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Dusty Books ?: the Liability of Oldness¹

Selznick, P. 1949. *TVA and the Grass Roots*. Berkeley, CA: University of California Press.

Burns, T. and Stalker, G.M. 1961. *The management of innovation*. London: Tavistock Publications. (1st edition, 1961; 2nd edition, 1966; 3rd edition, 1995).

Lawrence, P. and Lorsch, J. 1967. *Organization and environment: managing differentiation and integration*. Boston: Division of Research, Graduate School of Business Administration, Harvard University.

By C.R.(Bob) Hinings, University of Alberta, Royston Greenwood, University of Alberta and Renate Meyer, WU Vienna.

Books are foundational to management scholarship. Indeed, Suddaby and Quinn Trank (2013) go further and say that books are “arguably (the) *most* influential storehouse of academic knowledge in management”. We agree and hope to forward this line of reasoning with this essay. Our particular aim is to pique the interest of PhD students and early career academics in some of the foundational studies of our discipline, by showing what they could contribute to current concerns and issues. In particular, we highlight the importance of research monographs – books that are research studies, not books that attempt to summarize existing research. We believe that several of these earlier research monographs should not be left to molder in the library stacks. Instead we argue that they have been subject to selective attention and that there is much more to draw from them than received wisdom acknowledges. In short: reappraisals are warranted.

Having made the claim and hopefully shown that these studies retain relevance to day, three questions follow: First, why are they ignored? Second, is there anything about them that helps us

learn about how to do similarly insightful research. Third, why is research of this form not the model for today's efforts?

Various research monographs that were major landmarks some 50 years ago in establishing the study of organizations as a discipline continue to be cited, but most of the citations are to generic contributions rather than dealing with the specific research as a whole. To single out just three: Alvin Gouldner's *Patterns of Industrial Bureaucracy* (1955); Peter Blau's *The Dynamics of Bureaucracy* (1953); Michel Crozier's *The Bureaucratic Phenomenon* (1963) were foundational studies that have shaped our discipline. Other texts that, in their time, were also highly regarded now seem to have less influence and remain on the library shelves, possibly rightly so (e.g., Rice, 1958; Jacques, 1951). But given the importance of such works to the development of our discipline, and their comprehensive conceptual and empirical nature, our interest is in whether important insights and nuances are being missed in common reception. Can lessons that are relevant for today's concerns still be learned from them, or is honorific and limited citation their appropriate legacy? As Robert Merton said, we all stand on the shoulders of giants in order to take our ideas forward, but perhaps those shoulders are even broader than typically acknowledged. Our suspicion is that there are important insights to be learned from many early works and that better familiarity with them would obviate our tendency to 'reinvent' them anew.

Some re-examination has already occurred. Spearheaded vigorously by Matt Kraatz (Kraatz and Block, 2008; Kraatz, 2009; Kraatz and Flores, 2015) there has been a particular rediscovery of the work of Philip Selznick. As institutional theory has moved from a concern with diffusion and isomorphism to interest in the processual dynamics of institutional change, Selznick's *TVA and the Grass Roots* (1949) and *Leadership in Administration* (1957) have been re-introduced. Selznick was 'rediscovered' mainly because of his presumed relevance to power and politics, the study of

organizations as institutions, and organizational leaders as institutional guardians. But, as we will show in a moment, there is much more to be explored in his comprehensive oeuvre. More broadly we argue that what has been done with Selznick's work can and needs to be done with other important figures and their foundational contributions.

To make our point we focus upon three classic books of our discipline. We will begin by revisiting Selznick's *TVA and the Grass Roots: a study of politics and organization* (1949) which addresses issues of corporate cooptation, and the role of particular interests. Then we turn to Tom Burns and George Stalker's *The Management of Innovation* (1961) not only because of its emphasis on innovation, but especially because of its concern with organizational politics and dynamics. No doubt, this book is a cornerstone of the 'contingency' approach, but could (and, as we argue, should) also be read from the power-process and micro-political perspective that Burns developed all his life (e.g. 1961; 1977; Burns and Flam, 1987). Paul Lawrence and Jay Lorsch's *Organization and Environment: Managing differentiation and integration* (1967), another 'icon' of contingency theory is the final book, chosen for what it has to say to the current interest in organizational responses to multiple logics. These texts have received significant recognition – at the time of writing, according to Google Scholar, Burns and Stalker's work had gained 14,470 citations, Lawrence and Lorsch 14,019, and Selznick's *TVA* 3,768.

Each of these texts has a surface resonance with some of today's themes within organization theory, which makes them appropriate for our purpose of directing attention to them. A second reason for revisiting these particular texts is that they are detailed case analyses that at the outset were not theoretically but problem driven. Selznick was examining a New Deal organization that ran counter to the political culture of the time, and that was therefore subject to tension and conflict. It was essentially a politically charged organization. Burns and Stalker's study arose from an evaluation of a

Scottish regional economic policy that was attempting to deal with dying industries. Lawrence and Lorsch saw themselves as dealing with fundamental issues of managing complex organizations. Given the scale of the issues they were addressing and their comprehensive empirical studies, the format used by all of these researchers was the research monograph – a format that provides the space to develop a wealth of ideas. This is refreshingly old-fashioned and sweepingly symphonic in a time when ‘soundbite’ or ‘salami slicing’ (Greenwood, 2016) research strategies induce scholars to split their work into the maximum number of possible individual publications – on which we say more later.

A third reason is prompted by Golden-Biddle, Lock and Reay’s (2007) analysis of how citations are too frequently used. Their analysis shows that the obsession with ‘theoretical novelty’ required by today’s journal review process results in citations that utilize only those parts of a publication that speak to the citing author’s particular purpose. It is what they call ‘typification’ where citations refer to a small part of the asserted knowledge claims in the original research. Such typified citations place the focal work within a particular category of work – e.g., contingency theory, organization structure, institutionalized organizations – and blind out the broader, more nuanced story.

But then, these initially selective ways of citing, suggest Golden-Biddle, Lock and Reay, become institutionalized and, over time, honorific. Readership and numbers of citations start to fall apart – not, as could be imagined, in the sense of more readers than citations, but rather the reverse. Honorific, typified citations lead to isomorphic narrowing of knowledge accumulation as subsequent citations centre around the same limited idea of the research that is being handed down over decades. The result is that the initially ‘missed’ material and ideas remain lost – especially when the works cited are monographs that contain multiple and interwoven ideas (we return to this point later). We argue that this is exactly what has happened with the books that we review.

What follows is a discussion of how each of the three books still has considerable relevance for today's scholarship in ways that are off the beaten track of their 'standard' reception.

Philip Selznick

Philip Selznick was 30 when *TVA and the Grass Roots* was published in 1949; it was his PhD thesis from Columbia. How many of us have produced a seminal work from our PhD, especially as a research monograph, that continues to inform our discipline's thinking six years after his death at the age of 91? Selznick spent his academic career at the University of California, Berkeley, as professor of sociology and law. His work was groundbreaking in its ideas of institutional leadership (*Leadership in Administration*) and in its understanding of moral aspects of managing and studying organizations (e.g., *The Moral Commonwealth*).

Of the three authors we are reviewing, Selznick is the one who has continued to have the strongest substantial presence in ongoing organizational theories, especially in institutional theory even though he is, overall, the least cited of the authors we are reviewing, according to Google Scholar. We believe that this is linked to the 're-emergence' of institutional theory as a dominant approach. Yet, as Scott (2014) points out, while Selznick's presence has been centered on two of his works, *TVA and the Grass Roots* and *Leadership in Administration*, even those have been utilized in very limited ways. Hence, while recent scholarship has included these two books as part of a revived neo-institutionalism (e.g., Kraatz and Block, 2008; Kraatz, 2015; Kraatz and Flores, 2015; also, Washington, Boal and Davis, 2008), there is more in these works that deserves attention.

Selznick himself (2000: 277) commented on what he was trying to do in his study of the Tennessee Value Authority (TVA), a government-owned agency in the electricity sector that had the explicit aim of stimulating local participation in its policies and to promote a decentralized decision-making style – hence 'grass roots'. For Selznick, it was a story about the adaptation of an organization to the

threats and pressures that it faced. In particular, the story dealt with vested, organized interests and the extent to which TVA surrendered to those interests. Selznick was particularly focused on the ways in which the adaptations changed the character and purpose of the agency and, in his terms, not for the better. Prima facie, this is much more than what we currently take out of *TVA and the Grass Roots*. Selznick also saw *Leadership in Administration* as a chance to do some reanalysis of what he had written in *TVA*.

Fueled by the shift in neo-institutional theory to a concern with processes and dynamics, *TVA and the Grass Roots* and *Leadership in Administration* have received increasing citations but, as Washington et al. (2008) point out, such citations are usually to support a notion of institutions as infused with value – a clear example of Golden-Biddle et al.'s (2007) typified citation. But Selznick's work is itself infused with a central interest in power, politics, organizations as institutions, and leaders as institutional guardians. As Scott (2014: 273) observed: “the early thrust of his efforts appeared to focus on the “dark side” of organizations – the forces undermining their original mission [...]”. Indeed, the subtitle of *TVA* is ‘a study of politics and organization’ and as such, it speaks to issues of power, corporate cooptation, the role of vested interests, and corporate corruption.

All of these issues have resurfaced in the last decade as important issues for organizational analysis – often with the lament (ironical in our view) that these darker facets of organizations have been neglected. Fligstein and McAdam (2012), for example, reiterate a longstanding critique that the role of power has been completely underestimated in institutional theory and that there is little emphasis on actors with interests, resources and positions that help determine allocation. Lawrence (2008), similarly, suggests that there has been too much emphasis on institutional control and not enough on the ways by which actors avoid control and resist institutional pressures through the power that they can activate. And more generally, the criticism has been made that power and politics are the

“ugly duckling” of organizational theorizing and too often missing (Czarniawska-Joerges & Jacobsson, 1995: 375). We are told that “[p]ower is rarely, if ever, discussed in the management of change literature” (Mills, Dye & Mills, 2009: 139; see also Hardy and Clegg, 2006).

Our point is that much of this presumed neglect results from selective reading of foundational works. In *TVA* in particular, but also in *Leadership in Administration*, Selznick actually has much to say on issues of power and politics. *TVA* is about the ways in which external constituencies pressured the organization to recognize their interests, and how in responding to those powerful interests the TVA’s original goals were deflected. Cooptation of external interests into the leadership structure of the TVA undermined the organization’s original goals. But the sharing of power with such interests had to be kept informal because there was a degree of illegitimacy in yielding to those pressures. Selznick offers the following insight: cooptation that “results in an actual sharing of power will operate informally, and cooptation oriented toward legitimization or accessibility will be done formally” (Selznick, 1949: 260). This insight – that formal cooptation is often “the sharing of public symbols or administrative burdens of authority and public responsibility, but without an actual transfer of power” (ibid.) – could painfully inform current studies on, for instance, diversity, corporate responsibility, accountability, transparency, or participation. Moreover, as with so many writers of the 1950s and 1960s Selznick emphasized how much organizations matter because they are the primary tools of industrial society. But, they are imperfect tools especially because interest groups can hijack them for their own goals.

Selznick’s concerns with organizational power and politics, the role of organizations in society, and the consequences of their activity, especially when taken over by partisan interests and manipulated by elites, fits with emerging interests in organizational misbehavior, corruption, organizational political activity, ideologically driven organizations, or the diversion of organizations from their

stated aims (e.g., Hillman & Hitt 1999; Hillman, Keim & Schuler, 2004; Palmer, 2012). The cases of Enron, Worldcom, and Goldman Sachs, reaffirm the observation that organizations are tools that can be hijacked by interest groups and that they can serve as weapons. Selznick is a warning voice that though organizations are actors, we should not divert our attention away from questions about whose purposes they serve in the end.

Tom Burns and George Stalker

Rather like Selznick, Burns and Stalker were concerned with a policy issue – the attempt after World War II to develop an electronics industry in Scotland and northern England as part of the post-war reconstruction. At that time, television, air travel, nuclear power and electronics were all new, increasingly becoming mass products and services. As a result of such developments, organizations faced the necessity of profound change – they had to learn how to do new things. In the context of the electronics industry this involved, for example, attempts to turn organizations that had been producing wooden railway ties into producers of components for newly developed computers. In the language of Burns and Stalker, their study was about the experience of firms familiar with ‘mechanistic’ organizational arrangements attempting to become more ‘organic’.

The Management of Innovation (1961) captured those experiences. The book has been very widely cited and largely interpreted as being about the differences between mechanistic and organic organizational forms, and of their relationship to different environmental circumstances. We believe that this is a misinterpretation as (in our view) the book is actually about organizational change and the difficulties of accomplishing it. Of course, the distinction between mechanistic and organic forms was a major breakthrough in understanding how innovations are possible and it rightly became an important inspiration for thinking about that issue. It is a distinction that continues to resonate with organizational scholars today (e.g., Sine, Mitsuhashi and Kirsch, 2006). But it is very

interesting to read Tom Burns' own opening statement about the findings of the research: "Technical progress and organizational development are aspects of one and the same trend in human affairs; and the persons who work to make these processes actual are also their victims" (Burns and Stalker, 1961: 19). Does that sound anything like the Burns and Stalker we know in organization theory?

Tom Burns was 48 when *The Management of Innovation* was first published in 1961. He died in 2001. Between 1939 and 1945 he served in the Friends' Ambulance Unit and was a prisoner of war in Germany between 1941 and 1943. Immediately after the Second World War, from 1945 to 1949, his interests were in urban sociology and he worked with the West Midlands Group on Post-War Reconstruction and Planning. He became a lecturer at the University of Edinburgh in 1949 where he remained until his retirement. He was the founder and first chair of the sociology department. As with Selznick, and as his career prior to *The Management of Innovation* shows, Burns had a constant interest in the ways in which the study of organizations could inform policy. He expressed his approach to research in the preface to the second edition of *The Management of Innovation*: "by perceiving behaviour as a medium of constant interplay and mutual redefinition of individual identities and social institutions [...] it is possible to begin to grasp the nature of changes, developments and historical processes through which we move and which we help to create" (Burns and Stalker, 2nd edition, 1966: xxx).

We are not saying that Burns and Stalker didn't rightly become famous for their distinction between mechanistic and organic systems, but there is so much more than that to *The Management of Innovation*. Even at the level at which it has become part of the lexicon of organization theory, many nuances of their argument have been overlooked. For example, Burns and Stalker emphasize that "the two forms of system (not structure, our insert) represent a polarity, not a dichotomy; there are, as we

have tried to show, intermediate stages between the extremities empirically known to us. Also, the relation of one form to the other is elastic, so that a concern with oscillating between relative stability and relative change may also oscillate between the two forms” (1961: 122). They continue by emphasizing that an organization “may (and frequently does) operate with a management system which includes both types” (ibid.). Where do we find systematic discussions of organizational oscillation and the operation of dual or more management systems? Maybe with their analyses Burns and Stalker have preempted organizations now labeled as ‘hybrid’ and the impact of multiple logics on them?

Another important yet neglected concern within Burns and Stalker’s discussion of mechanistic and organic systems is that of commitment. All organizations demand commitment from their members; indeed they cannot operate without it. In organic organizations, however, the lesser emphasis upon formal rules and demands is compensated for by an emphasis upon shared beliefs and values. Organic organizations demand extensive commitment and the individual must be prepared to ‘yield himself’ (stet) as a resource whereas mechanistic organizations can operate with relatively low levels of member commitment. Not only does this idea provide quite a different take on the ‘iron cage’ of bureaucracy (the visible bars of the bureaucratic cage versus the invisible workings of ideological control), it is critical because of the way it links self-interest, organizational politics, innovation, and organizational change – themes foundational for micro-political approaches that regard organizations as multiple, interlocking power games (e.g. Crozier and Friedberg, 1979; Küpper and Ortmann, 1988). Read from such a power-process perspective, *The Management of Innovation* seems to be a completely different book from the one we find presented in standard organization theory textbooks.

Much of *The Management of Innovation* is dedicated to issues of power and status because of the way in which they are related to organizational change. Those issues were a lifelong interest for Burns. As he put it in his wide-ranging preface to the 3rd edition of the book: “People who work in firms are at one and the same time co-operators in a common enterprise and rivals for the material and intangible rewards of competition for advancement” (Burns, 1995: xvii). Indeed, Burns saw the internal politics and power struggles involved in organizational change as sometimes enabling radical change, and at other times as leading to defensive, resistant and obstructive strategy and tactics. These ideas of the intertwining of politics, status and careers were so important to him that in 1977 he published *The BBC: Public service and private world*, following and developing these ideas further. Interestingly, this book, like Selznick’s *The Organizational Weapon* (1951), made no impact on organization theory, perhaps because by then the journal article was becoming the dominant form for reporting research or perhaps because both books were too political to be picked up in Business Schools.

In examining issues connected with radical change, Burns and Stalker emphasized two aspects. First, organizations are made up of intertwined systems that make change difficult, including technical, political, status and career systems. Second, because of such systems (and their institutionalized nature) mechanistic organizations find it particularly difficult to accomplish radical change; indeed, these organizations unwittingly invoke ‘bureaucratic pathologies’ – i.e., they introduce additional bureaucracy that, though intended to help them change to more flexible and innovative organizations, actually makes them less able to do so. Crozier (1963) later included this paradox in his ‘circulus vitiosus’ of bureaucracy. Indeed, it is clear that Burns and Stalker doubted whether planned, conscious change could succeed when it was radical (echoes here of the ecology studies that appeared a decade and a half later).

Burns and Stalker argue that mechanistic organizations are made up of groups and departments with relatively stable career structures and sectional interests that lead to power struggles in any change process as groups struggle to gain control of new functions and new resources. These organizations cope with the challenge of change by introducing more specialized positions to handle the change and by giving them hierarchical authority. Such expansion of roles leads to more and more complex and ambiguous authority relationships – a ‘pathology’ distinctive of mechanistic organizations. A further pathology Burns and Stalker identify is the ready introduction of committees (definitely not teams). As Pugh, Hickson and Hinings later put it, these pathological systems “are attempts by mechanistic organizations to cope with new problems of change, innovation and uncertainty while sticking to the formal bureaucratic structure” (1985: 33).

To what extent, then, does the *Management of Innovation* inform contemporary organization theory? Based on the way that it is usually cited, the answer is: only very modestly. Rarely do scholars consult the book for its insights into change. Nonetheless, in the light of what they have to say about the difficulties that organizations experience in becoming more innovative, the ideas of Burns and Stalker are still highly relevant to today’s problems – perhaps especially for understanding the challenges of moving from exploitation to exploration.

The dynamic, processual nature of the Burns and Stalker study, the ideas of sectional interests, organizational politicking, system interrelationships and the resulting pathologies provide a far livelier and ‘inhabited’ view of organizations than today’s portrayal of contingency theory (with which Burns and Stalker are typically associated) would have us believe. The idea of bureaucratic pathologies highlights the difficulties organizations face in changing their ingrained practices. ‘Normal’ procedures are followed as ways of dealing with new situations; old solutions are applied to novel problems. In fact the extent to which many studies on institutional change echo such insights

without pointing to the similarities or using the language and examples of how to think about such difficulties provided by Burns and Stalker is quite astounding. For us, their concept of ‘pathology’ is especially worth exploring further as it offers a distinct way of theorizing possibilities and obstacles of change that avoids both overly agentic and deterministically structural accounts.

Paul Lawrence and Jay Lorsch

Paul Lawrence, who died in 2011, was Professor of Organizational Behavior at the Harvard Business School. He started his lifelong academic career at Harvard University in 1947. Jay Lorsch is the Louis Kirstein Professor of Human Relations at the Harvard Business School where he has spent his academic career since receiving his DBA in 1964. Lawrence was 46 and Lorsch ten years younger when *Organization and Environment* was published in 1967. The book received the Academy of Management’s “Best Management Book of the Year Award” in 1969. It was this book that added the term contingency theory to organization theory’s vocabulary.

The starting question for Lawrence and Lorsch was ‘What kind of organization does it take to deal with various economic and market conditions?’ The emphasis on the processes of ‘differentiation’ and ‘integration’ that followed, and that has been handed down in a wide variety of texts (e.g., Donaldson, 2001; Scott and Davis (2011), is a way of describing the structures, systems and actor characteristics that enable organizational effectiveness when *an* organization is facing a range of different environmental conditions. The research that produced *Organization and Environment* was carried out in ten firms in three different industries – plastics, food and containers. The firms in each industry were chosen for the very different environments that they faced.

Organization and Environment has a distinct managerial focus – much more so than the books of Selznick or Burns and Stalker. Starting from the observation that organizations internally need to mirror the degree of differentiation of their environments, the central theme of Lawrence and Lorsch's landmark study was the relationship between the two essential complementary organizational design processes – 'differentiation' (the allocation of tasks and responsibilities between organizational subunits/departments) and 'integration' (achievement of coordinated collective effort of the differentiated parts). For Lawrence and Lorsch, these processes are 'essentially antagonistic' and successfully undertaking them constitutes a central challenge for managers. Following the (then) dominant line of theorizing, Lawrence and Lorsch observed that in successful firms the differences in environmental and task uncertainties faced by subunits could and should result in potentially very different ways by which those subunits are structured and managed. The ability to enable and respect differences between the working styles of departments is thus critical – it allows each unit to perform its responsibilities in distinct ways.

This recognition of intra-organizational differences distinguishes the study from, for example, Burns and Stalker (1961). But how should the centrifugal implications of intra-organizational differentiation be overcome in order to achieve coordinated action? Lawrence and Lorsch observed various integrative arrangements being used in their sample of firms – from hierarchy, through use of project teams and/or integrators, to carefully designed incentive systems. The important finding was that in successful organizations the selection *and* the range of integrative devices varied according to the extent of differentiation. Put simply, Lawrence and Lorsch found that the higher the level of differentiation, the greater and more complex should be the range and forms of integration. Poorer performing organizations were those that either had too much or too little integrative effort, and the wrong forms of integration. Balance between differentiation and integration was essential.

In many ways, the contingency approach to understanding organizational design has been criticized and discredited (although see Van de Ven, Ganco and Hinings (2013) for a reassessment). Nonetheless, the core theme of *Organization and Environment* – the challenge of resolving ‘essentially antagonistic’ relationships between core organizational elements – still resonates with central issues in organizational research such as, for example, the current interest in ‘ambidexterity’ (e.g., Gibson & Birkinshaw, 2004; O’Reilly & Tushman, 2008) or in ‘hybrid organizations’ and especially with ‘structurally compartmentalized hybrids’ (Battilana & Lee, 2014; Battilana & Besharov, 2017). While the predominantly strategy oriented literature on ambidexterity mostly acknowledges, at least in passing, the legacy of Lawrence and Lorsch, neo-institutional scholars investigating hybrid organizations seem to be far more hesitant to embrace this heritage. Yet, the Lawrence and Lorsch study offers significant insights into the unfolding understanding of organizations that operate in institutionally pluralistic contexts.

To start with, their study exemplifies that in studying how organizations respond to multiple logics we should seek a more ‘complete’ story, in two ways. First, we need to consider efforts at integration as involving multiple aspects of design and managerial actions, including the structural aspects of organization design, styles and requirements of leadership, and incentive systems. For Lawrence and Lorsch it is the overall pattern (rather than a single component) that may be critical for understanding how organizations are managed. Looking at and analyzing the working of the individual parts is important (and, perhaps unfortunately, is often driven by the length restrictions of the journal format), but we need to also embrace a more encompassing approach. It is the range of integrative structures and arrangements and their interrelationships that matter – not any one of them alone.

The second aspect of the ‘complete’ story that Lawrence and Lorsch prompt us to pursue is the emphasis upon an appropriate *balance* between any given level of differentiation and the scope and combination of integrative devices. This idea of balance has been relatively ignored in the research on how organizations deal with multiple logics. The possibility that the degree of differentiation between logics can range from low to high and that this would affect if, and how, they can be integrated, has received very little (if any at all) attention. Instead, in most studies on hybrid organizations, logics are defined and treated as though they are incompatible or at least at odds. Yet, the Lawrence and Lorsch study would prompt us to ask just *how* differentiated logics are, and whether certain pairings of logics are particularly incompatible, complementary, or neutral towards each other. Lawrence and Lorsch’s strong stance against the idea that one set of integrative structures might fit all degrees of differentiation and their insight that too much integration is as much a source of organizational failure (similar to Burns and Stalker’s pathologies) as is too little should encourage neo-institutional researchers to explore the varied repertoires of forms and practices that emerge, stabilize and eventually become institutionalized.

Another inspiration we draw from *Organizations and Environment* relates to the question of time and temporality. Lawrence and Lorsch define differentiation in terms of four dimensions – the ‘formality’ of a subunit’s structures (e.g., whether mechanistic or organic), the ‘time orientation’ of the work involved (whether implicating long or short term performance feedback), management style (which they label ‘interpersonal orientation’), and ‘goal orientation’. While these are not necessarily the most critical dimensions today, one in particular prompts reflection – the notion of time orientation. Maybe a useful but neglected way of reflecting upon institutional logics and hybrid organizations would be to consider their different implications not only for what is done in order to gain social endorsement (which is the emphasis of much work on hybrids) but to think about the implications of different logics for *how* work is done and the extent to which particular pairings of

logics are especially problematical when they occur together. Further, the idea that institutional logics might have distinctive temporal rhythms (an idea that has not been picked up) is intriguing. Much as we are beginning to recognize that logics have their own ‘aesthetic’ (Jones et al., 2017) and ‘emotional registers’ (Toubiana & Zietsma, 2016), it might be instructive to explore whether they also have their own ‘temporal registers’.

Lawrence and Lorsch focused upon the use of integrative arrangements to countervail the centrifugal impulse of differentiation. But they acknowledged that even where an appropriate balance between differentiation and integration is achieved, instances of conflict are still inevitable and will characterize all organizations. Managing differentiation and integration, therefore, is more than putting into place structural bits and pieces. It is also about handling the outbursts of conflict and contestation that inevitably will occur where ‘tectonic plates’ meet. The lesson that we draw is that in studying hybrids we should give more explicit attention to the management of conflict. By emphasizing the structural arrangements that ‘satisfy’ external audiences we have learned much about how hybrids gain legitimacy and might prosper. But we have done little to explore how ‘outbursts’ and breakdowns within hybrid organizations are addressed and resolved. It’s one thing to please external audiences, but another to satisfy the drives and motivations of committed actors within the organization. This is a missing and important piece of the story.

In some ways the works of Philip Selznick, Tom Burns and George Stalker, and Paul Lawrence and Jay Lorsch are very different, but there are nonetheless some shared attributes. They all arise from close and detailed examination of real-life organizations – there is a very distinct sense in reading these works that the authors ‘know’ the objects of their examination. They all treat the organization as a whole rather than focus upon selective parts of it. They all highlight the political dimension of

organizational life. And, they all have continuing relevance to several of the important issues being addressed or raised today. These are not dusty books that should be relegated to the bookshelves for access only by historians. On the contrary, they should be making an ongoing and active contribution to our discussions. We have chosen three books examples, but, naturally, there are many other classics that deserve revisiting. At the beginning we singled out Gouldner's *Patterns of Industrial Bureaucracy* (1955); Blau's *The Dynamics of Bureaucracy* (1953); and Crozier's *The Bureaucratic Phenomenon* (1963) as foundational research monographs. We suggest that these are worthy candidates for renewed appraisal (see e.g., Hallett and Ventresca, 1996 on Gouldner).

This takes us back to our opening questions: Why are these foundational texts ignored? Is there anything about them that helps us learn about how to do similarly insightful research? And, why is research of this form not the model for today's efforts?

Three questions

Let's take the second question first. As we noted a moment ago, there are two aspects of the above texts and the research reported within them that are worth noting. First, they share a holistic approach to understanding organizations; and second, they exhibit a deep understanding of the organizations studied. The holistic approach is something that has been lost in many organizational studies. Yet, these texts continue to offer insights because they are concerned with 'the organization' as the unit of study, and deal with systems, structure, politics, power, change, status, and careers – as a totality. For Burns and Stalker, for example, organizational change, organizational politics, organizational commitment, etc., can only be understood and explained in relation to the particular type of organization which, in turn, is embedded in a particular type of environment. The penultimate sentence in *The Management of Innovation* says, "This book has, in fact, dealt with an array of internal manifestations of the external tasks and problems, and of changes in their disposition,

which affect the existence of the concern (organization, our insert) as a whole” (Burns and Stalker, 1961: 262).

There is, too, a deep understanding of the organizations studied. In all the texts the particular analytical points and theoretical developments are amply illustrated through epic presentations of the empirical basis for any assertions. What these various organizations do and how they do it is elaborated in depth because the authors spent considerable time in each organization interviewing as many people as possible in managerial and supervisory positions – all told, for example, some 300 people were interviewed by Burns and Stalker. This, clearly, is what Barley (2008) calls ‘coalface’ work. It is this deep understanding of the organization that makes the theoretical statements robust and it is a large part of the reason why these books have become classics of organization theory.

So, now to the first question – Why are these texts largely ignored? By which we mean why is their relevance to today’s issues rarely seen? One reason, mentioned above, is that journal articles typify research to the point where the original content of the research becomes institutionalized in a misleadingly narrow way (Golden-Biddle et al., 2006). Today, we work on the assumption that recently published works (usually articles) appropriately signal what has gone before. But, as we have sought to point out, that’s a mistaken assumption.

There is another aspect of this liability of oldness. Today there is such an overwhelming volume of research papers being produced that some filtering mechanism is necessary in order to avoid being overwhelmed. We can’t read everything, so we need some guidance. One such mechanism is the comforting myth that published research *is* cumulative and takes appropriate account of previous work. But we are arguing that this is not the case. The consequence is that books inappropriately become dustier and dustier and ignored.

Perhaps the journals (and reviewers) are also partly responsible because they/we allow the use of citations that are primarily about displaying that the author rather broadly ‘knows’ the field. Yet, as Golden- Biddle et al (2006) show, too often the reference is overly vague and (we think) perhaps the author does not really know the scope and coverage of the cited work – especially if they are older books and younger authors. Because of the emphasis on ‘novelty’ it is important to establish what is ‘new’ in an article, which encourages the selective citing of previous work. Dealing with the literature in the analytically summary fashion required for publishing in journals is quite different from the way in which it can be approached for a research monograph.

It could be argued, of course, that we as academics should be reading these books during our doctoral training and that all of us in that blissful and overly brief moment should be socialized to appreciate the value of research of that nature. But are we? Do we demand that our students properly dig into these foundational works? Or do we engage too much with more recent papers that give often vague, sometimes even misleading clues on what has gone before? Do we suffer from an unhealthy ‘obsession’ with the recent and the novel? Do we on the one hand, profess that the apprentices to our profession should know the history of their subject, and yet on the other hand give opposite signals by encouraging theses comprised of ‘three papers’?

Our third question asks why research of this form is not the model for today’s efforts instead of the emphasis on articles and getting out as many papers as possible from a particular piece of research. As we have already said, it is a hallmark of our dusty books that the timescale for the research was long and the researchers took time and effort to understand the organizations that they were studying; they created their own databases. The researchers were working at the ‘coalface’ of the organizations. Churning out papers doesn’t always fit easily with this style of research. Moreover, the rise of ranking systems (by, for example, *The Financial Times*) and of national assessment exercises (as

in the UK) that use journal publications as metrics of individual scholar's and institutional performance, further encourages this world of salami slicing of research. Isomorphism is strong!

It is not the case, of course, that books are not written any more, but that research monographs are no longer salient in our field. Books that win awards are often not research monographs. Moreover, they tend to be written later in scholars' careers when, having established a journal track record, synthesis and broader statements can be produced without incurring adverse career consequences. We believe that research monographs can be important initiators of subsequent journal articles that develop the theoretical ideas contained in such monographs. An example from our own work is the way in which Hinings et al. (1988 inspired much of our subsequent thinking on radical change that was later published in journals (e.g. 1993 and 1996). Our point is that there can and should be a dynamic relationship between research monographs and journal articles. Today, journal articles have replaced monographs so it become much more difficult to bring the 'slices' together.

In conclusion, we lament that some of the great works of our discipline still have insights of relevance and yet are largely ignored. Our starting point, as we said, has been to pique the interest of doctoral students and junior faculty and to nudge them to revisit these (and other classic) texts. We have also pointed our fingers at some of today's practices that contribute to the liability of oldness that seems to permeate our world. Food for thought?

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