

Part VII
Endnote

PROOF

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Essaying history and management

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Introduction

Students new to management might assume that it has a brief but glorious history, one in which the initial ideas of foundational thinkers such as F. W. Taylor, were slowly amended and corrected, in which ideas evolved in a Whiggish way, mounting up to the truths of today – or at least those embraced in the last few years' issues of the key journals that impact factors dictate.

In terms of the caricature, which accentuates elements of an all too pervasive reality, history, where it enters the textbook frame, usually consists of chronological studies in an evolutionary mode; a scientific parceling out of history's errors; the occasional retrieval of forgotten voices from the past, such as Follett or Deming; studies uncovering examples from the past to serve as benchmarks for the future along with studies aimed at distinguishing the eternal hard core of knowledge from the flotsam and jetsam of the moment. It should be evident from the outset that the present volume sets itself against such caricatures and is to be regarded as a contribution to the emerging turn to history in management and organization studies. I shall concentrate in what follow on the historiographical element of the various contributions that make up this *Companion to Management and Organizational History*.

The historic turn in management and organizational studies

The rather mundane ideas of Taylor, popularizer of popular mechanics, have played a disproportional and dismal role in management history, as Novecivic, Jones, and Carraher's critical analysis of Wren's foundational history discusses. Whatever the canon of management history it is a safe bet that Taylor will play a celebrated role in it – either as here, or less often, as in Braverman (1974), villain. This celebratory individuation dismisses the extent to which figures such as Taylor were more or less emblematic of deep-seated currents of social thought and functioned in the role of a public intellectual in advocating them. The constitution of canonical texts always privileges the individual and the text to such an extent that the wider structural factors at play are written out of the history at worst, or, at best marginalized (Clegg and Dunkerley 1980).

These factors were not neglected in the 1970s renaissance of Marx's labor process theory occasioned by the success of Braverman's (1974) book and the many labor process studies that

ensued. However, as Kieser notes, the enthusiasm for labor process theory had very little impact outside of the converted – by and large, mainstream organization theory simply ignored its existence. Its Marxist auspices did not fit well in the Business School frame nor, perversely, despite its avowed historical perspective, did its historical lack of specificity endear it to non-Marxist historically inclined scholars. General histories make abstract patterns into which many diverse histories have to be molded, doing violence to specific forms of variation and local patterning, industrially, regionally, nationally and cross-nationally, a point that Kieser makes clearly. He also makes the point that the very notion of a national culture or an organizational culture, in the singular, must always be an ideological gambit because its construction depends on suppression of difference. Hence, in looking at the development of management practices across nations it is far better to focus on some middle range empirical indicators, he suggests.

Historical analysis is antithetical to parodies of covering law-like explanations whether these derive from Marxist or positivist ambitions. Such ‘explanations’ cover more than they reveal and must always downplay the circumstances in which people make choices, albeit not often those of their choosing, in order to make those elegant patterns that straddle and shape specific histories. Of course, this does not mean that there can be no history other than the infinitesimal compilation of narratives; these must always be arranged in patterns if understanding is to flourish. But the patterns are neither invariant nor eternal: as Kieser writes, there is evolution of evolutionary mechanisms. Several of these are put to the test in explaining the development of the German putting-out system and all are found wanting when confronted with the specificities of the historical record. As Kieser states “it is probably always possible to select historical facts that fit any general model, but one can never be sure that the researcher is not leaving out important facts that contradict it.” Following an inductive strategy in regard to the known historical data is, he suggests, the best way to increase if never complete surety. Twenty years after writing the article that is reproduced as a chapter, Kieser has little to add, except irony.

Manifestos of historical purpose are not unknown amongst those who would write (and sometimes make) history and Booth and Rowlinson offer us a ten-point example of the genre, styled as an agenda. The agenda is wide ranging: it traverses orientations to history and organization studies stretching from perspectives that would supplement the latter with the former; integrate them or reorient them through a reformation of the basis for inquiry as is called for by critical management studies. The latter, however, seems particularly susceptible to the ideology critique that was made by Kieser twenty years ago: critique demands a viewpoint and that viewpoint is necessarily transcendental and a priori, wherever it seeks to articulate a specific interest as the basis of its critique. Such questions overlap with the practice of writing history: is the interest in seamless narrative or in its interruption? While the latter has become increasingly popular under the influence of Foucault’s histories of ideas the latter constitute the more mainstream approach. Historians are not the only practitioners to register history; organizations also do so in terms of their branding, promotions, symbolic posturing, and collective representations of memory and a moot point is the politics of their representation, the genealogical principles followed and repressed, the symbols valorized and glossed over. How they do these things is grist for the historical grind, which, in future, the authors hope to see being fashioned by instruments other than those purely of economic history. Ethics, as they note, are clearly one important compass through which to view the past but whether the past can be studied in terms of contemporary insights and dilemmas or whether it should be judged by the standards that were acknowledged to have prevailed then is a moot point. On the basis of the latter, considerable contemporary calumny can be avoided, whether rightfully or not; on the basis of the former many a fortune and the organizations on which such wealth was based would be de-legitimated by association with past misdemeanors and crimes such as slavery, plunder, conquest, and extermination.

Rowlinson also contributes a personal history of the history turn in which the significant and under-appreciated role of Peter Clarke as a crucible for fostering historical thinking is nicely made. Clarke was one of the very few people in 1970s business schools who read history seriously – very much a Braudel man in those days – and a figure whose contributions have been significant.

Debates in management and organizational history

There are pertinent questions to be addressed about the relation between business history and overarching history. For instance, it is difficult to inscribe the history of a firm and industry without making connections to the changing fortunes of national economics, politics, culture, and society, as well as the supranational events and institutions in which these are embedded. Likewise, without an understanding of how approaches such as structural contingency theory grew out of systematic accounts of management ideas at a particular moment in the evolution of modern organizations one could easily mistake their dimensions as eternal rather than historically contingent. Size, for instance, the most important explanatory variable in the Aston School's contingency theory, relies on the tacit assumption that organizations only grow or shrink through extending or diminishing the number of contracts of employment that they have with individual persons. In the early 1960s this was a reasonable assumption to make; in the twenty-first century, after a decade or more of outsourcing, external subcontracting, and complex supply chains, we now know that employment are not necessarily the most important contracts and that their scope does not necessarily delimit capabilities and size. Also, counterfactually, if this history had been read after Foucault, would not Weber's stress on discipline have been accorded a conceptual significance that the Aston scholars failed to see? Perhaps not, for the stress was already evidently there in Weber and was not amplified, conceptually.

Discipline, in its changing modes, did enter into the canon through Braverman's (1974) rounding out of Baran and Sweezy's (1966) theses concerning *Monopoly Capitalism*. Baran and Sweezy are an example of one field in which, as Marens argues, Marxist historiography has been fluent, fluid, and open to historical evidence and that is economic history. While business schools still typically tend to teach economic history in either an institutional or great men mode there has developed a robust and highly respected set of approaches to economic history that have built on core elements of the agenda that Marx sketched in *Capital*. The roll call of honor is formidable: Baran and Sweezy but also Hobsbawm, Arrighi, and Brenner amongst historians but also a considerable number of sociologists who have done fine work, such as Useem, Roy, Lash and Urry, and Fligstein, to name a few. These are remarkably insightful and empirically rich resources for management and organization theorists but are rarely drawn on.

It is not only that they are not drawn on; they are, as Jacques and Durepos argue, unacknowledged in those canonical textbooks that introduce students to the field. Indeed, it is worse than that: there is not only an absence of decent historiography in most textbooks, whether influenced by Marxian analysis or not; there is also an absence of much in the way of historical consciousness at all, which the authors see as a result of modernist emplotment. As a narrative device this systematically downplays discontinuous change in favor of continuous evolution that is linear and progressive. The narrative style assumed is that of value neutrality and disinterest, blind to power relations in all their complexity, paying homage to physics envy that produces a bizarre scientism in which social relations, such as those of gender and other forms of identity, seem absent. The effects are all too evident: a history emptied of perspective, made to read as if it were news from nowhere in particular; a history resolutely unreflexive about its assumptions and practices; historians whose lack of reflexivity is assured.

Terrance Weatherbee nails it: “The manner in which we present management knowledge once history and historical context is written-out of the content means that our textbooks are more likely to serve as an intellectual entry point and values-based framework for managerialism than for management.” Effectively, once the trade school approaches that existed prior to the Carnegie and Ford reforms that were shaping US Business Schools in the 1960s had been suspended in favor of the behavioral sciences, the new forms of knowledge eschewed historical causality in favor of cross-sectional causality between variables modeled on a presumption of experimental science, albeit in open systems impossible to close so that standing conditions could be controlled. Cold War, Big Science, and fear of failure in the wake of Sputnik drove the fetishization of these ‘physics envy’ (Flyvbjerg, 2001) models in US Business Schools.

Historical approaches continued in the margins. As Weatherbee notes there were a small number of researchers that persisted with historical research; some writers of monographs/textbooks such as Clegg and Dunkerley (1980) also conceived of the field in historical terms but these were to prove less popular than approaches that raised Platonic forms to the level of mutually incomprehensible paradigms or metaphors (Burrell and Morgan, 1979; Morgan, 1985). The historical turn of recent times, suggests Weatherbee, can be consigned to one or other camps of either an additive or transformative use of history in relationship with management.

The additive program, calling for more history to be incorporated in management education, is a failing project in terms of the numbers of those enrolled to it, suggests Weatherbee. By contrast, the transformative approach is emergent and its wave may still be cresting or merging with other undercurrents such as ‘critical’ management studies. At this point the text takes an abrupt turn into textbooks as the bearers of ‘normal’ science – normally bereft of history, infused with managerialist values, stuffed with solutions, and blissfully unproblematic as their production is increasingly market focused. What the market doesn’t want are history, ideology critique, and theoretical problematics.

There is an emerging problematization of specific histories in newer contributions, both of major figures and of textbook knowledge; however, this is not Weatherbee’s major concern – that turns out to be a guide to theories of history as forms of cultural production. Given that it has already been established that there is insufficient history in MOS the diverse approaches have little resonance. At this point the chapter concludes, inconclusively.

Methods and madness: doing management and organizational history

Does recourse to the postmodern effect a way out of the modernist impasse? In Munslow’s terms a postmodern account of historical method would see it as a narrative constructed by the historian. Such construction depends on past normative framings of observers and observations in terms of past–history ontological and epistemological underpinnings. Narratives from nowhere are simply impossible; there are no findings without narration. Inasmuch as the objects of analysis are documents created in the past then any subsequent narration is necessarily dependent on the narrations that created that past documentation; not only the subjectivities of the historian–narrator but also the recorder–narrators enter the frame. Adapting Hayden White’s terms ‘historical imagination’ consists of already constituted narrative fragments of evidence of various kinds, arranged in part by design and often more by accident, that are configured by the four major tropes of figurative language: metaphor, metonymy, synecdoche, and irony, each of which is as likely to be chosen through habit as reflexive awareness. Empiricism is blind without conceptualization, argument, ideology, and emplotment; which is to say that there is no such thing as a purely empirical history because past events have no historical meaning until they are narrated as history, albeit that they might have many other meanings in various non–historical

imaginings. History is what it is written to be and it can be written in as many genres as there are available. Historians are Weickian sensemakers (Weick, 1995): they are engaged in the ongoing retrospective development of plausible narratives that rationalize what people were doing at some time documented and otherwise indexed as being in the past. The fundamental nature of history consists of producing likely stories that can be legitimated according to genre requirements.

One major genre of historical understanding remains hermeneutics, which Scott Taylor addresses. Hermeneutics has many precursors but it is the work of Ricouer that is most often chosen as foundational by contemporary scholars such as Phillips and Brown (1993) and Prasad and Mir (2002). The latter analyze both the mode of representation in the texts they address as well as in the objective text their hermeneutic analysis produces. They acknowledge the dialectical epistemological and aesthetic aspects of hermeneutics to create a relation between text and theory, producing both narrative and explanatory intelligibility and plot as well as style.

Key ideas in hermeneutics are the hermeneutic circle, historicity (including the 'hermeneutic horizon'), dialogical understanding, the role of the author-as-interpreter, and critique.

The hermeneutic circle entails that any understanding of textual meaning requires consideration of 'parts' in the 'context' of the 'whole' and a process of interplay between the two. The interpreter engages the text in reflexive inquiry in which the possibility of the text as that text is deconstructed. Part of that possibility might be constituted by authorial intention, although this can only ever be assumed and never known for sure, nor can authorial intention be seen as authorizing whatever interpretation is made: legitimate interpretation does not kowtow to the presumed intentions of the author in authoring some text. As a stricture on interpretative understanding this is particularly important in the field of management and organization history, in which many of the historical traces that are left for the analysts to consider have been carefully chosen, designed, and fabricated by organizational elites with a very definite interpretation of events and other phenomena that they seek to advance. We must never forget that the subjects who produced the texts and contexts that we, as analysts, attend to, are themselves practical hermeneuticians with a range of interests in play in their social construction of the traces to which subsequent generations might attend.

Once the hermeneutic researcher begins to question the presumed or expressed intentional auspices of the texts under analysis then modernist stipulations that entail assumptions about the representation in an historical account of the true nature of events in and phenomena from the past become precarious. Such ontological realism is naïve and usually fulfills an ideological function of legitimating a particular historical gloss. More often than not it will do so through the assumptions of a linear narrative that recovers the facts and arranges them without adornment in a causal sequence. In this kind of history an accurate, complete, and uncontested representation of the past is presumed to be possible, at least in principle: more or new data may revise the picture slightly but the essential contours of the past, once uncovered, remain as they are, undisturbed by the authorial voice of the narrator whose sole concern is with the facts. Postmodern scholarship reverses each modernist assumption as we have seen in Munslow: it licenses histories rather than a singular history. Different historians will ask different questions, with different interests, and focus on different aspects of the possible historical traces that they make evident in text and context.

Durepos argues for neither a modern nor a postmodern history but one that is amodern, derived from the French sociologist, Latour (1993), in his attempt to overcome the binaries of the modern and the postmodern through his argument that *We Have Never Been Modern*. Key to overcoming the representational and relativist dualism cleaving the modern and the postmodern conceptions of what is knowledge is a turn to conceiving of it as a practice. Historians inevitably

interfere with the object of analysis if only because in their doing of history they constitute it on each occasion of its invocation. Different versions of the past are enacted through different histories, each of which inscribes that which it attends to – and differences will be evident in the phenomena attended to – differently, specifically in terms of differing relevancies. What is of significance are the relations between different enactments of the past as history, as well as the relations within which any enactment comes into being. What is significant is not the status of any particular history compared to another but the relations between these histories: how are they constituted, how are they possible?

These views are all well and good but they do presume a rather crowded interpretative landscape; in those areas of the field more sparsely populated than yet another Taylor interpretation or account of what Hawthorne really meant, in say the ranks of business histories, the plurality presumed is most unlikely to exist. Hence, the advice given is of limited value to the researcher who wants to try and get their one-shot at history ‘right.’ If, as Rostis suggests, it is through “an examination of the descent, emergence, and trajectory of events” that genealogically influenced histories attempt “to uncover other explanations that have been hidden in the construction of a logical narrative or explanation of how the past occurred,” then there is a reliance on other accounts as a foil.

Genealogical histories are, of course, influenced by Foucault’s work. What is entailed in this particular form of amodern history, to use Durepos’ phrase, is to first question the possibility of what is in the archive under inspection: why this knowledge, these facts, that interpretation? What role does context play in framing the text in question? What continuities and discontinuities are embedded in the sense being made in that context? How is this sense related to other discourses implicit in and around that context? What are the subjects and objects that are specified in and through these discourses? Using these sorts of questions as a guide Rostis provides a compelling analysis of the emergence of the Red Cross and Médecins sans Frontières as responses to seemingly quite similar but actually dissimilar political contexts. The assumed meaning of humanitarian NGOs is questioned and new insights generated.

In a characteristically Bojean piece Boje together with Saylor brings history down to the philosophically posthumanist salon where entrepreneurial storytelling reigns supreme. The story this time concerns climate change. What follows is a posthumanist manifesto, albeit modeled in part on earlier famous ‘Theses.’ If the point of the famous thesis eleven is to change the world, not philosophize it, then the future, if we are to have one in the long term, from the perspective of the posthumanist manifesto entails seven basic steps to environmental rehistorying. Entrepreneurial storytelling indeed: follow these steps and the “firm can be re-historicized as though a fundamental part of its ethos is sustainability.”

Coraiola, Foster, and Suddaby begin by reiterating Munslow’s (2006a, 2006b) distinctions between three perspectives on the nature of historical reality: these are generated by the extent to which historians consider history to be equivalent to the past, and the extent to which they recognize historical sources as being already a narrative. The three approaches are differing versions of ontology: the reconstructivist, constructivist, and deconstructivist. Reconstructivist historians seek to arrive at a truth about history based on correspondence theory that stresses verification, comparison, and colligation. Historical processes of interpretative understanding, which in the German tradition we know as *Verstehende*, from Weber (1978), are central devices. Faithfulness to sources, commitment to value freedom in their interpretation, and a dedication to getting the facts right, characterize this approach. The reality of the past can be captured in narrative accounts based on inductive processes of impartial selection and critical evaluation of documentary sources. The approach is essentially one of using fragments of data from the past to create a plausible evolutionary history as faithful to sources as possible. The more adventurous reconstructivists would admit that the creation of historical meaning is a product of the

interaction between the historian's knowledge and the content of the sources; the facts do not simply speak for themselves.

Constructivists do not accept that the past can be retrieved in its entirety. They are not witness to history as it happens but as fragments, themselves socially constructed at another time and place, according to interests and prejudices that are not wholly intelligible to the contemporary historian, but which can be imaginatively divined. Knowledge of the past is always bounded by the questions they are able to formulate and the answers available sources can provide. History cannot exist without theory – it is this that frames what is relevant and interesting for the researcher.

Deconstructivism, influenced by French scholars such as Derrida and Foucault, is highly inter-textual. There are texts from the past that the historian consults; there are the texts that the historian produces in the present; and the other texts that other people, including historians, may have produced in the past. History is what one makes of it through the narrative accounts that historians produce. What are central are the forms of representation that constitute these texts, both historical (as having been produced in the past), and as histories produced in the present. In a sense, one can thus only ever write histories of the present if only because the relevancies that one brings to bear on the past, and its archaeology of relevancies, are necessarily and irredeemably those of the present, as the authors demonstrate in their analysis of recent institutional theory influenced by historical methods or focused on history topics.

Decker also discusses institutional theory, joining it to recent Italian microhistory, as a way of warding off the overly deterministic currents of institutional isomorphism, organizational fields, and institutional logics. There are evident similarities in focus as her tabulation makes clear but also some significant differences. Mostly, institutional work approaches are focused on very short timespans compared to the typical periodicity of historical research. The focus is driven by methods that require presentism, such as audio and video recording, or participant observation and interviewing. As Decker points out, most significant transformations of institutional frameworks are likely to occur in the medium-term not a relatively brief timespan. Moreover, some of the structural mechanisms deployed by institutional theories, such as 'institutional logics,' would seem overly structural to most microhistorians.

Magnússon and Szijártó (2013) define microhistory in terms of an approach that is precise and small in scale, using a focal point for investigation while still trying to address what have been formulated as major historical questions in extant scholarship. Doing this enables them to maintain a central role for human agency in their analysis, such as being able to see how seemingly early market transactions were in fact systematically skewed by whether or not they were with kin or non-kin (Levi, 1991). In terms of case selection the microhistorians favor the exemplary anomaly, perhaps best seen in Ginsburg's (1980) classic *The Cheese and the Worms: The Cosmos of a Sixteenth Century Miller*. Indeed, this work is prototypical in demonstrating that social norms guiding human behavior are always multiple, inconsistent, conflicting; not homogeneous and easy to navigate, not handed down by institutional isomorphism or embedded in an institutional logic. Microhistory seeks to integrate and elucidate the particular and the general concurrently through its scaled-down focus on particular events and agencies, emphasizing the overlapping and conflicting nature of social structures, within the interstices of which individuals have freedom to act as other than the ciphers of institutions.

Rewriting management history

Peltonen provides an account of Mayo in his early Australian context – but it is a context that is quite partial inasmuch as the focus is on the history of ideas context for Mayo's early thought.

Much is made of Mayo's fidelity to ideas derived from British Liberal Idealism; however, there is no reference to the context in which he developed these ideas. Brisbane in the immediate post-First World War was a contested city. The Workers' Educational Association in Brisbane, with which Mayo was associated, saw a split after the First World War. The more militant members founded the Workers' School of Social Science in Brisbane. They were characterized by a distinct lack of neutrality on the educational front of the class war (unlike the Workers' Educational Association, from whom they had broken away) by standing for industrial democracy. What they meant, however, as can be seen in their series of lectures on job control, was direct action by revolutionary unions (*Syllabus of the Workers' School of Social Science*, 1919).

For the radicals, industrial democracy meant the same as socialism and the revolutionary method for attaining it. As Irving (no date) says, "[i]t was a usage that responded to the growing democratic mood while at the same time rejecting the initiatives by governments and employers to open up new avenues for dialogue with workers." In Queensland, unions in the railways and meatworks were militant and increasingly 'industrial,' as can be seen in a pamphlet called *Who Shall Control Industry?* The pamphlet was written by T. C. Witherby, who was Director of Tutorial Classes at the University of Queensland, where Mayo held the first chair of philosophy between 1919 and 1923:

Here in Queensland, because we are in Australia, and because we are under a Labor government, the wage-earners' position has elements of strength not known in Europe. And this for three reasons. These reasons I owe to a pamphlet by Mr Childe. They are that the politician is more under the control of the industrialist [i.e. the radical unionist] than in other countries; the permanent official is not so far apart from the ordinary man as in the bureaucracy of Europe; and more consumers in this country are producers. However autocratic therefore State enterprises may appear to the worker in Queensland, there is here the possibility scarcely existing in Europe of a large share of workers' control. It is only because of the indifference of the wage-earners on this matter that no advances have yet been made
(*Witherby 1919*)

These were anathema to Mayo's ideas.

Observing the disturbing level of industrial strife and political conflict in Australia, Mayo formulated an analogy between war neurosis and the psychological causes of industrial unrest. Drawing on social anthropology, he argued that the worker's morale, or mental health, depended on his perception of the social function of his work. He saw the solution to industrial unrest in sociological research and industrial management rather than in radical politics.
(*Bourke 1986*)

It should be evident that we cannot recover the context in which ideas developed simply by looking at the history of ideas. We need also to see, as an important part of the context, what gave these ideas life, vitality, which is to say how and in what ways were these ideas contesting other ideas? When we do this we can recover a more enveloping set of dynamics. The war legacy is crucial as it was the returning soldiers and sailors, especially, who had been most radicalized by the appalling experiences and who were well aware of what was happening in Russia. As Clegg, Courpasson, and Phillips (2006: 79) stated:

The interpretations that Mayo (1933) made of the Hawthorne Studies were in large part already well rehearsed and accorded with views that he had formed in Brisbane in the

aftermath of the First World War, in his work with ‘shell-shocked’ soldiers returning to sunny Queensland from the dark horrors of trench warfare in the European theater. It was Mayo’s early and incomplete training as a doctor, and his collaboration with a Brisbane specialist, which provided the occasion for the formation of his ideas about the importance of psychological subjectivity. From the treatment of maladjustment on the part of veterans, it was a small step to the treatment of industrial malaises: ‘Industrial unrest is not caused by mere dissatisfaction with wages and working conditions but by the fact that a conscious dissatisfaction serves to “light up” as it were the hidden fires of mental uncontrol’ (Mayo 1922: 64; cited in Bourke 1982: 226). Elton Mayo prescribed several short rest breaks per shift, to relieve physiological strain and to disrupt the negative feelings that led to industrial unrest. These ideas were subsequently to develop much further, as a result of his research involvements during the Second World War.

Idealist liberalism did not exist in a void: it was ^{apa} class struggle and Mayo was a keen protagonist in these struggles, waged against Guild Socialism:

Guild socialists were centrally concerned with democracy, differentiating themselves from the Fabians who failed to appreciate the aspirations of ordinary people for empowerment, and from the syndicalists who ignored the potential for selfishness and conflict in their schemes for industries to be owned by the workers in them. Guild socialists wanted to rise above the conflict in the movement between Fabian ‘political action’ and syndicalist ‘industrial action’ by drawing on pluralist and distributivist philosophies that recognised differences (local and occupational, for example) but assumed a common interest of all citizens as consumers. They were not Marxists, for although they recognised the class struggle, they did not, on theoretical as well as empirical grounds, see it as resolving itself in favour of the working-class. Although they disagreed on details, guild socialists in general accepted that the state would continue it was needed to act as trustee for the community and provide representation in parliament for citizens as consumers – but insisted that effective control would be vested in the industrial guilds, set up by the state and consisting of representatives of the relevant industrial union, the experts, and the state managers. It was therefore a scheme to unite citizen democracy with worker democracy.

(Irving, no date)

It is against this backdrop that the formation of Mayo’s early ideas and their consistency throughout his American career should be seen. A turn to history should enable attention to wider discursive contexts than just the history of ideas a key figure draws on; it should also explore the context in which they were contested.

Mir and Mir chart a narrative familiar to all scholars au fait with Marxist accounts of recent economic history in detailing the rise and contradictions of Fordism. Seeing the crisis of Fordism from the late 1960s onwards as one of a crisis of rigidity, the institutional and managerial solution was to call for enhanced flexibility: in manufacturing systems; work practices; consumption patterns; and new patterns of flexible accumulation based on increasing financialization, diminished unionization, increased globalization of manufacturing, and increasing digitalization enhancing space–time compression. Mir and Mir elaborate these themes at length. The historical component of their essay is to point out that flexibility did not spring on the world, fully formed, as a relatively recent phenomena but that, from a world systems perspective, there are far deeper roots to the present tendencies. Financialization is not a new phenomenon but a pattern that can be

seen in the cyclical and evolutionary phases of historical capitalism from the thirteenth century onwards. Clearly, in terms of historiographical method, Mir and Mir are interpreting the historical record through an essentially deterministic pattern, cast by the marriage of Braudel and Marx that produced the progeny of world systems theory (Wallerstein, 1974).

The focus of the chapter by Mills, Weatherbee, Foster, and Helms Mills is the New Deal era in US political economy. In this period some similar political currents that flowed through Mayo's Brisbane politics also animated the US scene. The New Deal was a large-scale national attempt to reorder relations of production and consumption on lines that were influenced by debates concerning democracy that were rooted in socialism of various kinds. Key figures are singled out for discussion. These figures, and the New Deal philosophy they embraced and exemplified, have been written out of Management and Organization Studies history: the authors seek to write them in.

The New Dealers embraced various forms of ideology in putting their programs together, not always coherently or consistently. By contrast, Spector argues, in the immediate post-Second War era, the capitalist opponents of the New Deal were much more coherent ideologically, despite professing an antipathy to the very idea of ideology. The emergence of ideological coherence was nurtured by the Cold War, corporate opposition to the New Deal, and an uneasy and equivocal relation on the part of Americans with the new conglomerates that were shaping the business scene.

It would be surprising if there were a coherent ideology of business if it did not find systematic expression in one of the key organs of business. Hence, the frame for analysis is the *Harvard Business Review*, in which four dominant and intertwined themes are identified. First, there was an expectation of minimum government interference combined with optimum government promotion, an ideological thrust directly aimed against the New Deal. Second, there was an explicit equation of Christianity and capitalism in terms of values ranged against ungodly communism. Third, individualism was constitutive of the American psyche and, as such, an expression of a natural state that the collectivist ideals of communism abroad and unionism at home ground down. Hence, fourth, at home business should do all it could to educate the broader public that it was not unions that could improve the prosperity of workers but unfettered free enterprise in which the interest of owners of capital and those who were merely employed in it were fused on an ever-improving trajectory that rendered class warfare redundant. The best cure for global poverty was the export of this free enterprise system to the more benighted parts of the globe that did not enjoy America's benefits; as national liberation movements adopted socialist colors in countries such as Korea and Vietnam the force of words subsequently had to be joined by the force of arms.

In the 1950s and 1960s, Spector concludes, the foundations of the "end of history" argument articulated by Fukuyama (1992), which suggested that liberal market capitalism had achieved vindication and victory with the death of Soviet Communism, were laid. Yet, history has a way of not ending and ideology a way of refolding: the global financial crisis and the escalating crisis of climate change being the folds in question.

The capitalist ideologies of the 1950s engaged in simplification (liberal market capitalism vs. communism), contradiction (against government *interference* but for government *protection*), misdirection (*People's Capitalism*) and denial (poverty). When ideologues present the subjective as objective, advocacy as observation, and self-interest as mutuality, the result is denial, distortion, false consciousness, and the concealment of real interests.

(Spector, *this volume*)

Well, I would not disagree with the verdict although I would question the terms in which it is expressed. The identification of distortion, false consciousness, and real interests presumes a position of theoretical sovereignty as an a priori that is every bit as ideological as the positions opposed (Clegg, 1989).

Scale is reduced greatly in the other essay in this section: the focus is on public relations (PR) in Canada, a field characterized by a broadly North American (read US) narrative of the progressive evolution of more ethical practice, represented in a move away from propaganda towards strategic, socially responsible communication. The development of textbooks as paradigms of ideological normalcy was central to the process.

Canadian experience belies ideological normalcy. In Canada it was the nineteenth-century desire to populate the vast tracts of open land with settlers from Europe that occasioned the development of a particular form of propaganda that might subsequently be recognized as PR. “Throughout the early 1900s and up until the mid-1920s in Canada, government communication and publicity campaigns were by far the most significant and most common of what would become identified as public relations practice,” Thurlow argues. As the railways opened up the frontier they developed rhetoric for communication that drew on the same skills. By the early 1900s a few private enterprises were developing a PR profile.

The explicit recognition of PR dates back to the foundation of a course at McGill University in 1948, from which incipient professionalization other courses and professional bodies developed. These are represented, discursively, in an archive that the author subjects to Critical Discourse Analysis (CDA), demonstrating the centrality of the theory of excellence, the model of symmetry for professional practice via mutually beneficial relationships between organizations and publics, an increasing representational concern with ethics, as well as an increasing Americanization of the field and the necessity of securing a privileged space for the nascent profession. In terms of historiography, the use of CDA to explore a specific focal object through analysis of an archive is the major contribution.

Management history at the margins

Management theory was born in the USA yet it has colonized the world. Why and how? McLaren and Mills seek to answer this question through researching a determinate absence – non-existent Canadian management theory. Their argument seeks to construct a history of Canadian difference that conventional accounts gloss over. These conventional arguments are usually first encountered by those entering into a career in management in Canadianized US textbooks – often where the place names and little else is changed to Canadian – a practice that Australian (and, one suspects, many other nations) management inductees would also be familiar with. Canadian scholars, they argue, further this Americanization by seeking publication in US rather than Canadian journals because of their greater ‘legitimacy’ globally, especially as Canadian Business Schools become more aligned with their US models through factors such as AACSB accreditation. These business schools were largely post Second World War creations forged on a US models, often hiring those Americans who did not find or want US employment or recruiting from cognate social science disciplines. Professionally, the demand grew for a universal management theory for all countries based on positivist assumptions, empiricist methods, and functionalist theories – all strongly enconced as US orthodoxy. An unfortunate paradox occurred: if management science was universal there was no need for specifically Canadian contexts; moreover, specifically Canadian contexts were of less universal value in a domain that defined the universe as principally that of the United States.

Canada is different, note the authors. It is not the US nor is it similar to the US in important ways:

the country developed as a resource-based economy with significant amounts of foreign investment; while large in size it has a small population spread across its southern border, divided regionally by cultural, linguistic, political, and economic differences; and it is a welfare state with a history of strong activist governments and an electorally-viable social democratic political party.

(McLaren and Mills, this volume)

As a settler dominion Canada has far more in common with societies such as Australia and New Zealand, than it does with the United States. The history of economic protectionism and the development of social welfare have far more parallels with these dominions than the United States. Indeed, although the essay does not mention it, a distinctive Canadian political economy centered on the staples thesis and the work of Harold Innis as an explanation for how the pattern of settlement and economic development of Canada was influenced by the exploitation and export of natural resources. Unfortunately, Innis' (1930; 1940) theory seems to have had negligible influence in Canadian business schools; had it done so there would be a far better basis for understanding the specificity of Canadian experience in its foundations than is offered by the Americanized texts, something that the authors recognize as only just beginning.

The legacy of Innis has been built on by researchers such as Wallace Clement (1997; Clement and Vosko, 2002) but seems not to be recognized in the business schools. Another missing aspect ~~missing~~ from consideration, must also be ~~consideration~~ of the distinct bicultural context of Canada's predominant multiculturalism split between Quebec and other French-speaking provinces and the dominant Anglo sphere. There is a distinctive and different account, albeit still subaltern, of management in the Francophone world that does not have such a strong US character.

There are many Americas. Canada to the north is a different reality to the United States to the south. Further south still, there are the more complex realities of Latin America. Across the US border in Mexico, one of the most significant voices in Latin and world management and organization studies, Eduardo Ibarra-Colado, was recently and sadly stilled. Before his death he left a significant legacy, informed by an acute understanding of the history of Mexico, an example of which is collected in the volume.

Central to Ibarra-Colado's concerns was the condition of coloniality:

Coloniality must be understood in its institutional dimensions, as a deliberate action of power to dominate and subjugate the other. It is the hidden face of Modernity, always considered the stage made possible by the development of reason, instead of the process of invasion/destruction/invention based on the power of weapons, ideas and symbols to impose a unique civilizing process. From this point of view, power instead of reason explains the splendour of Europe and America and the prevalence of some kind of knowledge over others.

At least three institutions have been essential in the operation of colonization: the Church and evangelism to produce silence and colonize the soul; the Hacienda and mills to produce discipline and obedience at work and to colonize the body; and the University and science to produce truth and colonize the understanding of the world. The organization of knowledge and their associated practices in the modern *Universitas* has guaranteed the reproduction of disciplines as we know them and, at the same time, has obstructed different ways to live and imagine academic work, discouraging or forbidding alternative practices.

The door to a whole world of potential differences is opened that, instead of being buried by putative universalism, might be explored for innovation and productive tensions. As noted, the vast majority of Latin American intellectual production in the field of management and organization studies is mimicry: it copies Anglo-Saxon norms in all areas of intellectual reproduction. Government funding of students to do doctoral work in these centers of excellence elsewhere inducts and indoctrinates the emerging generation of scholars to learn to think, speak, and write with inauthenticity. Elsewhere, in the broader fields of history and social sciences there are more authentic voices to be found but these are both neglected by indigenous management and organization scholars and are unknown to those from the hegemonic discourse. Fusion of these authentic and indigenous sources in a ‘trans-discipline’ corpus of knowledge ~~that~~ builds knowledge ‘transversally’ in a set of multiple dialogues and conversations. These must not be dominated by the hegemonies of language and orthodoxy and they must include both scholars and the people of different regions and cultures. In this way they might learn from each other and reconstruct management and organizational knowledge that recognizes, improves, and protects local styles of life through respecting the differences that are embedded therein.

Following the contribution from Eduardo there is a remembrance and homage from three colleagues, Srinivas, Guedes, and Faria. It is moving and fitting. Eduardo was one of my best friends. I came to know him during the period when I was editor of *Organization Studies*. He sent a paper that was not, for a variety of reasons, publishable as it was. I wrote back to him as editor explaining how and why this was the case and suggested some ways that it might be revised. The rest is history, as we say. Eduardo invited me to Mexico, there followed many collaborations around APROS, EGOS, and LAEMOS, and much correspondence between meetings. He was the best organization theorist; my first point of reference when writing my Foucauldian inspired history contributions to *Power and Organizations* with my colleagues David Courpasson and Nelson Phillips. As I wrote in the acknowledgments to that book he “made incisive, extended scholarly comments on a great deal of the manuscript.” In the words of Brecht that open the colleagues’ homage “there are those who struggle all their lives; those are the indispensable ones.” Eduardo’s legacy is indispensable.

Coller, McNally, and Mills explore some of the mechanism by which hegemonic forms of privileged discourse are constituted theoretically. Using the fusion of Actor Network Theory (ANT) and reconstructive history that has featured in several other chapters in the book that these authors have contributed to, the authors provide an ANTi-history of the Canadian networks of new institutional theory (NIT). A number of ~~key~~ scholars ~~to~~ these networks are stipulated using citation statistics as the basis. An inner circle, comprised, not surprisingly to scholars familiar with the field, of Hinings, Greenwood, Suddaby, and Oliver is identified, as well as an outer circle of still well-known but lesser figures. The chapter analyzes the mechanism of reproduction of these centers of excellence, stressing in particular recruitment and training of PhDs and institutional promotion. Historiographically, the authors use quite conventional network analysis and ranking methods to identify their circles, demonstrating, perhaps, that historical work need not be equipped with any specific method but can borrow freely from the broader social sciences.

Conclusion

The history turn is well represented in the volume and an eclectic range of approaches to the doing of a historical management and organization studies is well prepared. The time is certainly ripe for more such work as the recent call for papers that focus on the history turn in major journals, such as the *Academy of Management Review* and *Management and Organizational History*

suggests. Doubtless, some of the perspectives highlighted herein will find their way into these special issues. Should they do so what one might wish for would be both a more grounded and a more rounded management and organization theory, one able to appreciate the specific contours of the contexts in which inquiry is conducted, both in terms of the intellectual context of those conducting it and the contexts that are attended to empirically. It would make a change from much standard fare.

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