimes even contradictory meanings can be attached. By fixating cognition on common points of reference, ambiguous constructs bridge subgroup differences in perception and world view and facilitate communication. Ambiguous language allows researchers to "talk past one another" (Frost and Morgan, 1983: 220) and yet talk to each other. Abstract concepts give meaning and structure to the researcher's psychological reality; they are robust mechanisms for generating scientific communion.

Linguistic ambiguity, furthermore, enhances the pragmatic value of administrative theory, while empirical precision only diminishes a theory's applicability. Generality and abstraction do not reduce the usefulness of theoretical constructs; the maintenance of ambiguity is crucial to their practical functioning. By avoiding specification of particular courses of action relevant for particular circumstances, theories can provide justification for a wide variety of different actions to take place. Managers intent on applying such theories do not mechanically translate theoretical prescriptions into behavior; instead, they rationalize their activities by pointing to events that seem credibly related to the theories' abstract terms. In doing so, managers must rely on their inside knowledge of the organiza-

Administrative Science

tional setting and use ingenuity in accounting for practical actions by an appeal to general ideas. Theory, in this event, functions not as a technical guide to action, but as a device for enabling the manager to activate knowledge already possessed.

The theoretical frameworks offered by consulting firms specializing in specific consulting "packages" illustrate this point. Such firms have more or less standard sets of analytical tools which, through an artful redefinition of clients' problems, they can apply in one combination or another to every client in their portfolio. "Strategic planning" consultants provide one currently popular example of how this works. These consultants incorporate everything a firm does under its "corporate strategy." This notion is so vague it defies most efforts to define it; it can mean as little or as much as consultants or managers want it to mean. Owen (1982: 10) attributed the success of the Boston Consulting Group's market growth/market share matrix to exactly this kind of flexibility: "[The matrix] can be used to illustrate why a client company ought to fire a group of managers, or attempt an unfriendly merger, or unload an ailing division. It is, in short, the philosopher's stone of the consulting business." While this kind of consulting offers little in the way of specific techniques with direct application, it does legitimate managerial action through oblique symbolic references to the mystique of "strategic" necessity.

The Role of Imagery

The abstraction of theoretical language from empirical reality is nowhere better demonstrated than in the use of compelling visual imagery that requires an audience to join an author in a kind of make-believe. Theorists often self-consciously move beyond the data as they generate fictional constructs, products of imagination for which no empirical counterpart exists. In this regard, Kaplan (1964: 297) noted that "theoretical" means not only "abstract," in that theories select from the materials of experience, but also "conceptual," in that theories construct from the selected materials something that has no basis in experience at all. While theories must have empirical referents, they are also constituted by purely ideational ingredients. "A theory must somehow fit God's world, but in an important sense it creates a world of its own" (Kaplan, 1964: 309).

The widespread use of ideal types, such as Burns and Stalker's (1961) "mechanistic" and "organic" organizations and Mintzberg's (1979) "structural configurations," provides a good example. Such constructs are not merely compilations of empirical phenomena in taxonomical categories; they are intentional fictionalizations of the reality under examination. Typologically formed theory is, thus, empirically unverifiable, by its very nature (Hendricks and Peters, 1973: 38). Ideal types function not just as a summary and classification of observable phenomena but also as an embodiment of the theorist's sense of logical aesthetics. In the process of constructing an ideal type the scientist deliberately departs from reality by accentuating certain of the type's attributes:

An ideal type is formed by the one-sided accentuation of one or more points of view and by the synthesis of a great many diffuse, discrete,

more or less present and occasionally absent concrete individual phenomena, which are arranged according to those one-sided emphasized viewpoints into a unified analytical construct. In its conceptual purity, this mental construct cannot be found anywhere in reality. (Weber, 1963: 398)

An equally important use of fictional imagery lies in the use of metaphorical constructs. Morgan (1980) argued that the successful use of metaphor always involves a degree of creative invention because it is based on partial truth only; a selective comparison between the subjects involved in the metaphorical process emphasizes features of similarity between them while it suppresses features of dissimilarity between them. "Effective metaphor is a form of creative expression which relies upon constructive falsehood as a means of liberating the imagination" (Morgan, 1980: 612). Pinder and Bourgeois (1982), consequently, argued that the use of metaphors is not honest science and is misleading as a guide to objective truth. Despite lack of objectivity, however, a large amount of intellectual activity within administrative science is devoted to the trading of metaphors, representing organizations, for example, in the compelling imagery of "machines" (Ward, 1964), "garbage cans" (Cohen, March, and Olsen, 1972), "iron cages" (Weber, 1946), "theaters" (Mangham and Overington, 1983), "psychic prisons" (Morgan, Frost, and Pondy, 1983), and so on.

Crystallizing theory in attractive images is an important part of scientific writing. Imagery, not the bare reporting of facts, captures the scientific imagination. As Daft (1983) noted, our research products must have "symmetry" and "beauty"; they must "hang together" in meaningful units having a "poetic" quality. Theories gain favor because of their conceptual appeal, their logical structure, or their psychological plausibility. Internal coherence, parsimony, formal elegance, and so on prevail over empirical accuracy in determining a theory's impact. Published theoretical works generally attract greater attention than purely empirical studies because of their ability to excite these essentially aesthetic sensibilities.

THEORETICAL TRADITIONS AND ICONOCLASM

While the construction of images is important in creating attractive theories, the breaking of images is probably even more important. Nothing captivates interest more than iconoclasm. As Davis (1971) contended, the most "interesting" theories are those that constitute an attack on the taken-for-granted premises of established theoretical traditions. By denying the validity of routinely held cognitive assumptions, a theory attracts attention, forcing its audience to re-evaluate subject matter from a novel viewpoint. New propositions are interesting or uninteresting only in relation to this baseline of traditionally accepted knowledge. What makes a theory important is its novel stance vis à vis what others in the discipline previously believed, rather than its relationship to "objective" truth. "A theorist is considered great, not because his theories are true, but because they are interesting" (Davis, 1971: 309). Rationalizing (Staw, 1980), enactment (Weick, 1979), and anarchic (March and Olsen, 1976) theories, for example, are popular not because organizational life is necessarily more rationalizing than rational, more enacted than constrained, and more

Administrative Science

anarchic than organized, but because these theories stand in stark relief to old preconceptions.

The corollary is that a proposition will be uninteresting if, instead of denying taken-for-granted assumptions, it simply affirms those assumptions. Old paradigms fall from grace not because they are wrong but because they are boring. As Davis (1971: 309) put it, "Those who carefully and exhaustively verify trivial theories are soon forgotten; whereas those who cursorily and expediently verify interesting theories are long remembered." Mediocre scientists, then, are those who are unable to see beyond established paradigms because of their failure to grasp and articulate their colleagues' ground assumptions. Successful scientists, on the other hand, are intuitively perverse, always ready to question accepted world views and create opportunities for the critical rejection of what is taken as given by others.

Christensen-Szalanski and Beach (1984) found support for this idea that a theory's interest value, rather than its truth value, determines its popularity. An investigation of citation frequencies of journal articles in the judgmental decision-making literature revealed that studies corroborating the existence of rationality in decision making were cited far less often than studies observing apparently irrational behavior. Evidence of rational decision making, though widely observed and reported in journal articles, tended to be ignored in citations, while counterintuitive results received a disproportionate amount of attention from commentators in the literature. They concluded that such "citation bias" reflects a preoccupation with "fad and fashion," as the field jumps on the bandwagon of one "hot" topic after another. As this example illustrates, the value attached to a theory, and the assessment of its contribution to scientific progress, is primarily determined not by its capacity to describe empirical reality, but by its intellectual novelty vis à vis the field's established theoretical traditions. As Staw (1985: 97) observed, this is why much of our research tends to be "literature driven" rather than problem driven; instead of being directed toward extant organizational problems, new theoretical insights are often chosen primarily for their novelty — they fill gaps in the current literature, create controversy by challenging accepted beliefs, and so on.

FRAGMENTATION OF THE DISCIPLINE

Given the strong incentives for authors to create intellectual novelty and pursue distinctive paradigms, we should not be surprised that administrative science is a collection of loosely related topics. While subgroups within the discipline may converge on a given world view, no discipline-wide agreement about analytical perspectives, methodology, or even what is regarded as appropriate subject matter has emerged. Theories of the middle range proliferate, along with measures, terms, concepts, and research paradigms (Pfeffer, 1982: 1). New theoretical advances do not seem to build cumulatively on previous findings; instead they add to the bewildering variety of perspectives within the field (Calder, 1977; Whitley, 1984a). The result is disciplinary fragmentation. For example, topics in introductory textbooks are so loosely interconnected that virtually any of them can be arbitrarily dropped without damaging

the overall integration of the text or its internal flow of logic (Zammuto and Connolly, 1984a, 1984b).

Authors usually offer one of two possible explanations for the fragmented state of the discipline. First, administrative science might simply be in a preparadigmatic stage of development (Daft, 1980: 623; Zammuto and Connolly, 1984a, 1984b). As a fledgling discipline, the field may still be struggling to define an objective core of knowledge. Second, administrative science may be a "multiple paradigm" discipline rather than a preparadigmatic discipline (Pinder and Moore, 1980). We may need a wide variety of analytical perspectives to shed light on all of the diverse aspects of organizational life (Pondy and Boje, 1976). Any one theory captures only a part of organizational reality so that "no single language can represent the many complex dimensions of organizations" (Daft, 1980: 633).

This paper favors a third explanation: disciplinary fragmentation results from the proliferation of what Perrow (1980) calls "interest theories" — theories that reflect the kind of interests and problems that investigators have been trained and schooled in. As schools of thought increase in number, the variety of analytical approaches multiplies. The same practical reality can provide fodder for the generation of multiple perspectives reflecting the different agendas that observers bring to the arena of scientific inquiry. In this view, analytical perspectives do not proliferate because each sheds light on only a part of reality, so that a complete description of organizational life requires a plurality of theories. Rather, the problem is that different theorists bring different intentions to the study of administration; they are set on investigating quite different things, interpreting reality through their own conceptual filters, and drawing conclusions that fit their own world views. Nor, apparently, are we in the process of developing from a preparadigmatic, fragmented discipline to a mature, theoretically integrated discipline. An increasing divergence, rather than convergence, in perspectives characterizes the field. As Koontz (1980) put it, the "management theory jungle" is becoming progressively more dense and impenetrable.

The pluralistic nature of administrative science highlights what Kuhn (1970) and Ritzer (1980: 200) identified as the clearly political character of the struggle in the sciences in general between advocates of diverse theoretical approaches. We are likely to continue having a difficult time conducting normal science within established paradigms, since we must constantly defend our flanks against attacks from supporters of alternative perspectives. As far as scientific progress is concerned, the theories that gain dominance are those that are able to win the most converts; they need not necessarily have greater explanatory power in order to emerge victorious. To engage in the scientific enterprise, in this view, is simply to convince others of the intrinsic superiority of subjectively favored interpretations (Astley, 1984). Theories do not gain hegemony because they are better than their predecessors, but because they are able to attract adherents and withstand attacks from those who support other positions.

THE SOCIAL CONTROL OF INTELLECTUAL ADVANCE

The existence of multiple, competing paradigms within administrative science is a characteristic the field shares with other social sciences. Lodahl and Gordon (1972), for example, found far less consensus over paradigms within the social sciences than within the natural sciences. This difference between the natural and social sciences is rooted in their respective relationships to the phenomena they study. Social science research changes the phenomena under investigation more than does research in the natural sciences. Astronomic theory does not alter the movement of the planets in the way that behavioral theory affects social conduct. Social activity is shaped by culturally shared frameworks of conceptual understanding. At the same time, the chief product of social science is conceptual understanding. Consequently, as our concepts and theories are communicated and filter into lay discourse, they reconstitute the very reality they seek to investigate. This is particularly true in administrative science, where our theories function as instruments of managerial control (Bendix, 1956). As a practically oriented science, we furnish advice about potential interventions in organizational affairs, and so our concepts are especially accessible to managerial audiences (Whitley, 1984b: 373). The relationship between administrative theory and managerial practice is, consequently, a dialectical one, whereby theory not only reflects but also structures its own subject matter (Albrow, 1980).

Because of reflexivity between researchers and the objects they study, observed relationships between social and organizational phenomena are subject to constant change. Administrative scientists do not, therefore, map out objective and enduring contours of organizational life. Rather, we engage in a form of praxis (Benson, 1977). Our field does not simply reflect stable organizational realities; it is a life form that constantly infiltrates the domain of managerial action. This is why Gergen (1982) argued that the traditional yardstick for theoretical appraisal, namely "objectivity," or truth value, is not germane for the social sciences. The yardstick of objectivity presupposes stability in the phenomena under investigation, and application of this criterion to the constant flux of the interaction between researcher and the object studied is not possible.

In the absence of objective scientific criteria, the evaluation of theoretical worth is, inherently, a problematic task. In consequence, theoretical appraisal becomes influenced by social factors. Without universal standards to appeal to, appraisers of theory must, by necessity, fall back on particularistic, socially grounded frames of reference, in contrast to purely intellectual ones. Crane (1967), for example, found that the selection of articles for publication in social science journals was affected by social factors. Authors' institutional affiliations tended to correspond to those of the editors of the journals that published their work. Crane concluded that, as a result of academic training, editors respond to certain aspects of methodology, theoretical orientation, and mode of expression in the writings of those who received similar training; moreover, personal ties also seemed to influence the evaluation of manuscripts. Yoels (1974), additionally, found that editorial appointments were influenced by similarity in institutional affiliation between outgoing editors and their appointees.

Pfeffer, Leong, and Strehl (1977) linked the use of such particularistic standards within a scientific discipline to the degree to which that discipline exhibited consensus over paradigms. These authors found that particularistic criteria influenced the selection of journal publications in political science and sociology, but not in chemistry. They concluded that the relative dissensus and uncertainty over paradigms in the social sciences increases the likelihood of particularistic decision making, because, in the absence of objective criteria to anchor evaluations of manuscripts, decision makers turn to social cues as a means of reducing their uncertainty. Work is evaluated by an appeal to social standards deriving from the decision maker's background and position in social networks. Thus, social similarity between author and decision maker increases publication chances. Uncertainty in the theoretical sphere, in other words, is resolved by reference to social criteria. Intellectual advance is governed in part by social processes.

Another manifestation of this lies in the fact that the social apparatus for processing manuscripts is much more demanding in social science than in physical science journals. Beyer (1978), for example, found that editors of social science journals had greater difficulty than editors of physical science journals in arriving at publication decisions and, consequently, required far more intensive manuscript revision and copy editing in order to effect greater consensus, by bringing authors' views more into line with referees' wishes. The ambiguities generated by intellectual novelty require an elaborate system of feedback and collective control over research to maintain some degree of stability in those standards that maintain the field as a distinctive academic discipline. This is what Kuhn (1970: 180) meant when he stated: "A paradigm governs in the first instance, not a subject matter, but rather a group of practitioners." He argued that scientific communities act to preserve a certain level of consensus, ignoring empirical results and ideas that could potentially break down agreement on extant paradigms until new paradigms have emerged.

Because a minimum degree of consensus is a necessary condition for scientific communities to exist, new theoretical advances must overcome the resistance encountered in those political and institutional processes designed to maintain the discipline as a stable social system. The degree of such resistance is formidable, as evidenced in the high rejection rates of social science journals, as compared to physical science journals (Zuckerman and Merton, 1971). Administrative science is no exception. Our journals exhibit a distinctly conservative bias in which reviewers and editors adopt what Pondy (1985: 211) described as a "prosecution mentality" against authors, one that is typically overcome only through intense "negotiation and bargaining" (Pondy, 1985: 215). Most papers do not survive this social process of negotiation, as seen in *ASQ's* almost 90 percent rejection rate (Weick, 1985: 371). Such rejection rates in the social sciences may be a direct consequence of intellectual uncertainty and lack of consensus over paradigms.

In this context, getting manuscripts accepted always requires that authors, as part of their negotiation strategies, pay respect to the field's accepted paradigms. Though papers must be

Administrative Science

novel, they must not be offensively contrary or alienating to reviewers. Contributions must somehow "fit in" with the field's theoretical traditions. Relating new ideas to established analytical frameworks defines their significance and meaning. Innovation must, therefore, be balanced against orthodoxy. While papers that are too similar to articles already published are unlikely to survive the review process, papers falling clearly outside mainstream research are also liable to be rejected (Kerr, Tolliver, and Petree, 1977). New approaches must be oriented toward the work of colleagues and potentially useful in their research. Researchers must properly set the stage for the introduction of their own approach by first accurately articulating their colleagues' positions, even if they subsequently deny the validity of such positions. For example, Burrell and Morgan (1979) probably would not have gained much of an audience for their "radical humanist" paradigm without first counterposing it to the field's dominant "functionalist" paradigm in a 2×2 matrix.

Social Structure of the Discipline

The importance of social factors in the development of the field becomes particularly evident when we investigate the social structure of administrative science as a sociological phenomenon in its own right. The discipline can be viewed as a distinct type of work organization. Like other disciplines, it is organized around the competitive pursuit of intellectual reputations; the acquisition of a positive reputation determines career advancement and access to material rewards. Whitley (1982) has described such academic career systems as "reputational work organizations" and has traced the emergence of a reputational system within "management studies" in general and administrative science in particular in the 1950s and 1960s (Whitley, 1984a). During this period, the rapid rise of business schools, the establishment of management doctoral programs, and the consequent growth of a labor force invested with the research ethic allowed administrative science partially to emancipate itself from its origins in managerial practice. The field began to control its own allocations of resources, especially jobs, by reference to academic criteria rather than by reference to those of managers or students.

The reputational work organization of administrative science is, by its nature, hierarchical. The field is committed to the production of intellectual innovations, and yet it must restrict the extent of such innovations to maintain its differentiated structure of reputations. Thus, the discipline is governed by a hierarchical evaluation system in which a stringent selection process rejects the majority of research contributions. The key figures in this system are those gatekeepers who determine which articles will be accepted by prestigious journals. Opinions of what good science is and who has done good work are based on judgments made by these gatekeepers through the discipline's formal evaluation system. Evaluations of articles are not founded solely on a direct reading and analysis of their intrinsic ideas but also, to a large extent, on judgments made about those articles in the review process. In other words, we internalize the judgment of the formal evaluation system by giving people more credit for publications in prestigious journals (Cole, 1983). This internalized judgment, rather than a

direct personal assessment of a paper's worth, typically dominates our opinions of research.

For this reason, administrative science can be regarded as possessing social reality in its own right. An author's reputation is not solely derived from his contribution to the dissemination of knowledge, but also from his social achievement in being published in top journals. Reputations are generated by and lodged in the social structure of the discipline and may persist without being directly validated on intellectual grounds by the majority in the discipline who often do not read most of the work on which those reputations are supposedly based. Reputations generated from publishing one piece of work may produce a "halo" effect that carries over to other work. Thus, publications in edited volumes count much more after a scholarly reputation has already been established (Schneider, 1985: 241). To be highly regarded, authors must first get their credentials from publishing in mainline journals (Perrow, 1985: 222). Producing quality work is not enough; it must be certified as being of high quality by the right people. This privilege falls to the gatekeepers who control the discipline's formal evaluation system. These gatekeepers define what will count as important or unimportant work and, in effect, determine what constitutes valid knowledge.

This kind of hierarchical reputational system is characteristic of all scientific disciplines, but its influence is to some extent attenuated in administrative science. To a greater degree than most other disciplines, administrative science is characterized by multiple constituencies. Close ties to the business community, in particular, encourage sponsorship of research from parties external to the discipline. These external parties support lines of inquiry that the discipline's internal control system might not otherwise endorse. Thus, while administrative science is characterized by a hierarchical social structure, it is probably subject to a greater plurality of influences than most other disciplines (Whitley, 1984b). This may explain why the field, as a domain of study, is so fragmented. Even when compared to other social science disciplines, there is relatively little consensus over what makes up the body of knowledge constituting administrative science. The distinctive intellectual composition of the discipline derives directly from the peculiarities of its social structure. This observation reinforces the conclusion that administrative science is a socially constructed product.

CONCLUSION

The body of knowledge constituting administrative science is not an objective representation of administrative practice; it does not, through literal correspondence, simply reflect events and activities in the managerial world. Instead of discovering enduring facts of organizational life and reporting them through neutral description, we actively create truth by assigning meaning to the phenomena we observe and experience. This is not to deny the existence of an objective reality independent of minds, as critics of the social constructionist view sometimes imply (e.g., Armstrong, 1980); the point is only that our *knowledge* of objective reality is subjectively constructed. Just as organizational participants subjectively interpret events in order to experience everyday life as meaningful, so administra-

Administrative Science

tive scientists superimpose analytical frameworks on empirical observations to render knowledge meaningful.

It is, of course, always possible that objective reality may not support a particular theoretical interpretation. Weak or nonexistent support for a theory, however, may be a poor indicator of that theory's acceptance or popularity within the discipline. What counts as knowledge in the theoretical sphere may be only loosely coupled to the world of practice (Astley, 1984). Miner (1984) found that the perceived importance of theories in organizational science has little to do with their validity as measured by traditional standards of research. A theory's importance — estimated by what 100 knowledgeable scholars in the field considered to be a significant research contribution — was unrelated to its capacity to predict empirical findings. Bourgeois and Pinder (1984) similarly found that the hegemony of theories in organizational science is "independent of their evidence, predictive power, and internal consistency," and concluded that the model of scientific progress as consisting of "successively closer approximations to [objective] truth" is untenable. Theories may prevail, moreover, even when confronted by apparently decisive negative evidence. Samelson (1980), for example, drew attention to a number of classic studies that, despite repeated disconfirmation of their findings in subsequent research, continued to be heavily cited. Scientific communities often reach this kind of closure on ideas they find congenial and suppress evidence that does not confirm their preferred biases (Mahoney, 1977).

These considerations may seem paradoxical in view of the fact that most research in administrative science is based on empirical observation. If scientific advance within the discipline does not occur linearly through the cumulative discovery of objective fact, then what purpose is served by data collection? In light of the above arguments, perhaps we should abandon empirical investigation. Further consideration of the functions performed by empirical research, however, suggests that we should simply reinterpret the significance of such research rather than abandon it. Data can be used effectively as a form of illustration having persuasive appeal for a theoretical interpretation. By contextually grounding abstract terms and theories, we add imagery to them and invite others to employ these terms and theories in interpreting their own research experiences. Empirical documentation, in this case, serves not as a device for verifying a theory's agreement with objective fact, but as rhetorical support in persuading others to adopt a particular world view. Because empirical data is always theory-dependent, its main function is to embellish rather than validate the theory from which it was generated.

In this view, administrative science is, first and foremost, a theoretical enterprise. Theoretical creativity, rather than information-gathering, drives scientific progress. The generation of knowledge must be seen as an essentially interpretive or expressive activity. As soon as we abandon the notion that ideas and theories are like mirrors reflecting what is happening in the outside world, we cannot view research as the instrumental acquisition of information about external reality. Instead, external phenomena furnish opportunities for researchers to express themselves creatively through the theoretical interpretations they impose on data. As we engage in research

and communicate the results to others, we participate in what Morgan (1983: 406) described as a "reflective conversation." Through scientific dialogue we articulate our relationship to the world and fulfill our intellectual potential. The real significance of research lies not in the mechanical collection and reporting of data, but in the opportunity to extend scientific imagination by developing new modes of thinking and interpretation. Unfortunately, this goal of research is not adequately institutionalized in the social and value structure of the discipline or properly rewarded by its formal evaluation systems. Perhaps an explicit awareness of the socially constructed nature of knowledge may encourage a more liberated view of scientific progress, one that is reinforced by the social context in which research findings are communicated.

REFERENCES

- Albrow, Martin**
1973 "The study of organizations — Objectivity or bias?" In Graeme Salaman and Kenneth J. Thompson (eds.), *People and Organizations*: 396–413. London: Longman.
- 1980 "The dialectic of science and values in the study of organizations." In Graeme Salaman and Kenneth J. Thompson (eds.), *Control and Ideology in Organizations*: 278–296. Cambridge, MA: MIT Press.
- Armstrong, J. Scott**
1980 "Advocacy as a scientific strategy: The Mitroff myth." *Academy of Management Review*, 5: 509–512.
- Astley, W. Graham**
1984 "Subjectivity, sophistry and symbolism in management science." *Journal of Management Studies*, 21: 259–272.
- Astley, W. Graham, and Andrew H. Van de Ven**
1983 "Central perspectives and debates in organization theory." *Administrative Science Quarterly*, 23: 245–273.
- Bendix, Reinhard**
1956 *Work and Authority in Industry: Ideologies of Management in the Course of Industrialization*. Berkeley, CA: University of California Press.
- Benson, J. Kenneth**
1977 "Organizations: A dialectical view." *Administrative Science Quarterly*, 22: 1–21.
- Beyer, Janice M.**
1978 "Editorial policies and practices among leading journals in four scientific fields." *Sociological Quarterly*, 19: 68–88.
- Bittner, Egon**
1965 "The concept of organization." *Social Research*, 32: 239–255.
- Bourgeois, V. Warren, and Craig C. Pinder**
1984 "The nonlinearity of progress in organizational science: Implications for implementation." Unpublished working paper, University of British Columbia, Vancouver.
- Burns, Tom, and G. M. Stalker**
1961 *The Management of Innovation*. London: Tavistock.
- Burrell, Gibson, and Gareth Morgan**
1979 *Sociological Paradigms and Organizational Analysis*. London: Heinemann.
- Calder, Bobby J.**
1977 "An attribution theory of leadership." In Barry M. Staw and Gerald R. Salancik (eds.), *New Directions in Organizational Behavior*: 179–204. Chicago: St. Clair Press.
- Christensen-Szalanski, Jay J.J., and Lee Roy Beach**
1984 "The citation bias: Fad and fashion in the judgement and decision literature." *American Psychologist*, 39: 75–78.
- Cohen, Michael D., James G. March, and Johan P. Olsen**
1972 "A garbage can model of organizational choice." *Administrative Science Quarterly*, 17: 1–25.
- Cole, Stephen**
1983 "The hierarchy of the sciences?" *American Journal of Sociology*, 89: 111–139.
- Cook, Karen S., and Richard M. Emerson**
1984 "Exchange networks and the analysis of complex organizations." In Samuel B. Bacharach and Edward J. Lawler (eds.), *Research in the Sociology of Organizations*: 4: 1–30. Greenwich, CT: JAI Press.
- Crane, Diana**
1967 "The gatekeepers of science: Some factors affecting the selection of articles for scientific journals." *American Sociologist*, 2: 195–201.
- Daft, Richard**
1980 "The evolution of organization analysis in ASQ, 1959–1979." *Administrative Science Quarterly*, 25: 623–636.
- 1983 "Learning the craft of organizational research." *Academy of Management Review*, 8: 539–546.
- Davis, Murray S.**
1971 "That's interesting." *Philosophy of the Social Sciences*, 1: 309–344.
- Edwards, Richard**
1979 *Contested Terrain: The Transformation of the Workplace in the Twentieth Century*. New York: Basic Books.
- Evered, Roger, and Meryl R. Louis**
1981 "Alternative perspectives in the organizational sciences: Inquiry from the inside, and inquiry from the outside." *Academy of Management Review*, 6: 385–395.
- Feyerabend, Paul**
1975 *Against Method: Outline of an Anarchistic Theory of Knowledge*. London: Redwood Burn.
- Frost, Peter J., and Gareth Morgan**
1983 "Symbols and sensemaking: The realization of a framework." In Louis R. Pondy, Peter J. Frost, Gareth Morgan, and Thomas C. Dandridge (eds.), *Organizational Symbolism*: 207–236. Greenwich, CT: JAI Press.
- Gergen, Kenneth J.**
1982 *Toward Transformation in Social Knowledge*. New York: Springer-Verlag.

Administrative Science

- Hendricks, J., and C. B. Peters**
1973 "The ideal type and sociological theory." *Acta Sociologica*, 16: 31-40.
- Kaplan, Abraham**
1964 *The Conduct of Inquiry: Methodology for Behavioral Science*. New York: Thomas Y. Crowell.
- Kerr, Steven, James Tolliver, and Doretta Petree**
1977 "Manuscript characteristics which influence acceptance for management and social science journals." *Academy of Management Journal*, 20: 132-141.
- Koontz, Harold**
1980 "The management theory jungle revisited." *Academy of Management Review*, 5: 175-188.
- Kuhn, Thomas**
1970 *The Structure of Scientific Revolutions*, 2d ed. Chicago: University of Chicago Press.
- Leblebici, Huseyin**
1985 "Transactions and organizational forms: A reanalysis." *Organization Studies*, 6: 97-116.
- Leontiades, Milton**
1980 *Strategies for Diversification and Change*. Boston: Little, Brown.
- Lincoln, Yvonna S.**
1985 *Organizational Theory and Inquiry: The Paradigm Revolution*. Beverly Hills, CA: Sage.
- Lodahl, Janice B., and G. Gordon**
1972 "The structure of scientific fields and the functioning of university graduate departments." *American Sociological Review*, 37: 57-72.
- Mahoney, M.J.**
1977 "Publication prejudices: An experimental study of confirmatory bias in the peer review system." *Cognitive Therapy and Research*, 1: 161-175.
- Mangham, Iain L., and Michael A. Overington**
1983 "Dramatism and the theatrical metaphor." In Gareth Morgan (ed.), *Beyond Method: Strategies for Social Research*: 219-233. Beverly Hills, CA: Sage.
- March, James G., and Johan P. Olsen**
1976 *Ambiguity and Choice in Organizations*. Bergen, Norway: Universitetsforlaget.
- Miner, John B.**
1984 "The validity and usefulness of theories in an emerging organizational science." *Academy of Management Review*, 9: 296-306.
- Mintzberg, Henry**
1979 *The Structuring of Organizations*. Englewood Cliffs, NJ: Prentice-Hall.
- Mitroff, Ian I.**
1972 "The myth of objectivity, or, why science needs a new psychology of science." *Management Science*, 18: B613-B618.
- Morgan, Gareth**
1980 "Paradigms, metaphors and puzzle solving in organization theory." *Administrative Science Quarterly*, 25: 605-622.
1983 "In research, as in conversation, we meet ourselves." In Gareth Morgan (ed.), *Beyond Method: Strategies for Social Research*: 405-407. Beverly Hills, CA: Sage.
- Morgan, Gareth, Peter J. Frost, and Louis R. Pondy**
1983 "Organizational symbolism." In Louis R. Pondy, Peter J. Frost, Gareth Morgan, and Thomas C. Dandridge (eds.), *Organizational Symbolism*: 3-38. Greenwich, CT: JAI Press.
- Owen, David**
1982 "Those who can't consult." *Harpers*, November: 8-17.
- Peery, Newman S.**
1972 "General systems theory: An inquiry into its social philosophy." *Academy of Management Journal*, 15: 495-510.
- Perrow, Charles**
1980 "'Zoo story' or 'Life in the organizational sandpit.'" In Graeme Salaman and Kenneth Thompson (eds.), *Control and Ideology in Organizations*: 259-277. Cambridge, MA: MIT Press.
1981 "Markets, hierarchies and hegemony." In Andrew H. Van de Ven and William F. Joyce (eds.), *Perspectives on Organization Design and Behavior*: 371-386. New York: Wiley.
1985 "Journaling careers." In Larry L. Cummings and Peter J. Frost (eds.), *Publishing in the Organizational Sciences*: 220-230. Homewood, IL: Irwin.
- Pfeffer, Jeffrey**
1982 *Organizations and Organization Theory*. Boston: Pitman.
- Pfeffer, Jeffrey, Anthony Leong, and Katherine Strehl**
1977 "Paradigm development and particularism: Journal publication in three scientific disciplines." *Social Forces*, 55: 938-951.
- Pfeffer, Jeffrey, and Gerald R. Salancik**
1978 *The External Control of Organizations: A Resource Dependence Perspective*. New York: Harper & Row.
- Pinder, Craig C., and V. Warren Bourgeois**
1982 "Controlling tropes in administrative science." *Administrative Science Quarterly*, 27: 641-652.
- Pinder, Craig C., and Larry F. Moore**
1980 "The inevitability of multiple paradigms and the resultant need for middle range analysis in organization theory." In Craig C. Pinder and Larry F. Moore (eds.), *Middle Range Theory and the Study of Organizations*: 87-100. Boston: Martinus Nijhoff.
- Pondy, Louis R.**
1985 "The reviewer as defense attorney." In Larry L. Cummings and Peter J. Frost (eds.), *Publishing in the Organizational Sciences*: 210-219. Homewood, IL: Irwin.
- Pondy, Louis R., and David M. Boje**
1976 "Bringing mind back in." William M. Evan (ed.), *Frontiers in Organization and Management*: 83-104. New York: Praeger.
- Ritzer, George**
1980 *Sociology: A Multiple Paradigm Science*, rev. ed. Boston: Allyn & Bacon.
- Samelson, Franz**
1980 "J. B. Watson's Little Albert, Cyril Burt's twins, and the need for a critical science." *American Psychologist*, 35: 619-625.
- Schneider, Benjamin**
1985 "Some propositions about getting research published." In Larry L. Cummings and Peter J. Frost (eds.), *Publishing in the Organizational Sciences*: 238-247. Homewood, IL: Irwin.

Staw, Barry M.

- 1980 "Rationality and justification in organizational life." In Barry M. Staw and Larry L. Cummings (eds.), *Research in Organizational Behavior*, 2: 45-80. Greenwich, CT: JAI Press.
- 1985 "Repairs on the road to relevance and rigor: Some unexplored issues in publishing organizational research." In Larry L. Cummings, and Peter J. Frost (eds.), *Publishing in the Organizational Sciences*: 96-107. Homewood, IL: Irwin.

Thompson, James D.

- 1967 *Organizations in Action*. New York: McGraw-Hill.

Van de Ven, Andrew H., and William F. Joyce

- 1981 *Perspectives on Organization Design and Behavior*. New York: Wiley.

Ward, John W.

- 1964 "The ideal of individualism and the reality of organization." In Earl F. Cheit (ed.), *The Business Establishment*: 37-76. New York: Wiley.

Warriner, Charles K., Richard H. Hall, and Bill McKelvey

- 1981 "The comparative description of organizations: A research note and invitation." *Organization Studies*, 2: 173-175.

Weber, Max

- 1946 *From Max Weber: Essays in Sociology*. H. H. Gerth and C. W. Mills, trans. and eds. New York: Oxford University Press.
- 1963 "Objectivity in social science and science policy." In M. Nathanson (ed.), *Philosophy of the Social Sciences*: 355-418. New York: Random House.

Weick, Karl E.

- 1976 "Educational organizations as loosely coupled systems." *Administrative Science Quarterly*, 21: 1-19.
- 1979 *The Social Psychology of Organizing*, 2d ed. Reading, MA: Addison-Wesley.
- 1985 "Editing innovation into *Administrative Science Quarterly*." In Larry L. Cummings and Peter J. Frost (eds.), *Publishing in the Organizational Sciences*: 366-376. Homewood, IL: Irwin.

Whitley, Richard

- 1982 "The development and establishment of the sciences as reputational organizations." In N. Elias (ed.), *Scientific Establishments and Hierarchy*: 114-126. Dordrecht: Reidel.
- 1984a "The fragmented state of management studies: Reasons and consequences." *Journal of Management Studies*, 21: 331-348.
- 1984b "The scientific status of management research as a practically oriented social science." *Journal of Management Studies*, 21: 369-390.

Williamson, Oliver

- 1975 *Markets and Hierarchies*. New York: Free Press.

Williamson, Oliver, and William G. Ouchi

- 1981 "The markets and hierarchies program of research: Origins, implications, prospects." In Andrew H. Van de Ven and William F. Joyce (eds.), *Perspectives on Organization Design and Behavior*: 347-370. New York: Wiley.

Yoels, W. C.

- 1974 "The structure of scientific fields and the allocation of editorships on scientific journals: Some observations on the politics of knowledge." *Sociological Quarterly*, 15: 264-276.

Zammuto, Raymond, and Terry Connolly

- 1984a "Coping with disciplinary fragmentation." *Organizational Behavior Teaching Review*, 9 (2): 30-37.
- 1984b "On the dubious health of organizational research. Do we need a doctor or an undertaker?" Paper presented at the Western Academy of Management Meeting, Vancouver, Canada, April 14, 1984.

Zuckerman, H., and Robert K. Merton

- 1971 "Patterns of evaluation in science: Institutionalization, structure and function of the referee system." *Minerva*, 9: 66-101.