

**ORGANIZATIONAL THEORY DEVELOPMENT: DISPLACEMENT OF ENDS?\***

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## **ORGANIZATIONAL THEORY DEVELOPMENT: DISPLACEMENT OF ENDS?**

These are exciting times for organization theory. A proliferation of new theoretical perspectives and schools of thought has emerged in the discipline (McKinley & Mone, 2003; McKinley, Mone, & Moon, 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*, 2006; Smith & 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, 1995b), and about whether the existing theoretical schools in the discipline are incommensurable (Donaldson, 1998; Kaghan & Phillips, 1998; McKinley & Mone, 1998; Scherer, 1998). The difficulty of applying organization theory findings to practice seems also to have become a permanent issue (e.g., Beyer & Trice, 1982; Cheng & McKinley, 1983; Corwin & Louis, 1982; Thomas & Tymon, 1982; Tranfield & 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 mirrored in the closely related field of sociology, where an entire special issue of *Sociological 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, Cole, 1994; Collins, 1994; Davis, 1994; Lipset, 1994).

The current paper 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) that has changed new theory development from a means to further ends into an end in itself (*Academy of Management Journal*, 2006; Smith & Hitt, 2005). Merton (1940) developed the concept of displacement of ends to explain what happens when rules designed to contribute to organizational goals become goals themselves. However, he was at pains to emphasize that

displacement of ends is a human activity that also occurs outside the boundaries of bureaucracies.

In this paper I argue that in organization theory during the 1960s and 1970s the goal of investigators, at least implicitly, was a broader one than 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 organizational phenomena. New theory development was an important means to achieving this goal of consensus, 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 on offer was considered important, as was replication of empirical tests of the theories. These three activities -- proposition of new theories, development of standard measuring instruments, and replication -- 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 appears to have been a displacement of ends in which the goal of consensus about theory (in)validity has become submerged. In some ways this represents a retreat from a more complex objective to a simpler one, comparable to the movement in publicly traded corporations away from "effectiveness" toward the unidimensional performance criterion of shareholder value (Jensen, 2002). In the process of this retreat toward simplicity the empirical activities of instrumental standardization and replication of findings have been relegated to a position of second priority relative to new theory development (see, for example, Neuliep & Crandall, 1991). This situation has led to a proliferation of untested theories and a condition of theoretical incoherence 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.

In the first section of this paper I will expand this argument, asserting that in 1960s and 1970s organization theory investigators' sights were set on more than just new theory development. The ultimate goal was consensus about the validity or invalidity of the theories that were available at that time. I will illustrate this thesis by discussing the research streams on dimensions of organization structure and contextual predictors of organization structure. In the second section of the paper I will elaborate 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 goal of the discipline. Though I am generally critical of this displacement of ends, I also acknowledge that it may have some advantages for organization theory, including a stimulating proliferation of new theories that illuminate aspects of organizations that were not represented by the theories of the 1960s and 1970s. These potential advantages of the displacement of ends toward new theory development will be discussed in the third section of this paper. Despite potential advantages of the displacement of ends under examination, this displacement also has disadvantages, and these will be dealt with in the fourth section of the paper. Finally, in the fifth section I will propose some possible remedies for the disadvantages created by the emergence of new theory development as our ultimate end. These remedies include, among others, the creation of incentives for developing standardized measuring instruments (see McKinley, 2007) and the rejuvenation of the Research Notes section of *AMJ*. These remedies are designed to reallocate priorities in the discipline of organization theory, signalling to producers of research that 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 means -- to the ultimate end of achieving consensus on the validity or invalidity of theories.

## **The Goal of Consensus on Theory (In)Validity**

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 (or lack thereof) 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 the 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 broad goal of creating an empirically based consensus about theory (in)validity. 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 organization structure. This attention to structure ultimately 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., Udy, 1959; Hall, 1962, 1963; Hage, 1965) as the basis of a dimensionalization project that converted the attributes of Weber’s ideal-type scheme into dimensions that were argued to vary across organizations. Through this project, new research questions were opened up for exploration, including the issue of whether or not the dimensions covaried with one another and what contextual predictors might explain 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, Hickson, Hinings, Macdonald, Turner, & Lupton, 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 launching pad 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 structures and held that these dimensions would vary across a sample of organizations. Pugh, Hickson, Hinings, and Turner (1968) then sought to create a set of standard instruments that would measure the Pugh et al. (1963) structural dimensions and permit empirical research on them. Pugh et al. (1968) used data from a diverse set of 46 organizations in the Birmingham, England area to generate a complex set of scales, and these scales were then reduced by factor analysis to four underlying dimensions, labelled structuring of activities, concentration of authority, line control of workflow, and relative size of supportive component. Later, Pugh, Hickson, Hinings, and Turner (1969) used the same data set to delineate measures of several aspects of organizational context, including size, technology, and dependence. Pugh et al. (1969) and 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 four structural dimensions.

It seems unlikely that Pugh et al. (1968, 1969) would have undertaken the extensive instrumentation project reported in these papers if they had not had as their goal the development of cross-scholar consensus about the validity of the multidimensional profile of organizations proposed in Pugh et al. (1963). The extensive 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 hopefully participate in the production of this consensus. Apparently those colleagues saw things the same way, because the work of Pugh et al. (the Aston Group) was almost immediately subjected to an

extensive set of replications, re-examinations, and re-assessments using the same instruments on different data sets (see Child, 1972; Inkson, Pugh, & Hickson, 1970; Hinings & Lee, 1971; Reimann, 1973; Donaldson, Child, & Aldrich, 1975; Greenwood & Hinings, 1976). These replications and re-examinations sought to ascertain whether the Pugh et al. (1968, 1969) results generalized to a variety of different organizations 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 validity of the theory contained in the original Aston Group work. In fact, theoretical innovations appear to have been avoided, except perhaps in Child's (1972) work. This is a pattern typical of normal science (Kuhn, 1970). It is noteworthy, however, that these replications were published in the top organization studies and sociology journals, including *Administrative Science Quarterly*, *Sociology*, and the *Academy of Management Journal*. This is a measure of the perceived importance of the replications in the discipline at that time.

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 had been found by the Aston Group. 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" sample showed a negative correlation between these two dimensions. Child attributed this anomaly to differences in sample composition between his data set and that of the Aston group, but he also offered a theoretical explanation to account for the finding. Child (1972) argued that the structuring of activities, and particularly the impersonal rules and job descriptions that were an important sub-dimension of this structural component, created a mechanism of impersonal control that reduced the perceived risks of 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 reduced. 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 envisions delegation of authority down a hierarchy composed of specialized administrators.

The anomaly reported by Child (1972) set off a scramble to ascertain whether the original Aston group 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 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, Child, and Aldrich (1975) collaborated in a three-part research note, returning to Child's (1972) inference 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 anomaly 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, Child, & Aldrich, 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 English local

authority departments. 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 an alternate control mechanism that permitted decentralization of decision making to lower levels of an organization's administrative hierarchy. Greenwood and Hinings (1976) did not present their results as a conclusive refutation of the Child (1972) hypothesis, however, so the anomaly remained unresolved.

While the stream of work outlined above did not result in a conclusive consensus about whether Child (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 was apparently being devoted by a significant number of researchers to the goal of generating such a consensus. This involved extensive instrumentation, data manipulation, and replications, with the cooperation of prestigious 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, Hickson, Pugh, & Pheysey, 1969; Blau, Falbe, McKinley, & Tracy, 1976; Marsh & Mannari, 1980). A similar focus was present in attempts to validate Blau's (1970) formal theory of differentiation in organizations (e.g., Mileti, Gillespie, & Hass, 1977; Miller and Conati, 1980) and to achieve a consensus about whether the size-administrative intensity relationship was affected by definitional dependency (Freeman & Kronenfeld, 1973; Feinberg & Trotta, 1984; Kasarda & Nolan, 1979; MacMillan and Daft, 1979).

In summary, I argue that the field of organization theory in the 1960s and 1970s can be represented by the set of linkages diagrammed 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, though in some cases (particularly that of the “technology” construct), the instrumentation actually varied considerably from study to study and the construct was “stretched” (Osigweh, 1989) to accommodate the variations in measurement. The goal was not new theory development, but rather theory testing in the service of consensus about theory validity or invalidity. The theory development, instrumentation, and replication processes influenced one another, as indicated by the double-headed arrows at the left side of Figure 1, but my conclusion is that the overall motivation of the researchers carrying out these complicated, interdependent activities was to come to a conclusion about the empirical validity of the theories at issue and diffuse this conclusion across the discipline of organization theory. The achievement of this consensus was admittedly elusive, but the goal was there.

[Place Figure 1 about here]

While inferences about the motivation of organization theorists at this time have been indirect, as previewed 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 sceptical about theory and did not value theory for its own sake. Assuming that scepticism 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 validations beyond the site of their production to the discipline as a whole. In this process, theory development was a means to the end of consensus on theory validity or invalidity, rather than an end in itself.

### **Theory Development – 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

whether the theory is empirically valid but rather as worthwhile goal for its own sake. This displacement of ends tends to devalue the replication of tests of theory in favour of the generation of new theory. For example, two special issues of the *Academy of Management Review* (in 1989 and 1999) have 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* also published a well-received forum on theory development, in which the prominent theorists Sutton and Staw (1995), Weick (1995), 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 whether a given theory is valid. Finally, Smith and Hitt (2005: 587), in an epilogue to an edited volume in which distinguished organizational 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 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 studies.

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 my suggestion that these activities have assumed a subordinate status in organization theory today. Indeed, McKinley (2007: 135) has stated 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.” Hubbard, Vetter, and Little (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 replication studies among editors of social and behavioural science journals. It is also worth noting that in management scholarship a protected niche (the *Academy of Management Review*) exists for papers whose purpose is to espouse new theory, while there is no comparable niche for papers whose objective is to validate a pre-existing theory or to present instrumentation for pre-existing constructs. Arguably, the Research Notes section of the *Academy of Management Journal* formerly played this role, but that section has recently been phased out. In summary, the epistemological literature examining the nature of management and organization theory scholarship and the structure of the discipline’s distribution channels both tend to be consistent with the idea that theory development has emerged as “king of the hill”.

[Place Figure 2 about here]

The dominant status of theory and theory development in today’s organization theory is further supported by a sceptical attitude toward objectivity, a fundamental philosophical foundation of any project to develop empirically based consensus about theory validity or invalidity. Prominent commentators such as Astley (1985) and others have argued that organization theory is a fundamentally subjective enterprise in which theory functions as a social construction rather than a representation of some underlying empirical reality. To the extent that this type of argument is accepted by contemporary organization theorists, it

removes much of the consensus to embark on validation exercises such as those undertaken by researchers attempting to reproduce the Aston Group's results. Instead, theory becomes a narrative that dominates attention for its interest and uniqueness (Davis, 1971; Mone & McKinley, 1993), while replication and instrumentation of constructs fade into the background of second priority. This situation is further promoted by *prescriptions* for subjectivity, such as Case's (2003) advocacy of "subjective authenticity" as the appropriate standard for good organizational scholarship. All this helps create a context in which a displacement of ends toward theory development is facilitated, although the status of theory changes from representation device to story machine. Published theories are sometimes accompanied by confirmatory empirical results, but those results are almost never generalized beyond their sample of origin by replication and instrumentation efforts

### **Theory Development as End: Advantages**

Assuming my argument about the displacement of ends toward theory development has some credibility, what are the consequences for the discipline? Are there any advantages to the shift toward theory development as a goal *per se*? As stated earlier in the paper, one possible advantage is the evolution of a wide array of novel, and even exciting theories that invoke aspects of organizations that were not foci of attention in the 1960s and 1970s. Indeed, organization theory has witnessed a tremendous expansion in the number of theoretical schools it accommodates (McKinley et al., 1999; Pfeffer, 1993). Theory-based schools of thought now exist that feature 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 thirty years ago. These theories are engaging, at least as stories, and often their authors offer empirical support for them – but that support seems to be directed more toward legitimizing the theory and improving its publication chances than to producing a field-wide consensus about whether or not the theory is a valid representation of a phenomenon. Indeed, the

organized instrumentation and replication program required to generate such a consensus might, ironically, have detracted from the discipline's capacity to produce all these new theories. I am not arguing here that these new theories would necessarily have been disconfirmed if large-scale instrumentation and replication programs had been in force, but simply that the production of a wide array of new theories has left little time and energy for organized programs of this nature. Thus displacement of ends toward the goal of new theory development has brought excitement born of novelty.

At the same time, the emphasis on theory development as end has probably facilitated the rapid theoretical transitions that are necessary to keep up with the "relentlessly shifting" organizations that many argue are a prominent feature of today's organizational landscape (see, for example, Brown & Eisenhardt, 1997; Volberda, 1996). In a field like organization theory that arguably emphasizes theory development and deemphasizes large-scale theory validation, new theories can be quickly introduced to explain (or even represent) new organizational forms that are evolving. Indeed, theory development as end promotes the flexibility of new theory development, and helps ensure that 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 "true" by a majority of organization theorists, but whether it is plausible (Weick, 1995) and above all, relevant. There are occasional complaints that our theories lack relevance and are out of date (e.g., Daft & Lewin, 1993), but our emphasis on theory development as a goal in itself may actually minimize that irrelevance.

### **Theory Development as End: Disadvantages**

While the emergence of theory development as goal 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 an array of theories that have never been disciplined or modified through exposure to empirical data. Not all new theories fit this

category but many do, especially those that are published in the *Academy of Management Review*. Indeed, it is a striking feature of our field today that there is little demand for testing the theories that are collected in the pages of *AMR*, and little incentive for empirical researchers to do such testing. These researchers may perceive, with some justification, that testing a pre-existing theory without contributing original theory of their own is a recipe for disaster in the review process. Thus most of the theories in *AMR* have never been assessed empirically, or at best, have been assessed once without replication. Locke and Latham (2005: 147) hinted at this problem 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 has implications for our ability to advise practitioners: it suggests that we will be confined to the symbolic application of knowledge that was identified by Pelz (1978), while being inhibited from implementing the instrumental and conceptual uses he also discussed. Astley (1985; Astley & 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 studies knowledge are valuable to practicing managers, the goal of using management knowledge as an instrumental lever (Cheng & 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 replications and has attracted the consensus of a majority of experts in the relevant specialty area. Arguably the emphasis on theory development as an end undercuts that outcome by making empirical replication unfashionable (Neuliep & Crandall, 1991) and fragmenting the organization studies attention space (Collins, 1998) among many incommensurable theories (McKinley & Mone, 2003).

I think we sometimes underestimate the chaotic appearance that organization theory must have in the world of practice, because of the array of disconnected theories contained 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 is a desirable stimulus for intellectual freedom and flexibility; but does the practicing manager have the same view? In my opinion the answer is “probably not”. Instead, the wide array of idiosyncratic and untested theories likely makes our field look chaotic to practicing managers, fostering the impression that the field has little to offer them. Because of the information overload they experience, most managers are attracted to simplicity (Miller, 1993), and contemporary organization theory does not offer the appearance (or the reality) of theoretical simplicity. It may be more appealing to managers to simply follow the prescriptions of the latest popular management fashion (Abrahamson, 1996) or bow to whatever wave of institutional isomorphism is sweeping their industry (DiMaggio & Powell, 1983).

Admittedly my inferences about how managers perceive our field, if they perceive it at all, are open to debate. We should clearly be developing new theories about how managers perceive us and – at least under traditional criteria of good research – subjecting those theories to empirical testing and replication. At the same time, it might be valuable to become more sensitive to the ways in which our theoretical differentiation affects managers’ perceptions of us, and the role that taking theory development as the end may play in fostering such perceptions.

### **What Is To Be Done?**

As Stinchcombe (1994) put it succinctly in his paper 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 the disadvantages of our current (over)emphasis on new theory development. I believe there are some remedies that could be applied rather painlessly to

address these disadvantages by reallocating priorities and resources toward theory testing and away from the goal of theory development for its own sake. First, and perhaps most important, the editors of *AMJ* could restore the Research Notes section and explicitly dedicate it to papers that present new or improved instrumentation for previously published constructs or that test theories proposed in the pages of *AMR*. This would create a protected niche for theory testing that would balance the protected niche for theory development that already exists at *AMR* itself. A side benefit of such a policy shift would be to tighten the linkage between *AMR* and *AMJ*, so that together these two 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 the pages of *AMJ*, but on the other hand the current policy that every paper published in that journal must contribute to new theory development seems to be a contributing factor to our current state of theoretical dis-integration. Revitalizing the Research Notes section of *AMJ* and designating it as a protected arena for pure empirical assessment of existing theory would also send a strong signal to the field that the disciplinary elite value efforts to generalize theories beyond their point of origin to multiple empirical sites. 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.

Second, journals like *AMJ*, *Strategic Management Journal*, *Journal of Management*, *Administrative Science Quarterly*, *Organization Studies*, and *Organization Science* could offer expedited review to instrumentation studies, empirical tests of previously published but untested theories, or first replications of theory-testing studies. While a long stream of replications of the same relationship is undesirable unless there are significant variations in sample characteristics (e.g., Rosenthal, 1991), my recommendation is designed to address the low status that replication and instrumentation work seems to have currently (Neuliep & Crandall, 1991). Instrumentation studies, first empirical tests, and first replications of such tests 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 could be offered to the former types of papers to encourage authors to make such submissions.

Finally, and most radically, we might think about the possibility of shutting down *AMR* for a few years. This would provide an opportunity for empirical testing and winnowing of the large backlog of untested theory that has accumulated in the pages of this journal. A rigorous empirical selection process on the corpus of *AMR* theory might benefit the discipline by filtering out those theories that have little or no predictive value and generating some consensus about which theories are deserving of further research attention. Right now there is no way to make such decisions because almost none of the papers in *AMR* have been subjected to empirical scrutiny. I know this recommendation will appear misguided to some readers, and I hope, above all, that it is not perceived as an attack on *AMR*. I have published there, continue to submit there, and believe that the creation (in 1976) of a journal uniquely devoted to theory development was an important addition to the knowledge distribution channels in management and organization theory. Nevertheless, if theory is to do more than generate interesting conversations, it needs to be evaluated by comparison with data, so that a consensus can be built about the representational worth of the theory. Shutting down *AMR* for a few years might allow theory testers and replicators to catch up with the flood of new theory that has been distributed to the discipline in recent decades.

## **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 theory (in)validity 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 neglected and novel organizational

phenomena. But I have also claimed that the displacement of ends toward new theory development has disadvantages. Among those disadvantages are an accumulation of untested theories in the pages of journals like *AMR* and a chaotic presentation of self that inhibits our ability to advise practicing managers. Finally, I have recommended some policy initiatives that organization studies scholars and journal editors might adopt to reduce the negative consequences of the displacement of ends toward theory development as ultimate goal. Those recommendations include restoration of the Research Notes section of *AMJ*, dedication of that section to pure empirical assessment of previously published theories, and expedited review processes for instrumentation studies and replication studies. Most radically, I have suggested that we think about shutting down *AMR* for a few years, until empirical researchers have a chance to collect the data, create the instruments, and perform the data analyses necessary to test at least some of the theories published there. This does not mean the permanent disappearance of the *AMR* “brand” or the type of paper typically published there; instead, I am advocating a pause in the theory development juggernaut that *AMR* has (to its credit) become, in order to give empirical researchers a chance to catch up. Hopefully these recommendations will be taken as they are intended: as remedies to improve the production and application of knowledge in our field, rather than deconstructions of what organization theory has achieved thus far.

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

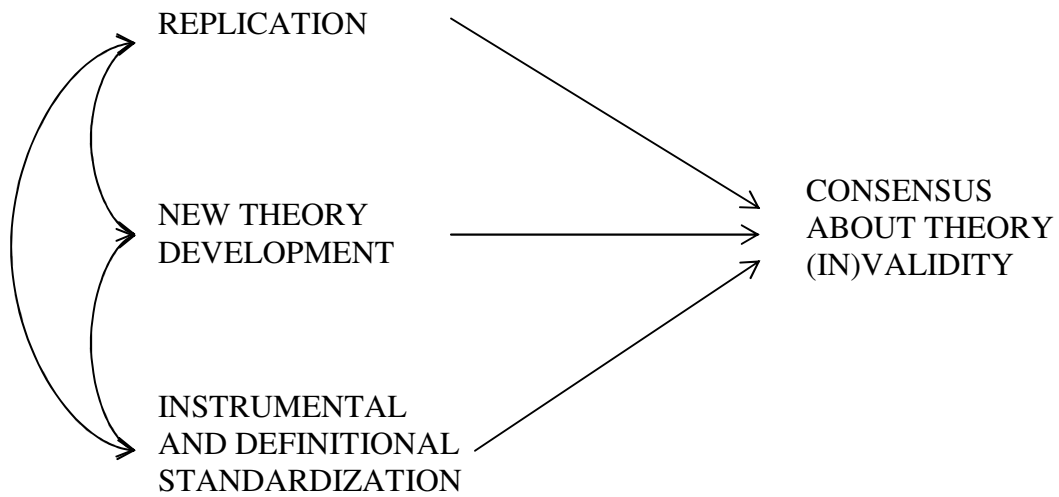
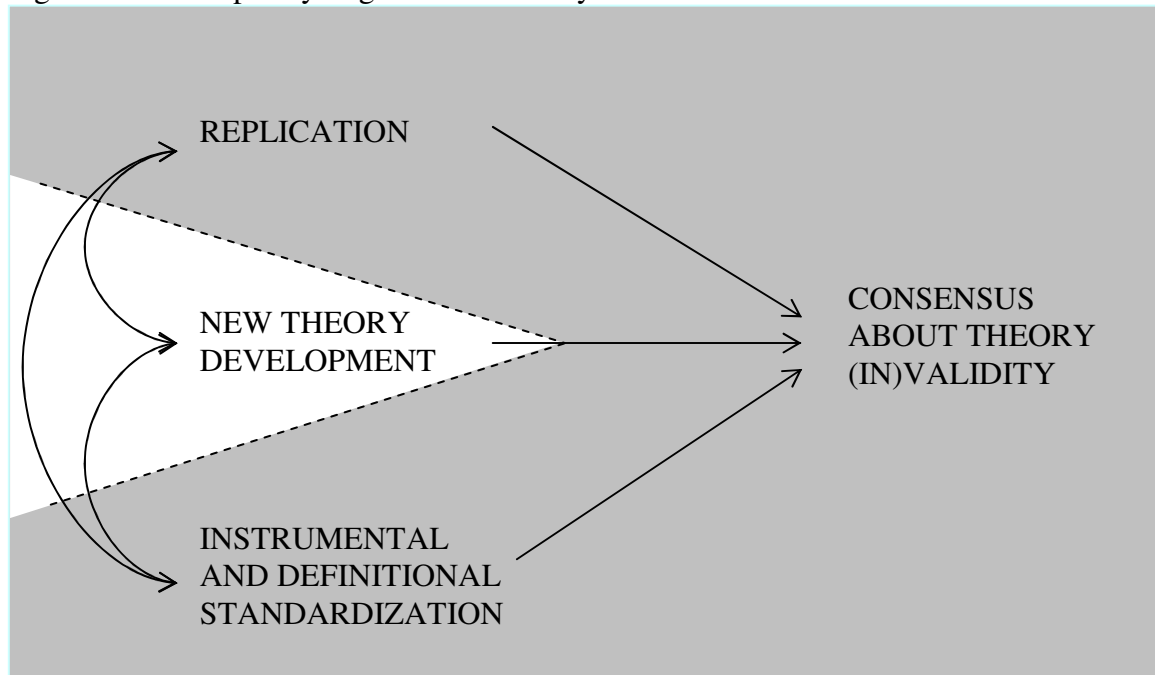


Figure 2: Contemporary Organization Theory



### BIOGRAPHICAL NOTE

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