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Organizational Theory Development: Displacement of Ends?

William McKinley

William McKinley
Southern Illinois University at Carbondale, USA

Abstract

In this essay I argue that organization theory has witnessed a significant displacement of ends over the last 30 years. Whereas in the 1960s and 1970s the dominant goal of the discipline was achieving consensus on the validity status of theories, today the overriding goal appears to be development of new theory. Formerly new theory development was considered a means to the end of attaining consensus on theory validity, but was not the only activity deemed necessary to accomplish that goal. In addition, instrumental standardization and replication were viewed as important. The contemporary displacement of ends toward new theory development creates the paradox that organization theory today is both epistemologically simpler (in terms of the intellectual activity deemed desirable) and more complex theoretically than it was 30 years ago. I discuss the advantages and disadvantages of the displacement of ends toward new theory development in organization theory, and offer some possible remedies that are designed to reallocate priorities and resources toward the instrumentation, theory testing, and replication components of the research process. I also propose an agenda of future research in the history and sociology of organization science that would study the displacement of ends hypothesized here, with a view to improving our understanding of how organization theory has evolved and how its knowledge could be made more useful to managers.

Keywords: displacement of ends, knowledge, management practice, organization theory

These are exciting times for organization theory. A proliferation of new theoretical perspectives and schools of thought has emerged in the discipline (McKinley et al. 1999; Pfeffer 1993), and new theory development has come to be seen as an important goal, if not the ultimate goal, of the field (e.g. *Academy of Management Journal* 2007; Hambrick 2007; Smith and Hitt 2005). At the same time, the sense of excitement is combined with a persistent malaise. Organization theorists worry about whether the discipline should or should not adopt a paradigm (Pfeffer 1993; Van Maanen 1995a,b), and about whether the existing theoretical schools in the discipline are incommensurable (Burrell and Morgan 1979; Donaldson 1998; Kaghan and Phillips 1998; McKinley and Mone 1998; Scherer 1998). The difficulty of applying organization studies findings to practice seems also to have become a permanent issue (e.g. Beyer and Trice 1982; Cheng and McKinley 1983; Cohen 2007; Rynes et al. 2007; Rousseau 2007; Thomas and Tymon 1982; Tranfield and Starkey 1998). And prominent academics wonder out loud what would happen if the Academy of Management 'really mattered' (Hambrick 1994). The malaise that these concerns suggest is also present in the closely related field of sociology, where an entire special issue of *Sociological*

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Forum was devoted to the theme: 'What's wrong with sociology?' The answer of the special issue contributors seemed to be just about everything (see, for example, Collins 1994; Davis 1994; Lipset 1994).

This essay suggests that the excitement and the malaise referred to above may stem, at least in part, from the same source: a Mertonian displacement of ends (Merton 1940) in organization theory that has changed new theory development from a means to further ends into the ultimate goal of the discipline (*Academy of Management Journal* 2007; Smith and Hitt 2005). Merton (1940) developed the concept of displacement of ends to explain what happens when rules designed to contribute to organizational goals become reified as goals themselves. Though Merton's (1940) focus was on bureaucracies, he was at pains to emphasize that displacement of ends is a human activity that also occurs outside the boundaries of bureaucracies.

I argue that in organization theory during the 1960s and 1970s the goal of investigators, at least implicitly, was a broader one than the current preoccupation with new theory development. Specifically, the goal was the production of a consensus about the validity or invalidity of the theories that were offered at that time to represent organizations. Theory development was an important means to achieving that goal (see, for example, Pugh et al. 1963), but it was not the only activity deemed necessary for accomplishing it (Donaldson 1997). In addition, the production of standardized measures for operationalizing variables contained in the theories was considered important, as were empirical tests of the theories and replications of those tests. These activities—proposition of new theories, development of standard measuring instruments, testing of theories, and replication of those tests—were undertaken in streams of literature designed to pursue the ultimate goal of developing a consensus among scholars about a particular theory's validity or invalidity.

In contemporary organization theory, by contrast, there has arguably been a displacement of ends in which new theory development has emerged as the ultimate end and the goal of consensus about the validity status of theory has become submerged. In some ways this represents a retreat from a more complex research endeavor to a simpler one, because the empirical activities of instrumental standardization, empirical testing of existing theories, and replication of those tests have been relegated to a position of second priority relative to new theory development (see, for example, Eden 2004; Neuliep and Crandall 1991). This is comparable to the movement in publicly traded corporations away from the goal of 'effectiveness' toward the unidimensional performance criterion of shareholder value (Jensen 2002). Unfortunately, the prioritization of new theory development has led to a proliferation of untested theories and a condition of theoretical fragmentation that accounts, in part, for the difficulty of applying organization theory to practice and the unease that many organization theorists feel about the worth of their discipline.

This essay elaborates the argument summarized above, illustrating the thesis by discussing the research streams on dimensions of organization structure and contextual predictors of organization structure (e.g. Pugh et al. 1963; Pugh et al. 1968). Using this literature as a comparative base, I make the case that contemporary organization theory has experienced a displacement of ends in which new theory development has been elevated from a means into an ultimate end of the discipline.

Though I am generally critical of this displacement of ends, I discuss some advantages that it may have for organization theory, including the production of an array of new theories that stimulate interesting discourses and illuminate aspects of organizations that were not foci of the theories of the 1960s and 1970s. Despite these potential advantages, the displacement of ends toward new theory development also has disadvantages, and these will also be discussed. These disadvantages include the aforementioned proliferation of untested theories, a dearth of replications of existing empirical work, and a reduced capacity for producing knowledge utilizable by practitioners. After detailing these disadvantages, I will propose some possible remedies for them, including, among others, the creation of incentives for developing standardized measuring instruments (McKinley 2007) and the rejuvenation of the value of replication. These remedies are designed to reallocate priorities in the discipline of organization theory, signaling to producers of research that empirical testing and replication studies of existing theories are as important as the development of new theories. This reallocation of priorities would restore new theory development to the status of means—albeit an important one—to the ultimate end of achieving consensus on the validity status of theories.

The Goal of Consensus on Validity Status

A close look at the organization theory literature of the 1960s and 1970s suggests that at that time the discipline was more focused than it is today on the ultimate goal of developing a consensus about the validity status of the theories that were being proposed to represent organizational phenomena. The evidence for the existence of this goal is indirect, stemming not so much from the statements of the organization theorists of the period as from the content and pattern of their publications. Despite the indirect nature of the evidence, I believe a compelling case can be made that researchers of the period were motivated by the objective of creating an empirically based consensus about the validity status of the theories that were current at that time. I will document this case by a brief look at the evolution of the organization theory literature from the 1960s through the end of the 1970s.

In the 1960s and the early 1970s much of the 'attention space' (Collins 1998) in organization theory was devoted to the study of organizational structure. This attention to structure owed its origins to Weber's (1946, 1947) ideal-type description of the characteristics of bureaucracy. This ideal-type description was used by several theorists (e.g. Hage 1965; Hall 1963; Udy 1959) as the basis of a dimensionalization project that sought to convert the attributes of Weber's ideal-type scheme into structural dimensions whose scores varied across organizations. Through this project, new research questions were opened up, including the issue of whether or not the dimensions covaried with one another, and what contextual predictors explained variance in individual structural dimensions.

In 1963 the dimensionalization project got a major boost with the publication of the first article in the Aston Group research program (Pugh et al. 1963). This theoretical paper proposed the existence of six dimensions of organization structure: specialization, standardization, formalization, centralization, configuration, and flexibility. The article was clearly intended not as a stand-alone contribution, but as a platform for a program of empirical investigation that would begin with the operationalization of the dimensions described in the article. Pugh et al. (1963: 315) stated that 'by setting up empirically defined scales for these, clear comparisons can be made between organizations. The result will be a typology based on empirical generalizations.'

At this point the Pugh et al. (1963) article, along with the work of Udy, Hall, and Hage, had developed a new theory that represented organizations as multidimensional entities (see also Hinings et al. 1967). Pugh et al. (1968) then sought to create a set of standard instruments that would measure the Pugh et al. (1963) structural dimensions and permit empirical research about them. Pugh et al. (1968) used data from a diverse sample of 46 organizations in the Birmingham, England, area to generate a complex set of scales, and those scales were then reduced by factor analysis to four underlying dimensions, labeled structuring of activities, concentration of authority, line control of workflow, and relative size of supportive component. Later, Pugh et al. (1969) used the same data set to define measures of several attributes of organizational context, including organizational size, technology, and dependence. Pugh et al. (1969) correlated these contextual variables with the measures of organizational structure described in Pugh et al. (1968). Pugh et al. (1969) found that organizational size, organizational dependence, and the 'charter-technology-location nexus' were the main predictors of variance in their structural dimensions.

It seems unlikely that Pugh et al. (1968, 1969) would have undertaken the extensive instrumentation project reported in their papers if they had not had as their goal the development of a consensus about the validity status of the multidimensional profile proposed in Pugh et al. (1963). The extensive empirical documentation that Pugh and co-authors offered for the scales described in Pugh et al. (1968, 1969) strongly suggests that these scales were intended for diffusion to other colleagues who would then participate in the production of this consensus. Apparently those colleagues saw things the same way, because the work of Pugh et al. (1963, 1968, 1969) was almost immediately subjected to an extensive set of replications, re-examinations, and re-assessments using the same instruments that Pugh and his co-authors had developed (see Child 1972; Inkson et al. 1970; Hinings and Lee 1971; Reimann 1973; Donaldson et al. 1975; Greenwood and Hinings 1976). These replications and re-examinations sought to ascertain whether the Pugh et al. (1968, 1969) results generalized to a variety of different organizational populations and thus whether a consensus could be achieved about their validity. The theoretical framework produced by Pugh et al. (1963) operated as a guide for this replication stream, but the goal was not development of new theory so much as a consensus about the existence of the dimensions and relationships reported in the original Pugh et al. work. In fact, theoretical innovations appear to have been avoided. This is a pattern typical of normal science (Kuhn 1970). It is noteworthy that these replications and re-examinations were published in the top organization studies and sociology journals of the time, including *Administrative Science Quarterly*, *Sociology*, and the *Academy of Management Journal*. This is a measure of the perceived importance of the replications in organization theory during this period.

One important feature of this replication stream was an anomaly that Child (1972) detected in his attempt to reproduce the relationships between dimensions of structure that Pugh et al. (1968) had found. While Pugh et al. (1968) had reported, on the basis of factor analysis, that the dimensions of structuring of activities and concentration of authority were independent, Child's (1972) national British sample showed a negative correlation between these two dimensions. Child (1972) attributed this anomaly to differences in sample composition between his data set and that of Pugh et al. (1968), but he also offered a logical explanation for the finding. Child (1972) argued that the structuring of activities, and particularly the impersonal rules and job descriptions that were an important component of this structural dimension, created a mechanism of impersonal control that reduced managers' perceptions of risk in delegating decision making authority to lower levels of an organization's hierarchy. Thus when activities were highly structured through impersonal control mechanisms, concentration of authority was lower. Child (1972) proposed that the negative relationship between structuring of activities and concentration of authority was consistent with the original Weberian description of bureaucracy, which envisioned progressive delegation of authority down a hierarchy composed of specialized administrators.

The anomaly reported by Child (1972) set off a flurry of activity designed to ascertain whether the original Pugh et al. (1968) claim or the contrarian position offered by Child (1972) was valid. Put slightly differently, an effort was launched to determine whether Child's (1972) result was a generalizable finding worthy of consensus, or whether it was an anomaly confined to samples of organizations with particular characteristics. First Mansfield (1973) classified the data in Child's (1972) National sample into six size bands to see whether size might influence the magnitude of the relationships between centralization (concentration of authority) and the variables underlying the Aston structuring of activities dimension. Mansfield (1973) concluded that size did not affect the strength of the relationships, and that centralization had a uniform (albeit weak) negative correlation with structuring of activities. Next Donaldson et al. (1975) collaborated on a three-part research note, returning to Child's (1972) suggestion that differences in sample composition between his data set and that of the Aston Group might account for the difference in findings. Classifications of the Aston Group data into various subsets designed to reproduce the distinctions between the Aston and National samples yielded the overall conclusion that the original Aston results (independence of concentration of authority and structuring of activities) still held. Thus the anomalous difference between the Pugh et al. (1968) and Child (1972) findings was not resolved. Reflecting the urgency to achieve consensus about which of the two competing positions was valid, Aldrich (Donaldson et al. 1975: 459) encouraged 'all hands to get back to the data and look this question over a little more carefully'.

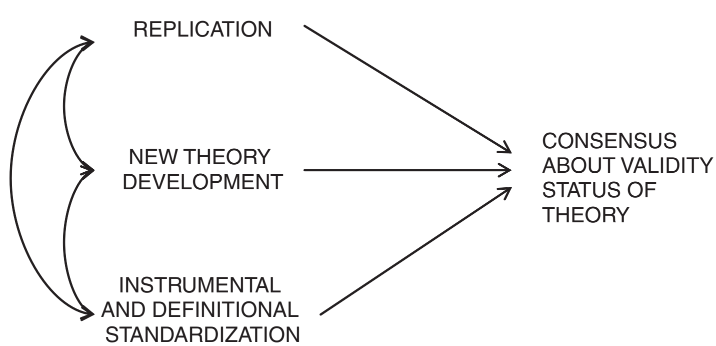
Subsequently, Greenwood and Hinings (1976) addressed the inconsistency between Pugh et al. (1968) and Child (1972) by presenting new data from a study of departments of English local authorities. These data revealed no significant relationships between subscales of standardization and subscales of centralization.

These findings were not supportive of Child's (1972) hypothesis that structuring of authority would provide a control mechanism that permitted decentralization of decision making to lower levels of an organization's hierarchy. Greenwood and Hinings (1976) did not present their results as a conclusive refutation of the Child (1972) hypothesis, however.

While the stream of work outlined above did not result in a conclusive consensus about whether Child's (1972) or Pugh et al.'s (1968) position on the relationship between structuring of activities and concentration of authority was correct, the point is that considerable effort appears to have been devoted by a significant number of researchers to the goal of generating such a consensus. This involved extensive instrumentation, data collection, data analysis, and replication, with the cooperation of well-regarded journals and (presumably) the allocation of much editorial and reviewer time to the adjudication of the controversy. During the same period in the history of organization theory equivalent effort was made to come to a consensus about whether or not the relationship between technology and structure reported by Woodward (1958) was generally valid (see, among many examples, Donaldson 1976; Hickson et al. 1969; Blau et al. 1976; Marsh and Mannari 1980). A similar focus was present in attempts to validate Blau's (1970) formal theory of differentiation in organizations (e.g. Mileti et al. 1977; Miller and Conaty 1980) and to achieve a consensus about whether the size-administrative intensity relationship was influenced by definitional dependency (Feinberg and Trotta 1984; Freeman and Kronenfeld 1973; Kasarda and Nolan 1979; MacMillan and Daft 1979).

In summary, I maintain that the field of organization theory in the 1960s and 1970s can be represented by the diagram in Figure 1. When a new theory was proposed, replications were routinely conducted in order to test the theory and attempt to develop a consensus among specialists about whether or not the theory could be considered valid. The replications attempted to deploy standard instrumentation across a variety of different organizational populations. The goal was not new theory development *per se*, but rather theory testing in the service of consensus about theory validity or invalidity. Theory development, instrumentation, and replication functioned as means to this end, and were interactive, as indicated by the double-headed arrows at the left side of Figure 1. The overall motivation of the researchers carrying out these complicated, interdependent activities was to come to a conclusion about the validity status of the theory of concern.

Figure 1.
Organization Theory in the 1960s and 1970s



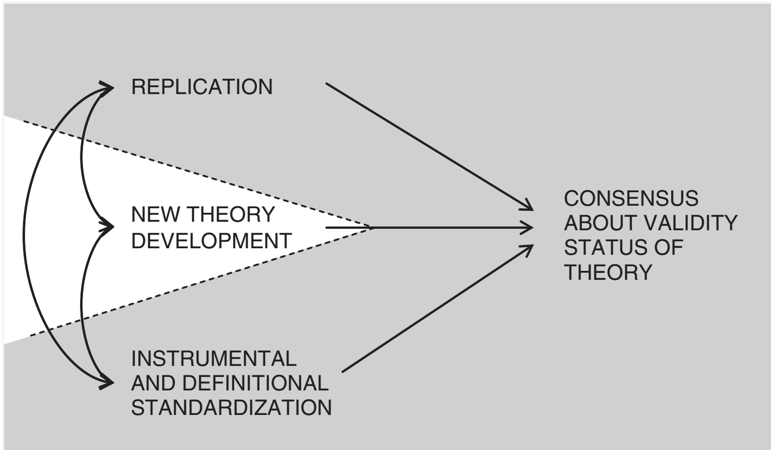
While my inferences about the motivation of organization theorists at this time are indirect, as noted at the beginning of this section, my argument takes on additional credibility given Donaldson's (1997) remarks about the philosophy of science espoused by Derek Pugh. In discussing the development of the Aston Group's research stream, Donaldson (1997) noted that Pugh was skeptical about theory and did not value theory for its own sake. Assuming that skepticism was shared by some other organization theorists of the period, it reinforces the conclusion that new theory development played a subsidiary role to theory validation and the attempt to generalize validation beyond the original site of a given finding (e.g. the Pugh et al. Birmingham sample) to other organizational populations. In this process, theory development was a means to the end of consensus on the validity status of theory, rather than an end in itself.

Contemporary Organization Theory: Displacement of Ends

Evidence abounds that the role of theory development has changed in contemporary organization theory—it is no longer seen as a means to the end of achieving consensus about the validity status of theory, but rather as an end in itself. One early manifestation of this shift was the establishment of the *Academy of Management Review* (*AMR*) in 1976. *AMR* provided a forum for the presentation of new theory, and to my knowledge, no requirement or suggestion was specified by the journal that that theory be tested. This is not intended as a criticism of *AMR*—the journal has been an important addition to the field of organization theory, and has published many novel and interesting articles over the past 30 years. The point is simply that *AMR* gave new theory development, shorn of subsequent instrumental standardization, empirical testing, or replication, a legitimate status in its own right.

Further evidence of the importance of new theory development as an end in itself in contemporary organization theory comes from two special issues of *AMR* (in 1989 and 1999) focused on the phenomenon of theory development and how to improve it. In these issues theory seems to be considered as worthy of development in its own right, rather than as a vehicle for the achievement of further ends. Mirroring this orientation, Weick (1999: 797), in an epilogue to the second of the special issues, noted that 'theorizing in organizational studies has taken on a life of its own in the last 10 years'. In 1995, the *Administrative Science Quarterly* (*ASQ*) also published a well-received forum on theory development, in which the prominent theorists Sutton and Staw (1995), Weick (1995a), and DiMaggio (1995) debated what theory is and what it is not. These discussions are noteworthy for their implicit assumption that theory is worth creating for its own sake, not as a route to empirically based consensus about the validity status of the theory. Finally, Smith and Hitt (2005: 587), in an epilogue to an edited volume in which distinguished organization theorists reflected on the development of their theories, stated the hope 'that [through this volume] more scholars will be better prepared to develop new theory. We hope that by understanding the different processes, roles, and characteristics, it will inspire and help more of us to advance theory and our profession.' Smith and Hitt (2005: 587) ended their epilogue with a quotation from John Lancaster Spalding: 'Your faith is what you

Figure 2. Contemporary Organization Theory



believe, not what you know.' These sentiments, though well-intentioned, reinforce the impression that theory development has gained an institutionalized status as an end in itself in contemporary organization theory.

The transition of theory development from means to end is represented in Figure 2. In Figure 2, the segment of the field's activities associated with new theory development has been highlighted to symbolize the perceived importance of this activity in present-day organizational scholarship. The shading covering the instrumentation and replication activities that were prominent in the streams of research cited above represents the argument that these activities have assumed a subordinate status in organization theory today (see Hambrick 2007). Indeed, McKinley (2007: 135) has noted that standard instrumentation is not a high priority in organization theory currently, arguing that 'the normative climate that dominates organization studies and governs what type of research will be considered 'good' is not very hospitable to standard instrumentation efforts.' Also, Hubbard et al. (1998) have reported that only about five percent of the articles currently published in management journals are replication studies, and Neuliep and Crandall (1991) have documented an editorial bias against publishing replications in social and behavioral science journals. It is also worth noting that while protected niches (e.g. *AMR*) exist for papers whose purpose is to espouse new theory, there is no comparable protected niche for papers whose objective is to validate an already existing theory, replicate such validations, or present instrumentation for constructs already existing in the literature. Arguably, the Research Notes section of the *Academy of Management Journal* formerly played this role, but that section has been eliminated in recent volumes of *AMJ*. In summary, the epistemological literature examining the nature of management and organization theory scholarship, and also the current configuration of the discipline's distribution channels, both lead to the conclusion that theory development is currently prioritized as scholarly 'king of the hill'.

The dominant status of theory development in today's organization theory is further reinforced by a skeptical attitude toward objectivity that has arisen in the last two decades in much organization theory scholarship. This is important because at least a minimum degree of belief in objectivity is necessary to motivate efforts to develop empirically based consensus about theory validity or invalidity. Prominent commentators like Astley (1985) have argued that organization theory is a fundamentally subjective enterprise in which theory functions as a social construction rather than a representation of an underlying empirical reality. To the extent that this type of argument is accepted by contemporary organization theorists, it removes much of the incentive to embark on validation exercises such as those undertaken by researchers attempting to reproduce the Aston Group's results and those of other organizational scholars working in the 1960s and 1970s. Instead, theory seems to have become a narrative that dominates attention for its interest and uniqueness (Davis 1971; Mone and McKinley 1993), while replication and instrumentation of constructs have faded into the background of the attention space (Collins 1998). This situation is further supported by *prescriptions* for subjectivity, such as Case's (2003) advocacy of 'subjective authenticity' as the appropriate standard for good organizational scholarship. All this encourages a displacement of ends from empirical consensus on the validity status of theory to new theory development, as the role of theory changes from provisional representation needing repeated verification to story generator.

New theories published in empirical journals are often accompanied by confirmatory empirical results, but in most cases those results are produced by the authors of the theory. Motives for self-enhancement, which most humans share (Pfeffer and Fong 2005) would suggest that authors of theories would have an incentive to interpret the results they present in a theory-supportive light. This is not the same as having one's theory independently tested by researchers who are not originators of the theory. Indeed, Rosenthal (1991) has suggested that replications are more valuable to the extent that they are removed from the experiment being replicated in terms of physical distance, personal characteristics of the replicators, and contact between the replicators and the original experimenters. This is consistent with the logic that empirical tests of a theory offered by the authors of the theory should not be considered final evaluations of the theory. The presentation of novel theories with confirmatory results but without follow-up replication, a common pattern in today's organization theory, seems symptomatic of the dominance of new theory development—rather than consensus about a theory's validity status—as the overriding goal.

Theory Development as End: Advantages

Assuming my argument about the displacement of ends toward new theory development has some credibility, what are the consequences for organization theory? Are there any advantages of this shift? As stated earlier in the paper, one possible advantage is the evolution of a wide array of novel theories that capture aspects of organizations that were not foci of attention in the 1960s and 1970s. Organization theory has indeed witnessed a tremendous expansion in the number of theoretical schools included within its boundaries (McKinley et al. 1999; Pfeffer 1993). Theory-based schools of thought now exist that offer narratives about managerial cognition, institutional fields, transaction costs, power and dependence, institutional entrepreneurship, and many other topics that transcend the narrower focus on structural dimensions that dominated 30 years ago. These theories are engaging, and even sometimes exciting, although the empirical support offered for them often seems to be directed more toward legitimizing the theory than toward facilitating additional empirical testing and producing a discipline-wide consensus about whether or not a given theory is valid. There may be little incentive to work toward such a consensus because such concerns would detract from the time available to generate new theory and therefore satisfy the publication criteria of major journals in the field (*Academy of Management Journal* 2007; *Administrative Science Quarterly* 2007). In summary, displacement of ends toward the goal of new theory development has brought excitement, novelty, and less attention to organized empirical verification work.

At the same time, the emphasis on theory development as end has probably had the effect of engendering rapid theoretical transitions that are helpful in keeping up with the 'relentlessly shifting' organizations many argue are a prominent feature of today's organizational landscape (e.g. Brown and Eisenhardt 1997; Volberda 1996). While there is debate about whether task environments are becoming more dynamic (e.g. Castrogiovanni 2002), there is considerable anecdotal evidence that new organizational forms are evolving rapidly in the global economy we face today. Examples of such forms include disintegrated supply chains based on the principle of outsourcing, and various types of transnational organizations (see Walsh et al. 2006 for a longer list). In a field like organization theory that currently emphasizes theory development and de-emphasizes large-scale theory validation and replication, new theories can be quickly introduced to explain new forms (e.g. Powell et al.'s (1996) work on R&D alliances). Theory development as end promotes the flexibility of new theory development, and helps ensure that theory development matches the speed with which new organizational forms emerge on the scene. Here the ultimate test of good theory is not so much whether it is acknowledged as valid by a majority of organization theorists, but whether it is plausible (Weick 1995b) and above all, relevant to the moment.

There are occasional complaints that our theories are out of date (e.g. Daft and Lewin 1993; Walsh et al. 2006), but the discipline's emphasis on theory development as an end in itself may actually mitigate that problem. The same dynamics that apply in the market for fashionable change programs originated by consultants (Abrahamson 1996; Kieser 1997) may also apply in the market for new academic theories about organizations (McKinley 1996), leading to relatively frequent shifts in existing theoretical frameworks. Examples of those shifts include the restructuring of population ecology's central construct of inertia (compare Hannan and Freeman 1977 and Hannan and Freeman 1984), and the subsequent reorientation of population ecology away from environmental selection to density dependence (Hannan and Carroll 1992). Neo-institutional theory has also undergone many reorientations during its history, most dramatically from a theory that questioned managerial rationality (Meyer and Rowan 1977) to a theory that assumes rational 'institutional entrepreneurs' actively seeking to change institutions (e.g. Lawrence 1999; Lawrence and Suddaby 2006). These shifts are symptomatic of the importance of positioning a perspective at the cutting edge of new theory development in today's discipline of organization theory.

Theory Development as End: Disadvantages

While the emergence of theory development as an end in itself may have the advantages outlined above, it also has significant disadvantages. For one thing, theory development as end tends to encourage the accumulation of theories that have never been tested at all. Of course not all new theories fit this category but some do, especially those that are published in such journals as *AMR*. Again, without intending any critique of *AMR*, I simply note Hambrick's (2007) observation (citing Kacmar and Whitfield 2000), that only nine percent of the theoretical presentations in *AMR* articles have ever been tested. Researchers may perceive, with some justification, that testing a pre-existing theory without contributing novel theory of their own is a recipe for disaster in the review process (Eden 2004; Hambrick 2007; Sitkin 2007). Locke and Latham (2005: 147) hinted at the problem discussed here when they stated that '…the history of science…has implications for the *Academy of Management Review*. We encourage the editorial staff to discourage hypothetico-deductive theorizing and to promote more inductive theorizing'.

The elevation of theory development from means to end also reduces the diversity of intellectual work in organization theory, focusing both theoreticians and empirical researchers on new theory development and leaving less room for a division of labor between these two groups. Indeed, it is interesting that a well-established division of labor between theorists and empirical researchers, such as that existing in high-energy physics, for example, has not evolved in contemporary organization theory (see Knorr Cetina 1999 for an extended epistemological analysis of high-energy physics). In contrast with the theoretical diversity one finds in current organization theory, there is an epistemological uniformity, in which both theorists and empirical researchers are trying to develop new theory, and neglecting tests of existing theory (Hambrick 2007). Ironically, this epistemological uniformity encourages the theoretical fragmentation of the discipline, because most empirical research projects are self-contained exercises directed toward the deductive or inductive building of new theory. These self-contained theoretical 'islands' often do not lead to integrated streams of empirical research that would provide a source of continuity in the discipline. This picture of theoretical fragmentation is confirmed by Gerald Davis's report, described in Walsh et al. (2006), of the theoretical dispersion of topics in submissions to the 2005 Organization and Management Theory Division program of the Academy of Management.

The transformation of theory development from means to end also has implications for our ability to advise practitioners: it suggests that we may be confined to the symbolic application of knowledge that was identified by Pelz (1978), while being prevented from implementing the instrumental and conceptual uses he also discussed. Astley (1985; Astley and Zammuto 1992) referred to such symbolic uses of knowledge when they argued that the language of organization science can be employed by managers to unify their organizations politically. While symbolic or political uses of organization theory are valuable to practicing managers, the goal of using management knowledge as an instrumental lever (Cheng and McKinley 1983; Pelz 1978) is also worthy of attention. Yet practicing managers are unlikely to adopt management knowledge for instrumental purposes unless it has been validated by first empirical tests and subsequent replications, and has attracted the consensus of a majority of experts in the appropriate specialty area. Arguably the emphasis on theory development as an end undercuts that possibility by making empirical replication unfashionable (Hambrick 2007; Neuliep and Crandall 1991).

I think we sometimes underestimate the chaotic appearance that organization theory must present to the world of management practice, because of the array of disconnected theories in the field and the lack of an overarching consensus about the relative validity of those theories. For many scholars within the discipline this situation constitutes a desirable state of intellectual freedom and flexibility; but does the practicing manager have the same view? In my opinion the answer is 'probably not'. This assessment is consistent with a recent forum published in *AMJ* on evidence-based management in the human resources specialty. The articles in the forum (see, for example, Cohen 2007; Rynes et al. 2007) indicate that practitioners do not find academic research in human resource management very useful. The same would appear to be true in the macro disciplines of organization theory and strategy (e.g. Hambrick 1994). While there are many possible reasons for this 'research–practice gap' in organization studies, one may be the emphasis on theory as end that obtains in much of the discipline, and therefore the proliferation of new theory without an accompanying effort to develop independent tests of each theory.

Thus, the wide array of idiosyncratic and untested theories in the domain of macro-organization studies likely makes the field confusing to practicing managers, fostering the impression that the discipline has little to offer them. Because of the information overload they typically experience, most managers are attracted to simplicity (Miller 1993), and contemporary organization theory does not offer the appearance (or the reality) of theoretical simplicity. Rather than trying to decipher organization theory, it may be more appealing to managers to simply bow to whatever wave of isomorphism in change management programs is sweeping their industry at the moment (DiMaggio and Powell 1983; Abrahamson 1996).

Admittedly my inferences about how managers perceive our field, if they perceive it at all, are open to debate. In addition to developing new theories about organizational phenomena, we should also be conducting empirical investigations of how managers perceive us and subject those investigations to replication. Such a program might allow us to achieve some consensus about the validity of suggestions that the shift to theory development as an end has influenced the way managers perceive organization theory.

What Is To Be Done?

As Stinchcombe (1994) put it succinctly in his article in the aforementioned special issue of *Sociological Forum*: what is to be done? He was referring, of course, to the state of sociology, but I unashamedly pilfer his section title to raise the same question concerning what might be done about our own practice in organizational scholarship, and specifically about the disadvantages of our current (over)emphasis on new theory development. As I suggested in the introduction, a first step toward remedying these disadvantages might be to increase the value placed on the shaded areas in Figure 2, including instrumental and definitional standardization, as well as replication of first empirical tests across new populations and industries (Hubbard et al. 1998; McKinley and Mone 1998). This would reallocate priorities and resources back toward attempts to achieve consensus on the validity status of theories, and away from theory development for its own sake. In order to accomplish these goals, the Academy of Management might begin by restoring the Research Notes section of *AMJ* and explicitly dedicating it to papers that present new or improved instrumentation for existing constructs, that empirically evaluate theories published in the pages of *AMR*, or that replicate such tests. This would create a protected niche for theory validation that would balance the protected niche for theory development already established at *AMR*. European journals such as *Organization Studies* (*OS*) or *Journal of Management Studies* (*JMS*) might also initiate 'instrumentation and validation' sections. Contra Hambrick (2007), I do not suggest reconfiguring second-tier journals as outlets for this theory validation work. Such reconfiguration would merely perpetuate the status quo, in which such work is viewed as low in prestige and priority.

A side benefit of the policy shift I am recommending would be to tighten the linkage between the aforementioned journals and *AMR*, so that together this collection of journals could function in a more integrated fashion as a theory proposal and evaluation device. I do not advocate that no new theories be published in empirical articles, but on the other hand the current policy that every paper published in a journal like *AMJ* must contribute to new theory development seems to be a contributing factor to our current state of theoretical disintegration (see Eden 2004). Revitalizing the Research Notes section of *AMJ* or beginning similar sections for such journals as *OS*, *JMS*, and the *British Journal of Management* would send a strong signal to the field that the disciplinary elite value efforts to generalize existing theories beyond their point of origin to multiple empirical populations. This in turn might help shift the balance back toward the empirical consensus-building efforts that I argue were more prominent in 1960s and 1970s organization theory.

Also, journals like *AMJ*, *Strategic Management Journal*, *Journal of Management*, *JMS*, *ASQ*, *OS*, and *Organization Science* could offer expedited review to instrumentation studies, first empirical tests of previously published theories, or first replications of such tests. These instrumentation studies, first empirical tests, and replications should be held to the same rigorous review standards that currently apply to empirical papers that present original theory, but a fast-track review process would signal to authors that there is a market for such work, while offering strong incentives to do the work and submit it. Such a fast-track review policy might require the dedication of additional resources to the review process, but the results might be worth it. This approach could speed up empirical assessment of theories to a level that would match the rate of production of new theory. Many new theories are plausible explanatory devices, but many of them are also likely to be less than fully accurate representations of the phenomena they are explaining. This is because several competing explanations are usually possible for a given phenomenon—think about accounting for the existence of water in the streets—although not all of the explanations will be valid. Matching the rate of assessment of equally plausible theoretical explanations to the rate of production of those explanations would have the salutary effect of winnowing out the less accurate explanations.

Most radically, I suggest that the Academy of Management might think about changing the mission of *AMR*. Specifically, the Academy could open up *AMR* to empirical articles that present tests of theories that have previously been published in *AMR*. This would widen the niche available for incremental hypothesis testing research and increase the number of pages devoted to the empirical groundwork necessary to converge around more valid theories. A section of *AMR* devoted to short research notes testing propositions that have previously appeared in *AMR* would also strengthen the relationship between the theory development and theory assessment components of organization theory. At the same time, it would encourage a division of labor between theory developers and theory evaluators, thus producing a more skeptical evaluation process that is less subject to self-enhancement motives (Pfeffer and Fong 2005). If theory is to do more than generate interesting conversations, it needs to be evaluated by comparison with the objective or constructed reality it purports to represent, so that a consensus can be built about the representational worth of the theory. Opening up *AMR* to empirical tests of theories previously published there might facilitate this goal.

If multi-study empirical evaluations of existing theories can be developed by empirical researchers who are not affiliated with the originators of the theories, we may begin to see more consensus about which theories are valid and which are less so. It is important to emphasize that a consensus is never likely to be total, so we should become sensitive to relative degrees of consensus in empirically assessing the array of new theories with which we are confronted. Theories attracting relatively high consensus from independent empirical researchers who have generated and examined the evidence for them should be preferred over theories attracting relatively low consensus.

Theories that have garnered relatively high levels of consensus might then be distributed to practitioners through a channel specifically engineered for the needs of its audience. The *Academy of Management Executive* was intended to serve that purpose, but it is no longer published by the Academy of Management. The *Academy of Management Perspectives* has replaced it, but it is unclear whether that journal is targeted primarily at an audience of university instructors or an audience of practicing managers. The available evidence (e.g. Cohen 2007; Guest 2007; Saari 2007) suggests that any distribution channel to practitioners would need to include a mechanism for translating consensus-backed theories into a form that is digestible by busy managers who have little time for extensive reading. Nevertheless, given appropriate translation, consensus-based organization theory might prove useful to such managers, particularly if it offers 'levers' that can be manipulated to produce desired outcomes (Cheng and McKinley 1983) relatively reliably. Most of all, I think managers would value knowledge that moves beyond plausibility to approach a consensus about validity. The managers already have plenty of plausible theories that have been produced by consulting firms operating in the market for management fashion (Abrahamson 1996; Armbruster and Gluckler 2007; Kieser 1997; Sorge and van Witteloostuijn 2007). Our competitive advantage as organization theorists is to offer this audience a knowledge product that has been rigorously tested and agreed on by a number of independent specialists. The evidence-based management movement (e.g. Rynes et al. 2007; Rousseau 2006) is a good start in this direction, and hopefully the suggestions made in this section will contribute to that movement.

An Agenda for Future Research

While my argument about the displacement of ends toward new theory development has some implications for how we might change our practice in future organization theory scholarship, particularly to enhance utilization of our knowledge by managers, the argument also suggests some possible areas for future empirical research in the history and sociology of organization science. First, it would be interesting to trace the shift of scholarly values underlying the elevation of new theory development from means to end, and therefore empirically assess the validity of my claims in this article. This might be done by content analysis of published statements by editors and editorial board members, and of chapters in edited collections like *Publishing in the Organizational Sciences* (Cummings and Frost 1995). *Notice to Contributors* sections of journals also provide clues to the values that underlie knowledge assessment (and therefore production) in organization theory. Mone and McKinley (1993) examined such material to document the development of a 'uniqueness value' in organization studies, and similar techniques, appropriately refined by recent advances in content analysis (e.g. Golden-Biddle et al. 2006; Locke and Golden-Biddle 1997), might be applied to studying the displacement of ends toward new theory development.

If empirical evidence from content analysis indicates that new theory development has indeed acquired a more valued status than it had in organization theory 30 years ago, historical researchers might inquire into the reasons for this shift. One plausible theory, as already suggested, is simply a change in the reward structure of the field. New theory development may have come to prominence in organization theory because that is what is perceived as desirable by gatekeepers in the discipline. On this historical theory, authors have responded to perceived shifts in what journal editors and editorial boards want, and the novel theories the authors have produced have affirmed the value of such production in the eyes of the gatekeepers. Those gatekeepers have then responded by making new theory development a more formal requirement, institutionalizing it by recording it in policy statements as a necessary attribute for publishable contributions in their journals (*Academy of Management Journal* 2007; *Administrative Science Quarterly* 2007). On this account, the displacement of ends toward new theory development is an iterative, self-reinforcing interaction between the evolution of a new reward structure and the actions of the individuals subject to that reward structure.

Another possible explanation, moving beyond reward structures and the personal interests of organization theory journal authors, is the development of a new epistemological paradigm in the discipline. According to this historical scenario, which was hinted at earlier, new theory development has emerged as an end in itself because of shifts in more general epistemological norms about what constitutes desirable knowledge. For example, the interpretive paradigm described by Burrell and Morgan (1979), while not dominant at the time their book was published, may have become more so in the last 30 years. The epistemological position espoused by this paradigm is anti-positivist, a position that 'tends to reject the notion that science can generate objective knowledge of any kind' (Burrell and Morgan 1979: 5). This is consistent with Astley's (1985) argument that organization theory does not offer an objective representation of an underlying empirical reality, but is rather a social construction. Such epistemological beliefs would support a transition away from consensus on the validity status of theory as the ultimate goal, helping rationalize the creative act of new theory generation as the ultimate end. Historical researchers might attempt to develop measures that would tap the emergence of epistemological beliefs such as those associated with the interpretive paradigm, using content analysis or citation counts as indices of those beliefs. Relating those measures to the incidence of statements by commentators extolling the priority of new theory development might permit an empirical assessment of the causal role of epistemology in the displacement of ends toward new theory development. I believe that research on this linkage would help us understand changes in the norms that govern acceptable contribution in our discipline. Such comprehension could help us assess the norms from both descriptive and prescriptive viewpoints, asking how they came into being and also whether they are optimal for production of knowledge and for evidence-based recommendations to managers.

Conclusion

This paper began with the argument that since the 1960s and 1970s organization theory has experienced a displacement of ends in which the ultimate goal of building empirically based consensus about the validity status of theory has been replaced by the goal of new theory development. The latter activity, once taken as a means to the aforementioned consensus, has been elevated into an end in itself. I have acknowledged that this displacement of ends may have certain advantages for organization theory, including the production of a flexible, quickly evolving body of theory that explains a wide variety of novel organizational phenomena. But I have also argued that the displacement of ends toward new theory development has disadvantages. Among those disadvantages are an accumulation of untested theories and a disorganized 'presentation of self' that may be affecting managers' perceptions of our discipline and our ability to produce knowledge that is useful to practitioners on instrumental grounds (Pelz 1978; Rousseau 2006; Rynes et al. 2007).

I have recommended some policy initiatives that the Academy of Management, European journals, and the organization studies community in general might take to reduce the negative consequences of the displacement of ends toward new theory development, without disrupting the theoretical flexibility and intellectual novelty that have been by-products of this displacement. These recommendations involve rejuvenation of emphasis on incremental theory testing and replication, and a reallocation of resources toward such efforts. This shift could be fostered by setting aside protected niches for pure empirical tests and replications of such tests in prominent management journals, including *AMR*. In my view editorial policy at major organization studies journals should also favor instrumental standardization (McKinley 2007) and the conduct of theory tests and replications by scholars who are not affiliated with the originators of the theory being assessed. Such policies would be valuable for the signal they send to scholars, and I hope that they would ultimately help rehabilitate consensus on the validity status of theories as the ultimate goal of our discipline. I believe such a rehabilitation would be a radical regime change, but it is important to stress that it would be a regime *change*, rather than an imposition of an undesirable intellectual regime where today there is none. To believe that contemporary organization theory has no epistemological paradigm may be short-sighted.

Finally, I have argued that the displacement of ends toward new theory development poses an opportunity for research in the history of organization science. To call on Merton again, the first step would be to do what he (Merton 1987) called ‘establishing the phenomenon’. That is, empirical researchers should use available content analysis methods to assess my claim that the displacement of ends toward new theory development has actually taken place. Assuming evidence is found to back up this claim, researchers might seek to evaluate the explanations that were offered above for this shift. The explanations—change in the reward structure, change in organization theory’s epistemological paradigm, or a combination of these—should be rigorously examined. The examination should not be considered complete after the first empirical test—repeated testing and replication should take place to converge on the explanation that has greater empirical validity. In this way, the empirical investigation would become a self-exemplifying illustration of the type of research advocated in this paper. If applied across several studies, this style of research might facilitate some consensus about historical transitions in the normative structure and the epistemology of organization theory, and might even re-establish convergence on the validity status of theory as the ultimate objective toward which our discipline should strive.

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William McKinley

William McKinley received his PhD in organizational sociology from Columbia University, and is currently Rehn Professor of Management at Southern Illinois University at Carbondale. His research interests are organizational restructuring and downsizing, organizational change, organizational decline, epistemological issues in organizational research, and the history, sociology, and philosophy of organization science. He currently serves as Senior Editor of *Organization Studies*.
Address: Department of Management, Southern Illinois University at Carbondale, Carbondale, IL 62901, USA.
Email: decline@siu.edu