

Organization theory:

Bright prospects for a permanently failing field

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Erasmus Research Institute of Management - ERIM



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Organization theory: Bright prospects for a permanently failing field

Inaugural Address

Address given in shortened form
on the occasion of accepting the appointment as
Full Professor of Organization Theory, Development, and Change
at the Rotterdam School of Management, Erasmus University Rotterdam
on Friday, September 12, 2008

by

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Voor Marieke

Samenvatting

Organisatietheorie is een paradoxaal veld van wetenschapsbeoefening. Het worstelt al meer dan vijftig jaar met de vraag hoe een geïntegreerde theorie over de effectiviteit van organisaties te produceren, die geschraagd wordt door breed gedragen assumpties. Tot op heden is het er niet in geslaagd zo'n theorie te produceren. Tegelijkertijd is het veld ook zeer succesvol. Het heeft grote mobiliserende vermogens en organisatietheoretische publicaties worden gewaardeerd en geciteerd tot ver over de grenzen van het veld. In deze rede probeer ik deze paradox te ontcijferen. Ik bespreek allereerst een aantal van de tekortkomingen van het veld, zoals theoretische fragmentatie en strijd tussen methodologische kampen. Maar ik stip ook enkele formidabele sterke punten aan, zoals de eendeloos intrigerende vragen die het zichzelf durft te stellen en de unieke veelzijdigheid waarmee het veld het probleem van pluralisme adresseert. Bovendien wijs ik een drietal overkoepelende methodologische strategieën aan die organisatietheoretici kunnen gebruiken om de problemen aan te pakken die het veld momenteel hinderen in haar ontwikkeling. In de eerste plaats beargumenteer ik dat een grotere mate van integratie tussen bestaande theorieën kan worden bereikt door het in kaart brengen van verborgen modererende variabelen. Zulke ongespecificeerde variabelen kunnen tot tegengestelde onderzoeksbevindingen leiden wanneer dezelfde theorie getoetst wordt in verschillende macrosociale contexten. In de tweede plaats stel ik dat het veld de relevantie van haar theorieën kan verhogen door het identificeren en erkennen van empirische hoofdfeiten ('stylized facts'). In de derde plaats kunnen we ons gezamenlijke begrip van organisatiefenomenen vergroten door het verkennen van de microfundering van onze macrotheorieën. Met andere woorden, wanneer we het lef hebben om onze methodologische veren eens goed op te schudden, zouden de vooruitzichten voor het veld weleens uiterst zonnig kunnen zijn.

Abstract

Organization theory is a paradoxical field of scientific inquiry. It has struggled for more than fifty years to develop a unified theory of organizational effectiveness undergirded by a coherent set of assumptions, and it has thus far failed to produce one. Yet, by other standards it is simultaneously a tremendously successful field. It has great intellectual mobilizing powers and its publications – journals as well as books – are highly esteemed. In this address I attempt to unravel this paradox by discussing the field's considerable pathologies, such as its tendency towards theoretical fragmentation and methodological factionalism, as well as its formidable strengths, like the endlessly intriguing questions it asks itself and the considerable ambidexterity with which it handles pluralism problems. Most importantly, however, I propose three overarching methodological strategies with which organization theorists can address the problems currently hampering their field. First, I argue that greater integration amongst extant theories might be reached by exploring hidden moderators that can produce contradictory research findings across macrosocial contexts. Second, the field can improve upon its theoretical relevance by discovering and acknowledging the stylized facts of organizational life. Third, we can collectively increase our understanding of organizational phenomena by exploring the microfoundations of our macro theories. In short, if we dared to ruffle our methodological feathers, the prospects for the organization theory field could be very bright indeed.

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Modern organization theory is an amorphous aggregation of synthesizers and restaters, with a few extending leadership on the frontier. For the sake of these few, it is well to admonish that pouring old wine into new bottles may make the spirits cloudy. Unfortunately, modern organization theory has almost succeeded in achieving the status of a fad.

William G. Scott (1961)

*Mijnheer de Rector Magnificus,
Geacht College van Dekanen,
Geachte collega's,
Zeer gewaardeerde toehoorders,*

The view that most of us hold of any field of science is easy to explain in a few sentences. Central to most fields is a singular object of study. About this object we entertain a uniform theory, which tells us how the object behaves under all known circumstances. Behind this theory is a set of assumptions, related to (a) how the object is constituted, (b) what knowledge we can possibly gather about the object, and (c) how we should go about doing so.¹ In short, the beliefs we hold about the scientific enterprise tend to be neat, tractable, and reassuring.

Unfortunately, this view proves to be entirely wrong when we use it to understand either the historical development or the present condition of the field of organization theory. For example, organization theorists do not agree on an object of study – do government bureaucracies, interorganizational networks, and new forms of organizing qualify as ‘organizations’ or not (Kallinikos, 2006)? Furthermore, instead of being informed by a single theory, these theorists derive their inspiration from – and of course inevitably take a stand behind – literally dozens of different theories.²

¹ In more formal language, these assumptions relate to a field’s ontological, epistemological, and methodological beliefs. For a concise explanation of these terms, please consult Okasha (2002).

² Tellingly, the *Academy of Management Journal*’s Subject Index Form asks of authors to identify the theoretical perspective to which they adhere. It offers them a choice of no less than 63 options. Interested readers can consult the form here: <http://www.aom.pace.edu/amj/forms.htm>.

Also, bitter bickering takes place over the issues of how organizations are constituted – are modern complex organizations the epitome of a Durkheimian ‘emergent property’ or is organizational life wholly reducible to the actions and intentions of individuals (Weick, 1979)?; what there is to know about them – can we understand organizations directly and objectively or only from the mediated point of view of the individuals who live and work in them (Hatch & Cunliffe, 2006)?; and how should they be studied – should we aim for quantitative measurement of organizational life or should we leave its narrative structure intact (Heugens & Mol, 2005)? In short, any deeper look into the field of organization theory will reveal that it is in fact messy, intractable, and upsetting.

In this address I will concentrate on a central issue that emerges from the tension between our ideal-typical views of scientific work and the messy state of affairs in organization theory: How can we discover rigorous and relevant knowledge in a permanently failing field?³ I will address this question in two steps. First, I briefly survey the history and present state of the field by comparing it, somewhat ominously, to that of a modern Balkan state. This metaphor is illuminating, or so I will argue, since the organization theory field and modern Balkan states share at least three ‘nasty’ habits, notably they: (a) falsely claim kinship and lineage to distant heroes; (b) cherish foundational myths which legitimize their existence; and (c) never tire of challenging the boundaries they share with their neighbors. This analogy culminates in an enumeration of the pathologies to which the field has fallen victim. Second, I point out a number of successes that the field has managed to accumulate, in spite of these pathologies. I show that the field has a set of formidable strengths that apparently help it to compensate for many of its shortcomings. Building on these strengths, I discuss three possible ‘meta research strategies’ which allow us to capitalize on these strengths and overcome the pathologies.

³ The term ‘permanently failing field’ is a nod to Meyer and Zucker’s (1989) work on permanently failing organizations. In my view, a permanently failing field of science is one that is remarkably persistent due to its legitimated status in the eyes of universities, scholarly associations, and funding agencies, in spite of its inability to effectively produce knowledge that is recognized as both rigorous and relevant by all its major constituencies. For a concise history of the rigor versus relevance debate in management research, see: Vermeulen (2005).

These research strategies include: (a) the discovery of stylized facts in organizational life (Helfat, 2007); (b) searching for macrosocial contingency variables that influence the tenability of theories and foster the integration of certain theories (Donaldson, 2001); and (c) theoretical rejuvenation through the unpacking of organizational nuts and bolts (Elster, 1989). I illustrate each of these macro-research strategies with examples of my own – past, current, and future – research agenda.

Analogies and pathologies

A popular kick-off for texts on organization theory is to claim that organizations are ubiquitous (Perrow, 2002; Pfeffer, 1997; Scott, 2006; Simon, 1991). I would like to start with the related observation that the same holds true for the people who study them. Yet, not every organizational researcher is an organization theorist. Reputed scholars agree that organization theory is a sub-discipline of the larger field of organization studies,⁴ which also includes the related sub-disciplines of “industrial relations, industrial and organizational psychology, organizational sociology, management, administrative theory, and organizational behavior” (Hatch & Cunliffe, 2006: 26).

Three aspects set organization theory apart from these other branches of the organization studies family, notably its: (1) mother discipline; (2) explanandum; and (3) explanans.⁵ First, organization theory’s mother discipline is sociology. This contrasts it with organizational behavior, which is more firmly rooted in (social) psychology, and with industrial relations, which is more akin to (labor) economics. It also puts it more subtly apart from administrative theory, which has more pronounced multidisciplinary origins in both sociology and political science. Second, the organization theory field is broadly interested in explaining organizational effectiveness, or the ability of the organizations to (a) meet goals like survival and legitimacy; (b) fulfill social functions like benefitting the larger society or stakeholder environment to which they belong; and (c) induce its social environment to supply it continuously with scarce and valued

⁴ Pfeffer (1997: 4) describes the field of organization studies as follows: [it] comprises an interdisciplinary focus on (a) the effect of social organizations on the behavior and attitudes of individuals within them, (b) the effects of individual characteristics and actions on organizations (...), (c) the performance, success, and survival of organizations, (d) the mutual effects of environments (...) on organizations and vice versa, and (e) concerns with both the epistemology and methodology that undergird research on each of these topics.

⁵ This short typification of the field owes a lot to several discussions I have had on this topic with Dr. Will Felps at the Organization and Personnel Management Department of the Rotterdam School of Management, Erasmus University, with whom I team-teach the Master of Philosophy course *Advanced Topics in Organization Theory*.

resources (Lewin & Minton, 1986; Yuchtman & Seashore, 1967).⁶ Third, organization theorists traditionally explain organizational effectiveness by pointing at the influence of social-structural variables. Inside the organization, they explore variables like hierarchy and authority, as well as reward and disciplining systems (van Oosterhout, 2008). Between organizations, they assess the effect of such variables like status differentials and network positions on organizational effectiveness. Organization theorists thus typically assume that individuals are simply constituted and somewhat homogenous, in order to fully appreciate the complexity of the organizational systems they study. In short, organization theory is a branch of applied sociology that seeks to explain organizational effectiveness differentials by exploring intra- and interorganizational social-structural influences.

⁶ Please note that this notion of 'organizational effectiveness' is considerably broader than the concept of 'organizational performance' used by strategic management scholars, which is traditionally defined as the persistence of firm profits (Rumelt, Schendel, & Teece, 1991: 12).

Analogies

I opened this address by stating that organization theory is different from other fields of science, or at least different from our stylized accounts of what a field of science *ought* to look like. So if organization theory is different from ‘regular’ paradigmatic (Kuhn, 1962) fields of science, what *does* it look like? In order to better understand its present state of affairs, it makes a lot of sense to compare the field to a (modern) Balkan state. Like Montenegro, Macedonia or Slovenia, the community of organization theorists is the product of several recent (scholarly) migrations, it is multiethnic / multidisciplinary, and above all: young. Since these nascent communities – national and scholarly – have no real independent history of more than a few decades at most, they are still struggling on a daily basis to literally invent something of a unifying identity for themselves. To fight this struggle, they tend to employ a broad arsenal of mobilizing and propagandizing public relations weaponry. Specifically, they: (a) claim grand old men and women as their (intellectual) ancestors; (b) feverishly cling to (predominantly fictional) foundational myths; and (c) never tire from bickering over geographical and disciplinary boundaries. Since this address is on the scholarly field of organization theory rather than European geo-politics, I will now illustrate these analogies with examples from the scholarly side.

A first ‘weapon’ organization theorists commonly use in their struggle to craft a compelling collective identity for their community is to claim distant heroes as their own. This usually takes the form of claiming continuity of thought between distant intellectual forbearers and present-day contributors to the discipline. A survey of seven best-selling organization theory textbooks⁷ reveals that members of the discipline commonly (i.e., in the majority of cases) claim kinship to Weber, Marx, and Durkheim, and regularly (i.e., more than twice) to Smith, Barnard, and Taylor (amongst others). It is easy to understand why so many respected members of the profession claim intellectual ancestry to these towering figures in sociological, economic, and administrative thought.

⁷ I have consulted the following internationally bestselling texts: Daft (2004); Hatch & Cunliffe (2006); Jaffee (2001); Jones (2003); MacAuley, Duberley, & Johnson (2007); Robbins & Barnwell, 2002); and Scott (2003).

Organization theorists are engaged in constant status competition with scientists from other disciplines,⁸ both in management as well as in adjacent disciplines (cf. Becker, 2007: Chapter 2). By spiking their writings with references to commonly recognized intellectual giants, they hope to come across as more worldly and mature, or, as Becker (2007) calls it plainly, ‘fancy.’⁹ Although such claiming behavior is often innocent and occasionally productive, it is also often false and misplaced for two reasons. First, these claimed ancestors were writing about, say, bureaucracy, authority, and the division of labor long before organization theory was known by its name. Thus, the connections between these writers and the discipline of organization theory can only be made in retrospect and without their consent. Second, many of these individuals are simultaneously and often more appropriately claimed by other legitimacy-seeking disciplines. The problem with these claims is not so much one of (dual) classification, as it is one of a lack of respect for the history of ideas. Organization theorists are often untrained to appreciate the works they cite in their entirety or in their appropriate historical and intellectual context, which tends to make their references to these works increasingly superficial and ceremonious (cf. Lounsbury & Carberry, 2005). In short, although the pedigree weapon is usually chosen to heighten the status of organization theory as a discipline, its careless use can just as easily backfire and result in significant status loss.

A second ‘ploy’ that is commonly used by organization theorists to elevate their collective sense of community and shared identity is the construction and preservation of what can legitimately be called ‘foundational myths’: partially factual and partially fictitious narratives on the emergence and early history of the field. These stories are told and retold time and again in graduate courses and PhD seminars all over the world in order to socialize new members into the lore of the profession, and to engage them in the nitty-gritty work of upholding the discipline.

⁸ This type of status competition between scholarly fields must be distinguished from the more commonly discussed status competition between schools and/or departments, of which the outcome is primarily determined by scholarly productivity and by the centrality of a school or department in the networks of association that emerge from the exchange of PhDs (Burris, 2004).

⁹ To be perfectly honest, I regularly fall victim to this behavior myself. See, for example: Heugens (2005) and Heugens, Kaptein, and van Oosterhout (2008).

They are simultaneously a sensemaking device which helps new members understand the boundaries and inner workings of their field as well as a mobilizing device which encourages them to take pride in their intellectual origins and to begin contributing to the maintenance of their field through the reproduction of its research and teaching practices. As many of these narratives are the product of semi-private sensemaking efforts by graduate instructors themselves, and as they are partially oral histories which are retold in the classroom setting more frequently than that they are published in research articles or textbooks, it is impossible to tell which of these foundational myths, of which plenty exist, are the most important or the most frequent. To give an impression of what I am talking about, however, I will present two common myths, notably: 'The Source' and 'The Great Migration.' These appear in separate text boxes below. Although these narratives are rather different from one another, they each serve the purpose of inventing a shared history for the organization theory community.

Foundational Myth 1: 'The Source'

According to one popular foundational myth, organization theory began (put somewhat chargingly) as a series of footnotes to Weber. In the words of Lounsbury and Carberry, "the work of Max Weber was an omnipresent guiding force in the early development of organizational theory" (2005: 501). In fact, Weber's role in the emergence of the discipline is believed to be so foundational by some that Charles Perrow simply refers to him as 'The Source.' The story goes more or less as follows: Weber's *magnum opus* *Economy and Society* was published posthumously in 1922, but it took until 1946/1947 for the first translations into English to appear (including a famous translation of selected chapters by Talcott Parsons, then the most renowned sociologist of his generation). These translations 'disclosed' Weber to US scholars, first and foremost to the students of Robert Merton at Columbia. These students included Philip Selznick, Peter Blau, and Alvin Gouldner, who all took the inspiration from Weber's writings on bureaucracy to conduct detailed case studies of bureaucratic organizations. Soon, they were followed by other neo-Weberians like Amitai Etzioni and Richard Scott. These authors 'invented' organization theory as a separate area of study as they found out, probably to their own amazement, that the Weberian principles of bureaucracy provided no fool-proof blueprint for the design of effective organizations. Rather, organizations designed according to these principles were easily co-opted (Selznick, 1949) by internal and external factions, and the rules they issued were often mocked by organizational constituents (Gouldner, 1954). Explaining these deviations from the ideal-typical bureaucratic pattern then became the bread and butter of the first few generations of organization theorists.

Foundational Myth 2: 'The Great Migration'

A second foundational myth is one that centers on the joint phenomena of scholarly migration and acculturation. The story behind this myth usually begins by pointing out that in the 1940s, 1950s, and 1960s, there really was no such thing as organization theory. There only was the sociology of organizations, but this was (and remains) a relatively minor field of applied study within the larger sociology discipline (historically, roughly ten to fifteen percent of all articles published in the major sociology journals *American Journal of Sociology* and *American Sociological Review* focus on organizational sociology). A great change came about due to the meteoric rise of business schools as new departments in public and private universities as well as liberal arts colleges in the US from the 1950s onward (e.g., see Kraatz & Zajac, 1996). These new institutions were staffed with individuals with disciplinary training, including many professional sociologists who were looking for improved employment prospects and better pay (Hinings & Greenwood, 2002). But this migration alone did not 'cause' the emergence of organization theory as a discipline. A second necessary ingredient was the change in topical focus that working in a business school implied for sociologists. Originally, organizational sociologists used to study the impact of organizations on society. Typical organizational sociology research questions addressed in the 1900 – 1950 period include: How do organizations affect political life? How vulnerable are democratic institutions to corporate pressure? What role do social elites play in governing firms? Do organizations contribute to or nibble away from pre-existing status stratifications in society? When they became business school professors, however, the focus of organizational sociologists shifted towards managerialism and organizational effectiveness (Hinings & Greenwood, 2002). The guiding research question became how managers could improve the effectiveness of their organizations (Hinings & Greenwood, 2002; Perrow, 1986, 2002). In short, when their increasing acculturation to the business school climate began to change these organizational sociologists' teaching and research agendas, the discipline of organization theory was born.

A third practice organization theorists tend to resort to in order to secure the status of their field as a scholarly discipline (as well as the resources that are associated with that status) is to bicker with members of other disciplines over the boundaries of their field. Usually, these conflicts take the form of arguing that one or several organizational theories are more appropriate instruments for addressing fundamental questions about organizations (such as: "Why do they exist?" "How do they operate?" and "How are they affected by social structure?"; cf. Heugens, 2005; Heugens & Lander, 2008) than

theories derived from other disciplines. The presumed upshot of 'winning' such an intellectual battle is that the victorious discipline can claim more resources from philanthropists, public administrators, and corporate Maecenases to conduct further research (and teach) on the newly acquired territory. The original disciplines are rarely competed away altogether, but they will have to become accustomed to the other occupants in their resource space. Traditionally, the gravest conflicts take place between organization theory on the one hand and organizational economics and organizational sociology on the other. Organization theorists accuse organizational economists of always foolhardily assuming perfect rationality, equilibrium, and perfect information (Barney & Hesterly, 2006). In turn, organizational economists criticize organizational theorists for using ill-defined assumptions in their work. Organizational theorists accuse organizational sociologists of merely being interested in macro-phenomena, thereby ignoring key micro variables and micro-historical processes (cf. Zucker, 1999). To reciprocate, organizational sociologists accuse organization theorists of using overly simplified treatments of sociological variables in their work, which do little justice to the original concepts (Kraatz & Zajac, 1993). What matters for this address is not whether any of these parties is 'right,' but to understand this bickering as a reflex of a young academic field that is trying to secure a position and resources for itself amidst well-established disciplines studying a related area of content.

Pathologies

The foregoing observations merely suggest that organization theory is a nascent (or perhaps adolescent; cf. Scott, 1987) discipline that is trying to secure its prolonged existence and establish an integrative identity for itself. This alone is insufficient cause for the title of my address, in which I call organization theory a ‘permanently failing field.’ So are there any signs that would suggest that organization theory is durably in crisis?

Unfortunately, there are. I opened this lecture with a hand-picked quote by William G. Scott, and rarely have I agreed more with a commentator. The field of organization theory is now some fifty years old, and it is still jockeying for a sustainable position amongst the other disciplines comprising the organization studies field, as it has not yet discovered a convincing set of widely shared identity claims that might differentiate it beyond doubt from its sibling disciplines. Also, in contrast to closely affiliated disciplines like organizational behavior and strategic management, organization theory tends to be offered as an elective course (as opposed to a compulsory one) in many business schools. Furthermore, the membership of the Organization and Management Theory Division (OMT) of the Academy of Management (AOM) is stable,¹⁰ but the Business Policy and Strategy Division and the Organizational Behavior Divisions are substantially larger. Most importantly perhaps, the field of organization theory still has to produce the definitive answer as to why certain organizations effectively reach their self-proclaimed goals, whereas others fail to do so, sometimes over a prolonged period of time (Meyer & Zucker, 1989). Finally, it should be taken into account that the aforementioned quote by Scott is not recent, but dates back to 1961. In other words, to the extent that the organization theory field is itself experiencing failure, it has had this experience for a long time. Why is this the case? A closer inspection of the field reveals at least three ‘pathologies’ that have long hampered the field’s progress on all fronts, notably: (a) stifling theory development; (b) borrowed methods; and (c) internal fault lines.

¹⁰ The Academy of Management (www.aonline.org) is the premier professional association for management scholars. It has numerous divisions, of which the Organization and Management Theory Division (<http://division.aonline.org/omt/>) is the one frequented by organization theorists.

A first pathology I will touch upon here is theoretical stifling in the organization theory field. This phenomenon comes in various forms. As stated above, the field is fragmented to an almost mind-boggling degree. Furthermore, the core theories organization theorists have worked with for decades are quickly running out of steam. They fail to account for new organizational forms and changing social realities, which raises the concern that “organization theory is in danger of becoming isolated and irrelevant to leading the emergence of new paradigms” (Daft & Lewin, 1993: i). Also, recent meta-analyses have shown that these major organization theories are, even though they are not outright wrong, at best trivial and at worst irrelevant. Dalton, Daily, Certo, and Roengpitya (2003) and Heugens, van Essen, and van Oosterhout (2008) have discovered that central agency-theoretical predictions yield disappointingly weak support. Geyskens, Steenkamp, and Kumar (2006) and Heugens and Lander (2008) have discovered the same for transaction cost theory and institutional theory respectively. Still, these theories continue to feature on the curriculum in most organization theory courses and they maintain a formidable presence in our scholarly journals. This is pathological. As Webster and Starbuck put it: “ineffective theories sustain themselves and tend to stabilize [organization theory] in a state of incompetence (...) Theories about which scientists disagree foster divergent findings and incomparable studies that claim to be comparable” (1988: 95). As organization theorists, we are in dire need of theoretical rejuvenation and integration.

A second pathology confronting organization theorists is the fact that the field has not succeeded in establishing a distinctive methodological tradition of its own. Neighboring disciplines like economics, political science, and sociology all have strong methodological roots, and have succeeded in developing disciplinary empirical research methods and techniques which can precisely address the specific research problems they have claimed for themselves. For example, economists have invested more than a century of methodological labor in the development of econometric techniques that are suitable for addressing problems of economic growth and economic trend analysis. Political science, similarly, has spawned qualitative comparative analysis (cf. Fiss, 2007) which has its roots in John Stuart Mill’s methods of comparison, and which is particularly suited to the comparative analysis of concepts like ‘democracy’ and ‘political revolution.’ Finally, sociologists have crafted both qualitative methods like ethnography, which is particularly suited for the study of complex social settings like large urban environments (Whyte, 1955), and quantitative techniques like network analysis, which has the unique capacity of analyzing webs of

asymmetric relationships between social actors. In sharp contrast, organization theory has generated no real methodological tradition of its own. There is no commonly agreed upon method for testing hypotheses related to organizational effectiveness that is capable of doing so under all the various conditions confronting modern complex organizations. At best, organization theorists specialize in adapting research techniques, which they 'borrow' from the aforementioned disciplines (and others), to suit their own research needs. What we should not obfuscate, however, is that this often results in makeshift and scientifically unsatisfactory solutions. In short, the inability of the organization theory field to produce the 'definitive' answer concerning organizational effectiveness is, at least in part, the result of methodological shortcomings.

A final pathology of the field is that it has fallen victim to several 'fault lines': geographical, intellectual, and methodological chasms which divide the organization theory field up into several separate communities of which the respective members have Granovetterian (1973) 'strong ties' amongst one another, but across which no fruitful exchange of thoughts seems possible. Several such chasms can be identified, but the most commonly recognized ones are the divide between US and European organization theorists (Mizruchi & Fein, 1993; van Witteloostuijn, 2008) and between qualitative and quantitative organizational researchers (Heugens & Mol, 2008; van Witteloostuijn, 2008). Mizruchi and Fein (1993) noted that US and European organization theorists tend to have rather different worldviews and study different research topics. US scholars purportedly emphasize the voluntary actions of organizational leaders, and downplay the role of power and coercion (Mizruchi & Fein, 1993). In contrast, European scholars appear to have a far greater appetite for the latter issues (Uzdiken & Pasadeos, 1995). Van Witteloostuijn (2008) notes that theoretical preferences similarly differ. He notes that many of the dominant theories in the field – such as resource dependence theory (Pfeffer & Salancik, 1978), upper echelon theory (Hambrick & Mason, 1984), and institutional theory (Meyer & Rowan, 1977) – are American inventions, whereas Europeans specialize in 'fringe' theories like staged internationalization theory (Johanson & Wiedersheim-Paul, 1975) and societal effect theory (Sorge, 1991). The second commonly acknowledged chasm is that between quantitative organizational researchers (who believe that organizations are best studied analytically and their properties best measured numerically) and qualitative researchers (who are convinced that organizations ought to be studied holistically and that the narrative structure of organizational life ought to be left intact and reproduced 'thickly' in our research accounts). As much as we would love to believe that these two

research traditions are complementary, contacts between the two associated research communities are sparse and are mixed-method studies are even sparser. Most disturbingly, perhaps, research findings by Heugens and Mol (2005) show that these two fault lines largely overlap: US universities tend to train, appoint, and tenure quantitative researchers, whereas European universities still overwhelmingly opt for qualitative researchers. Even though the two geographically separated groups may occasionally meet at professional meetings like EGOS¹¹ or the Academy of Management Meeting, they are unlikely to engage more than sporadically in shared intellectual debates as long as they continue to cling to their private research interests, theories, and methodologies.

¹¹ The European Group for Organization Studies (www.egosnet.org).

Strengths and strategies

So wither organization theory? Are these pathologies formidable enough to condemn the field to a position near the bottom of the organization studies food chain? Is there sufficient cause for self-loathing amongst organization theorists, and is it perhaps time for them to start thinking about an alternate profession? I would argue that this is certainly not the case, and that a recognition of the unmistakable pathologies of the field must go hand-in-hand with an acknowledgement of the field's formidable successes. And there are plenty of these to celebrate.

First off, the most prestigious journal in the entire management field, bar none, is the *Administrative Science Quarterly*. It does not have the highest impact factor of all management journals, it rarely publishes more than sixteen articles a year, it is strongly biased towards sociological theory, and it is not a general management journal like the *Academy of Management Journal* or the *Journal of Management Studies*, but everyone who claims that other journals in the management field rank higher in the commonly agreed upon status ordering of the field is, well, a stone-faced liar. It is, of course, a disciplinary organization theory journal. Furthermore, publishing management journals is incredibly profitable, and so dozens (!) of new journal titles appear on the market each year. Not surprisingly, most new management journal titles are discontinued again after a few years, and very few attain even the smallest modicum of academic prestige. In fact, only one journal that was founded in the last two decades managed to break into the traditional hierarchical apex of commonly agreed upon 'top journals,' an elite group usually seen to consist of *Administrative Science Quarterly*, *Academy of Management Journal*, *Academy of Management Review*, *Strategic Management Journal*, *Journal of International Business Studies*, *Organization Studies*, and *Journal of Management Studies*. This journal is *Organization Science*, and it is again a disciplinary organization theory journal. Finally, if the broadest possible community of management scholars would be asked to nominate the most influential scholarly books on management ever published, the following titles would almost certainly be included in the top-ten of the chart:¹² Cyert and March's (1963) *Behavioral Theory of the Firm*;

¹² Of course, a book's status is a highly intangible and ill-observable variable. Yet, consider the number of 'hits' on Google Scholar these books had accumulated per July 31st, 2008: Cyert and March: 5842; Lawrence and Lorsch: 2322; March and Simon: 5601; Mintzberg: 3244; Morgan: 3472; Pfeffer and Salancik: 3692; Thompson: 5839.

Lawrence and Lorsch's (1967) *Organization and Environment*; March and Simon's (1958) *Organizations*; Mintzberg's (1979) *Structuring of Organizations*; Morgan's (1986) *Images of Organization*; Pfeffer and Salancik's (1978) *External Control of Organizations*; and Thompson's (1967) *Organizations in Action*. Almost needless to say, each of these titles is in organization theory.

Strengths

Surely, if the field of organization theory is able to boast such successes in spite of being hampered by such clear pathologies, it must also feature some formidable strengths. What are these foundational assets upon which this field is constituted? I will identify two such strengths, notably its: (1) intellectual mobilizing potential and (2) ability to cope with theoretical and methodological pluralism.

A first, formidable strength of the field is that it centers on a set of questions that are so fundamental that they serve, in effect, as the Holy Grail of the management field: Why do organizations exist? Why are organizations structured the way they are? Why do organizations succeed or fail in the pursuit of their explicit goals? Each of these puzzles is exciting and important enough to arouse the interest of even the best and brightest. As DiMaggio (1989: 9) has put it: “Few questions bear more pressingly on our collective welfare.” It is by virtue of claiming what are perhaps the most basic and central questions in the management discipline that the organization theory field secures itself of a constant influx of new talent and of a high position on the agenda of general management scholars, journal editors, and university administrators.

A second strength of the field is its ability to cope with theoretical and methodological pluralism. In a sense, these qualities are the flipside of the field’s theoretical fragmentation and methodological division over qualitative and quantitative camps. Many organizational scholars have expressed the hope that the fragmented nature of the field could diminish over time through theoretical and methodological integration into a more confined set of distinct research paradigms (e.g., see Burrell and Morgan, 1979) or through paradigm interplay – the “simultaneous recognition of both contrasts and connections between paradigms” (Schultz and Hatch, 1996: 530). I think that this hope is in vain, but also that we should not despair. Although a somewhat greater degree of theoretical integration in organization theory would surely be desirable, a fundamental property of organizational life (as our object of study) is its theoretical and methodological pluralism. Organizational life is theoretically pluralistic in that it is so complex and multifaceted that it requires multiple theoretical explanations to account for its nature. It is also methodologically pluralistic in that its complexity also prevents it from being captured adequately by any single research method. I believe that the field of organization theory, more so than its sibling disciplines, is open-minded towards new theoretical and methodological approaches.

I also believe that organization theorists, who are professionally trained – in the parlance of Levi-Strauss – as bricoleurs, are often in a better position than most to address complex and opaque organizational problems with tailor-made (though rarely perfect) research designs. This almost innate affinity with the dazzlingly fluid and manifold manifestations of organizational life makes organization theorists the researchers *par excellence* to address the aforementioned towering questions about organizations. More importantly, I would like to propose that far-reaching theoretical and methodological integration, although aspired to by some, would in fact diminish the discipline's ability to satisfactorily address the overriding question of organizational effectiveness, as this would hurt its ability to cope with the pluralism inherent in organizational life.

Strategies

So far I have produced an inventory of the pathologies of the organization theory field, and pointed out two of its formidable strengths that help it compensate for its handicaps. Still, the field is partially in a state of permanent deadlock. Scott (1961) criticized the field well over four decades ago for being amorphous, auto-plagiarizing, and fad-like, and we have admittedly done far too little to counter his critique and similar critiques by others. This raises the following question: How can the field become more authentic and less obfuscating and fad-like? Or in other words: How can the field capitalize on its strengths in order to address its pathologies? These 'how' questions rightly point us in the direction of methodological and research practice-oriented solutions, and in the remainder of this address I will point out three 'meta research strategies'¹³ that, in my opinion, have the potential to help the field move further along.

These strategies are: (a) the discovery of stylized facts in organizational life (Helfat, 2007); (b) searching for macrosocial contingency variables that influence the tenability of all theories and foster the integration of certain theories (Donaldson, 2001); and (c) theoretical rejuvenation through the unpacking of organizational nuts and bolts (Elster, 1989). For each of these strategies, I will use examples from my own past, present, and future research agenda to illustrate how they work in practice.

A first meta research strategy I would like to propose entails the discovery of stylized facts in organizational life. One of the main reasons why the organization theory community continues to be trapped in the habit of 'reinventing' and 'restating' extant arguments (Scott, 1961) is that the field has a profound disdain for the identification of stylized facts, defined as 'observations that have been made in so many contexts that they are widely understood to be empirical truths, to which theories must fit' (<http://economics.about.com>). As long as we continue to put a premium on new theory proliferation (Hambrick, 2007), without always asking ourselves whether we can identify generalizable empirical truths which these new frameworks ought to take into

¹³ I call them 'meta research strategies' not out of a misplaced lust for big words, but because these research strategies are applicable to many different domains of content, even though I will illustrate them with specific examples from my own research.

account, we run the risk of being involved in the mass-production of “theory unhinged from reality” (Helfat, 2007: 187). So what does the discovery of such truths require us to do? First, we must encourage more descriptive work. Again in the words of Helfat: “In a field that seeks to understand the real world, it makes little sense to always put theory before the facts. We must understand at least the broad outlines of ‘what’ a phenomenon consist of before we try to explain ‘why’ it occurs” (Helfat, 2007: 185). No matter how commonsensical these words sound, they stand in sharp contrast to the common opinion in the organization theory field, which states that all empirical work should either aim for theory building or testing (Hambrick, 2007). Second, we should encourage more replication studies (Eden, 2002). The organization theory field almost certainly overemphasizes novelty. Journal editors and reviewers habitually reject fine studies on the sole basis that their underlying research question has already been addressed before by other authors. This practice discourages the discovery of stylized facts, as such empirical generalizations can only be established once a certain effect has been tested across a wide range of contexts and situations. Third, once a sufficiently large pool of primary replication studies exists, researchers should make every effort to uncover stylized facts through a synthesis of their findings. Such syntheses should preferably be quantitative in orientation (using various meta-analytical techniques), and draw on every bit of available information. Surprisingly, many meta-analyses of even the most fundamental theories in the field are only of recent date, and many sizeable bodies of literature remain ‘unsynthesized’ to this day.¹⁴ Fourth, and very importantly, researchers must begin to put organizational theories back into their hinges by taking these stylized facts on board in their new theory building and testing efforts. For example, a recent meta-analysis of the structure-agency debate in institutional theory has revealed that agents generally do experience the influence of social structure on their actions, but that the effect is generally weak (Heugens & Lander, 2008). Clearly, institutional theory researchers should be stimulated to build this ‘moderate agency’ perspective into their research designs, and abandon the radical

¹⁴ For example, consider the following literatures (author and publication date of first available meta-analysis given in parentheses): agency theory (Dalton, Daily, Ellstrand, & Johnson, 1998); bureaucracy theory (Walton, 2005); new institutional theory (Heugens & Lander, 2008); and transaction cost theory (Geyskens, Steenkamp, & Kumar, 2006). To date, the following literatures remain statistically unsynthesized: behavioral theory of the firm; population ecology theory; and resource dependence theory.

agency/radical structure observation plans which continue to inspire many institutional entrepreneurship and new institutionalism diffusion studies respectively. More in general, the point I wish to stress here is that many stylized facts in organization theory are yet to be discovered, and that the field might experience considerable progress if it were to acknowledge these facts in its research designs and observation plans.

A second meta research strategy I will touch upon here is the discovery of macro-social contingency variables that influence the (contextual) tenability of organizational theories (cf. Donaldson, 2001). What I call a macro-social contingency variable here is any variable that might be measured at the level of the macro-social unit (usually country or industry) in which a population of organizations is embedded, and which may affect the associational strength of commonly hypothesized relationships as they play out in that population.¹⁵ Identifying such moderators is important, especially in light of the various aforementioned (geographical) fault lines that cut across the organization theory field. As long as scholars work in geographically and intellectually isolated communities, there is a clear and present danger that they will develop theories that are less general in scope and orientation than they could have been. To be more precise, many organizational theories are developed in a single national context (usually the US; van Witteloostuijn, 2008) and tested almost exclusively on data that derive from that very same context. In doing so, researchers knowingly or unknowingly hold constant an entire matrix consisting of (but not limited to) cultural, political, economic, legal, and social background institutions which might critically affect the hypothesized focal relationship. Identifying these moderating factors is important because they critically affect the generalizability of our theories. Organizational theories are simply misspecified when they fail to acknowledge the institutional arrangements that enable or inhibit the focal relationships which they predict.

¹⁵ For a sociological account of how populations of firms adapt to local circumstances and in doing so change the properties of local institutional matrices and resource niches, see: Sorge (1991). He provides the following summary of his core argument: "Societal differences in organizing and generating human resources, and the pursuit of different business strategies, are reciprocally related. An economy and society becomes populated by specific institutionalized organizational and human resource forms and practices, because economic niches and business strategies are different, and vice versa" (Sorge, 1991: 163).

An example suffices to make the point. In a recent study, Heugens, van Essen, and van Oosterhout (2008) explored the associational strength of the ownership concentration to firm performance relationship across 11 Asian nations. This relationship is often predicted to be positive and significant, as larger owners are better monitors than dispersed owners, and they can thus better prevent managers from doing things with their money that go against their interests (also see van Oosterhout, 2008). Yet the authors discovered that this relationship only holds when legal background institutions have at least attained a certain threshold level of effectiveness, such that the large owners can take the managers of the firms they own to court with the justified expectation that this court will punish managerial misconduct and more generally uphold contractual agreements. Where courts are wholly ineffective when it comes to protecting business interests (such as in China, for example), firms with concentrated owners are equally worse off as firms with dispersed owners (Heugens, van Essen, & van Oosterhout, 2008). No study on a population of firms from a single jurisdiction could have systematically revealed the moderating effect of legal effectiveness on the associational strength of the concentrated ownership – corporate performance relationship. In more general terms, my suggestion is that the organization theory field can only begin to understand its contextual embeddedness and work on the generalizability of its theories when it starts to put more emphasis on comparative work and especially on the role of macro-social contingency variables. These variables should not merely be seen as boundary conditions to localized theories, but they should actively be built into our theoretical frameworks in order to produce more precise specifications and greater generalizability.

A third and final meta research strategy I discuss here is theoretical rejuvenation through the unpacking of organizational nuts and bolts (Elster, 1989). No matter how much credence we put in the foundational myth that the history of the organizational theory field began when sociologists started migrating towards business schools, it is a fact that a sizeable group of organization theorists consists of sociologically trained and inclined individuals. One 'nasty' habit the sociologists have brought to the field is the tendency to 'explain' macro-level outcomes (like organizational effectiveness, performance, founding, mortality, and reputation) by attributing them to macro-level antecedent factors (like organizational structure, culture, hierarchy, monitoring, and incentive systems). As Mizruchi and Fein (1999: 664) have noted, it is common practice in the organization theory field that: 'researchers are positing a particular process that results in a behavioral outcome, but they are measuring only the outcome while

assuming the process' (1999: 664). In doing so, they obfuscate the fact that in organizational settings macro-level antecedents are usually connected to macro-level outcomes through the intervention of human agents. I propose that organization theorists – especially bureaucracy, institutional, and population ecology theorists – ought to become more explicit about the micro-foundations of their work, in the sense that they should attempt to reshape their theoretical accounts in such a way that they become consistent with how human agents act, decide, and are motivated. This will inevitably involve the identification of what Elster (1989) calls 'nuts and bolts': chains of often small but interlinking mechanisms which trace and detail the process by which macro-level antecedents are connected – by human intervention and agentic behavior – to macro-level outcomes. Such theoretical rejuvenation is certainly due in population ecology and institutional theory, but I will focus on the case of bureaucracy theory. One of the enigmas in that field is how large complex organizations can develop and maintain a memory function in which they store and from which they retrieve information about their past actions. A classic insight is that they do so by encoding past experiences in organizational rule systems (Levitt & March, 1988). Of course, the prediction that new experiences will result in changes to the repository of organizational rules is a macro-macro argument. In a recent paper, Heugens and Osadchiy (2008) unveiled the nuts and bolts of this process by showing the types of agents involved in the process (organizational legislators and organizational subjects, which they call 'rule givers' and 'rule followers' respectively) as well as the various action alternatives that are open to these agents along the various stages of the rule life cycle (the sum total of which they denote as rule work). In doing so, they lifted the lid from the black box of organizational memorizing, if only by an inch or two. The more generic point which I wish to make is that many organizational theories are quite capable of *explaining* how certain macro-organizational phenomena come about, but they are simultaneously rather limited in their capacity to further our understanding of the underlying processes. Progress in the field of organization theory will critically depend on our willingness to explore the micro-foundations of organizational processes without surrendering the commitment to explore macro-level antecedents and explain organizational effectiveness.

Conclusion

The self-proclaimed goal of the organization theory field is to explain organizational effectiveness. If we judge its ability to meet this goal by conventional standards – does the field have a uniform theory for explaining effectiveness? Do its participants agree on a common ontology, epistemology, and methodology? – we can only conclude that it appears to fail miserably. It has produced and still produces a sheer endless array of effectiveness theories, most of which are merely variations upon older themes and rely on rather idiosyncratic combinations of background assumptions. Given that Scott already came to the same conclusion in 1961, even individuals with very moderate beliefs in evolutionary selection processes will find it hard to understand that the field even exists at all in this day and age. But the enigma is even greater. Being an organization theorist I will have to risk the accusation of chauvinism, but I believe that it is hard to deny that organization theory is one of the *most prestigious* of the organization studies disciplines, perhaps even of the management disciplines in their entirety. Whether we look at the standing of the journals and books the field has spawned, or whether we look at the representation of organization theorists amongst, say, general management journal editors and Fellows of the Academy of Management, it is evident that they hold their own. As argued, I believe that it is the magnetism of the type of questions organization theorists routinely ask as well as their great tolerance for theoretical and methodological pluralism that has kept university administrators, funding and accreditation agencies, and scholarly associations from cashing in their chips. But success on some parameters (such as prestige and mobilizing potential) in the face of poor performance on others (theoretical progress and integration) only perpetuates failure (DiMaggio, 1989). We certainly should not be content with this track record, but I believe that we should not despair. We should be able to improve on it, especially if we daringly ruffle our methodological feathers. Greater theoretical *integration* can be reached by exploring the hidden moderators that have produced contradictory research findings and even localized effectiveness theories across macrosocial contexts. Our theoretical *relevance* can be improved through the discovery of stylized facts, and by their integration into our extant explanatory frameworks. Finally, our theoretical *understanding* of organizational life can be increased by exploring the microfoundations of our macro theories, such that we can reach a better grasp of how social structure influences and is influenced by human agency. Progress on all three fronts will make the future prospects for the organization theory field very bright indeed.

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