



Conflicting Visions: A Recap About the Debates Within Management History

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Abstract

The purpose of this chapter is to recap the various debates about management history by considering the roles of traditional history, postmodernism, radical history, and ANTi-history. I also track the development of various myths and discuss the competing strengths and weaknesses of the various models. My purpose is to provide a discussion of the models – not to offer judgment but a reasoned appraisal for the purpose of edification and debate.

Keywords

Traditional history · Postmodernism · Management history · Radical history

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Introduction

I begin, *mea maxima culpa*, by stating that I did not write this chapter as a detached scholar but rather as an active participant and curious observer on some of the ongoing methodological and epistemological issues in the field. As such, I cannot claim to be comprehensive nor unbiased. What I can do is to provide the observations of a young but published scholar, one who has been involved in several debates about the field (Muldoon 2019). My perception is that the field is very different from the one I entered into about 10 years ago. In some ways, the field is worse. Although the *Journal of Management History* remains a well-regarded journal, we no longer see history articles published in higher-level journals such as the *Journal of Management*. Although we have new perceptions, the clarity and precision that I believe characterized the field during the period of ascendancy and domination by the University of Oklahoma (Dan Wren) and the Louisiana State University ascendancy is increasingly gone. Yet, at the same time, we also have witnessed increased debates that are furthering the field.

No matter one's perspective, these current debates about traditional history versus the new postmodernist history can only benefit the field, as it moves us beyond single studies with little connection to each other to rigorous debates that may advance the discipline. Although I am a critic of much of the new history, I am respectful of the talents of the postmodernist side, their intellectual contributions, and mostly, because it is inspiring debate. As a younger scholar, there is much to learn about their steadfastness in questioning ongoing nostrums. That Rowlinson's (2015) initial paper on the Historic Turn, which was rejected by the Academy of Management, provides hope to any scholar who has had a good paper rejected. Like it or not, Rowlinson's work is something that a management historian has to come to grips with. The hope I have is that we can provide sufficient attention to the field of management history, increasing the prestige of what is currently one of the smallest divisions in the academy of management.

Management and organizational research and historical analysis have a curious history. Joseph Litterer, whose work on the standardization of business was one of the first works of management history published, abandoned his research to become a traditional organizational behaviorist. Twenty or so years later, Arthur Bedeian did not quite abandon history, but he moved his research line from a purely historical approach to one that embraced traditional empirical research and theory building. In fact, during one seminar at Louisiana State, Bedeian stated that scholars seeking a future in management history had gloomy prospects. Of his contemporaries, he noted only Charles Wrege and Daniel Wren were able to make a career as a management historian. Yet, it was also true that many scholars, such as Alfred Chandler, George Homans, and Peter Drucker produced, at one point or another, historical research. Unfortunately, the "management" and "organizational studies" fields have moved into a more purely "scientific approach," one that often ignores history. A number of prodigious management historians, such as Milorad Novicevic and John Humphreys, have a successful publication record in other areas of

management. In fact, some of their management history has contributed to theory building elsewhere (e.g., Novicevic et al. 2017).

Management history as a disciplinary field arguably started in 1972 when Daniel Wren (1972) published the first substantial work of management history. Previously, Claude George's (1968) work focused on merely recounting dates, works, and figures. Wren's attempted to provide a framework on the evolution of management thought – one that explained how figures from ancient history up to the past have influenced the field. Subsequently, Wren occupied the commanding heights of the field. Management scholars often felt the need to either contradict this analysis or, alternatively, use his framework as a basis of their own research. In fact in my first two published papers, I took exception to several parts of his work and wrote papers that were a respectful rebuttal (Muldoon 2012) but nevertheless a rebuttal. Yet, there has been an influential and growing movement that has challenged his work – often noting that he placed management history in an iron box, ignoring some figures at the expense of others and ignoring topics (such as slavery) that could cause embarrassment to management (Novicevic et al. 2015; Cummings et al. 2017).

I regard this critical movement as a mixed bag. Firstly, for the field of management history to grow, we need different voices. The voice of one person, no matter how sober and well-informed, cannot and should not speak for the voice of entire history of the field. I appreciate some of the different voices and approaches. Revision and argument are not antithetical to history. In fact both are necessary for the field to grow and expand. Therefore, revisionism is not a specific brand of history but how history is developed. Yet, and this part I decry, we often unfairly and unnecessarily attack the works of great previous historians. I write this chapter to provide an overview of the debate between traditional histories versus the new history. I talk about how this debate informs the following topics: an overview of traditional history, the challengers, and the role of myth.

Traditional History

Management history started largely with the work of Daniel Wren (1972) and his friends and colleagues, Ronald Greenwood, Charles Wrege, and Arthur Bedeian. Wren and his colleagues' work was grounded in empiricism and provided the traditional narrative of management history – with a progression of scholars starting in ancient times to the era of Frederick Winslow Taylor that created management thought. Wren was especially important in developing this approach, as his work on management thought has enhanced his reputation to the point where he is considered the “Alfred Chandler” of management history (Novicevic et al. 2015). Wren's work was the first to take management's history seriously. Unlike Claude George (1968), Wren provided not just a description but also a background and context for his work. The purpose was to provide the reader with the “gift of professional maturity” so that scholars can understand where we came from, where we are going, and the perils and promise of the field (Bedeian 2004). Wren and Bedeian also viewed the work of the

past as the foundation on which current management thought is built, thereby providing assurances for those in the present that “progress in scholarship is a multigenerational endeavor” (Wren and Bedeian 2018, pg. xix).

Contrary to the criticisms sometimes made against him, Wren’s history as embodied in *The Evolution of Management Thought* (now in its 7th edition) is consistently changing from one edition to the next. The method that Wren and (later Bedeian) provided was one based around examining scholarly works to produce a narrative. Although unstated, the primary method employed by Wren and Bedeian is probability based on the preponderance of evidence (Bogue 1983). Necessarily, both methods require consistent revision and review of the historical record in the light of new scholarship. As a craft, historians carefully weigh the evidence based on the historical record. Interpretations change as evidence changes – much as it would in Bayesian statistics. The individual historian may be prone to error – but the hope is that the review process and the community of scholars can correct errors and promote more accurate histories. The net process of management history over the last 50 years is an impressive body of evidence.

In essence, Wren and Bedeian sought to understand how various theories in management have emerged by capturing their chronological relationship in the belief that this approach best captures the “zeitgeist” that “compose the people and time” who form management thought. Wren and Bedeian therefore use time as a boundary for writing a compelling history that demonstrates the development of management thought. This is revealed by their selection of the term evolution – suggesting not Darwin, but a development of the field. The term evolution implies two distinct things. Firstly, evolution implies a development over time. It is that way they track development from one time to the next. Second, it implies things get better. These assumptions and methods have been challenged by those associated with the so-called Historic Turn in management thought.

One of the salient elements of Wren and Bedeian’s work is the concept that they divide management history into four chronological periods – which are the (1) the early period, (2) the scientific management era, (3) the social person era, and (4) the modern era. They use these four simple but elegant domains to track the transformation of management, demonstrating how management has unfolded since the ancient Egyptians and Sumerians. One of the implications of their research is that management moves through fads and that one generation leaves a general framework through which other generations emerge. The basis of this type of research, as noted by Novecivic, Logan, and Carraher, is that:

1. Chronological studies proposing periodization in the evolution of management thought
2. Studies aimed at ensuring that management’s knowledge base does not contain any myths or misinterpretation from the past
3. Studies uncovering examples from the past to serve as benchmarks for the future
4. Studies aimed at reevaluating the past against our current knowledge base to identify management fads and fashions

Up until recently, scholars adopted this type of framework, relatively uncritically. In fact, in one of the reviews of my Hawthorne paper (2012), a reviewer complained that I was being too critical of Wren's perspective. My own perspective was that Wren's work was so good that it should serve as an inspiration for criticism, either because there was a gap in his study or because there was a quibble with his interpretation. I felt that Wren's work was the Bible of management thought in the same way Viteles work has been for motivation and industrial/organizational psychology (Mills 2012). I have recommended this book to colleagues, doctoral students, and even practitioners who wish to understand management. There is no topic of management thought that I do not want to read Wren's opinion, as I have found it sober and well-informed. I am also in complete agreement with him that history runs through periods as it usually is the experience of generations that produced and interpret historical events.

A New History

Despite the commanding heights that Wren's work has long occupied, there has been a group of scholars who have criticized not just the findings of Wren and Bedeian but also the philosophical underpinnings of it – namely, the use of time periods. Critics of this type of format argue that they are artificial constructs imposed upon history by historians, with little regard or understanding of the nuances of the past. For example, one of the contemporary criticisms is that there were recognized systems for systematic or if not scientific management long before Taylor. Starting with Bill Cooke, scholars have demonstrated that slavery, especially in the United States, could be viewed as a massive management enterprise – one that combined elements of human capital, profit, and strategy – well before Taylor. Therefore, what Wren and Bedeian did was to impose a heuristic and, indeed, random structure on the past that did not adequately reflect it. In fact, what they did was to ignore negative events that would damage management's legitimacy.

One of the major criticisms of Wren's approach has come from Novecivic et al. (2015). The argument provided is that Wren and traditional management scholars “developed their web of shared beliefs about the history of management thought not only objectively but also relationally through their socialization in their academic communities.” As a correction, Novecivic and his coauthors have argued for a more eclectic approach to conceptualizing the history of management thought. This approach has been echoed by other scholars who have sought to include those have previously been excluded from the construction of the dominant historical narratives. Notable among these are those associated with the growing body of postmodernist thought.

Novecivic and his coauthors have extended their criticisms by arguing that approaches such as those pursued by Wren and Bedeian ignore the polarized views that often develop in history. For example, they note that different scholars have varying viewpoints of Taylorism – observing how “Taylorism has been both

recognized as the legitimate object of theorizing (Wagner-Tsukamoto 2008) and denied as a myth (Wrege and Greenwood 1991).” Yet, Wren ignores these viewpoints, positing instead the view that Taylorism was a valid undertaking to promote both social harmony and management’s own legitimacy. Novecivic et al. also draw upon Bevir’s (2003) framework, which holds that we are constrained in developing meaningful interpretations of the past. Moreover, it is suggested those interpretations that do emerge and become part of the accepted field are often the result of subjective experience. Bevir also argues that we often use theory to provide a grossly simplified account of the past that ignores the inherent complexity of history. Seen from this perspective, Wren’s assumption that his work is based upon positivist factual claims is nothing than subjective thought.

According to the critique of Novecivic and his coauthors the framework that Wren produced is, therefore, nothing more than an organizing structure that is “fictive” and useful only as a narrative device. Rather than being an objective outcome of history, it is in fact subjective. As Novecivic et al. write:

Wren has had the capacity to change the webs of shared beliefs about management history that he inherited. In particular, we claim that Wren changed the way in which the history of management thought is now commonly viewed and taught by introducing the analytical framework of social, political, and economic factors that influenced historical change in management practices. We argue that in his book Wren depicts this historical change through the meanings that we uncover from his framework-based conceptualization and narration of the history of management thought.

That Wren’s framework became dominant, Novecivic and his coauthors suggest, primarily reflected not its intellectual strengths but rather its spread and adoption through socialization – a salient point in that Novecivic and Carragher were students of Wren. The implication that what Wren produced was not real. Instead, it was but one socially constructed reality, created through Wren’s synthesis. Similarly, attempts to develop a “normal history” (to use a Kuhnian phrase) – such as that created by Wren – require a foundation. The past only becomes historical if it consists of events that could have been made only at the time. Wren uses an interdependent viewpoint that consists of both historical and his own judgment.

My take is very different. Although history has been studied for thousands of years, it did not assume its modern form until the time of the mid-nineteenth-century German historian and philosopher, Leopold Von Ranke. Ranke, influenced by the Germanic idealistic tradition, sought to understand the past “as it essentially was.” The basis of this research was empirical approach that used critical treatment of unused, original resources. These new historians eliminated the use of the will of God to explain historical outcomes (unlike the mid-nineteenth-century American historian Frances Parkman, who believed that God enabled Wolfe to beat de Montcalm). The ideal historian was someone that would cross an ocean to verify a comma. Therefore, the path that Ranke set historians on was evidence-based analysis that sought to reconstruct the past based on judgment calls. Factual analysis was the key. Despite arguments from postmodernists, there was, even here, an understanding that the past was separate from history. History is based on judgment

calls based on the concept of probability. It is possible that Abraham Lincoln was not elected President in 1860 – but it is not probable. There are such things as facts, and not all interpretations are equal – a point even postmodernists recognize.

Let's take the facts of Taylorism. Was Taylorism a myth or legitimate? Those are two different viewpoints by two sets of historians. How could we reconcile them? Well, it does not mean that those two viewpoints are equal validity. For example, as I will note later, Wrege was an excellent empirical historian, who often lacked judgment and failed to see the big picture. To be sure, Wrege did advance the field, but his principle motivation was to debunk Taylor and other historical figures. He did not continue and ask why Taylorism was so esteemed, even if it was built on flawed foundations. Yet, Wrege and Greenwood (especially Greenwood) did believe that Taylorism had benefits even if the man was a jerk. To this day, we continue to use Taylorite concepts of testing, measuring, and theorizing about work. Taylor imagined a new world of plenty produced by management, a new world that has come about. But at the same time, Taylor was also elitist, a liar, a braggart, and a narcissist. Wrege was right, but he did not go far enough. Likewise, Wagner-Tsukamoto (2007, 2008) recognized the limitations of Taylorism while conceding its underlying excellence. The separation into two camps ignores nuance.

The historian must make judgment calls – those judgment calls must be based on evidence; they must consider alternate evidence, and those must be based on the preponderance of the evidence. Furthermore, there is no such thing as “normal history.” The field itself is fraught with contentious arguments about interpretations. History is argument without end – there are no all-powerful covering laws in history. Instead, economic, social, and psychological forces combine to move human beings in differing ways at differing times. In fact, the way to get published in history is very different than science. Whereas scientists are bound by normal science (only theory shifts can rescue them), historians are geared toward finding new evidence, to alter the given historical record. Applied to Wren, he consistently updates his work to consider new evidence.

That being said, the one criticism of Wren is that he has kept his superstructure. However, I could not find of any reason why he would have to abandon it. For example, during the scientific management era, management thinkers talked about issues related to scientific management. Likewise, during the human relations era, management thinkers sought to research and understand social relationships at work. That is not to say that scholars did not research social relationships during the scientific management or scientific management during the human relations era. Rather one field was dominant during the time. The use of time is exceptionally important to historical research and narrative. Without such, history would not make much sense or provide little value. Should we, for example, stop referring to America in the period from 1848 to 1877 as the Civil War era? Yes, it is true that many other things happened in these years besides the prospect or reality of civil war. But it was the threat of Civil War and, subsequently, the profound effects of actual armed conflict that did more than anything else to shape the lived experience during these years. Eras take form by major events, social, political, economic, or military.

A New History of Management

The fundamental divide that has opened between traditional forms of management history and advocated by the supporters of the so-called Historic Turn has become even more pronounced with the publication of *NHM*, a work co-authored in 2017 by Cummings, Bridgman, Hassard, and Rowlinson. Claiming that their work provides the basis “for a new, deeper history of management,” Cummings et al. (2017, p. 33, xii) declare that the work of “pioneers,” such as Wren, is outmoded and unfit for use in “our times.” Explicitly framing their study within “a Foucauldian” perspective, Cummings et al. (2017, p. 41) declare that their aim is to write a “counter-history” that no longer accepts “conventional truths.” In doing so, they argue several radical positions: that Adam Smith was not a believer in free markets; that Frederick Taylor was no fan of the “efficiency” movement that adopted his name; that the Hawthorne studies were poor scholarship and a detriment to the field; and that Weber was not concerned with bureaucracy.

The postmodernist move into management history is a comparatively recent development. While there were antecedents to this movement, it really started in 2004, with the publication of Clark and Rowlinson’s paper calling for a Historic Turn in management research. According to Clark and Rowlinson (2004, pg. 331), the Historic Turn “is an increasing call for an historical perspective in organisation studies . . . the ‘historic turn’ that has transformed the way other branches of the social sciences and humanities ‘go about their business.’” This statement was not controversial that scholars over the years, including Wren and Bedeian, have called upon historical awareness and method in the profession. What was controversial was Clark and Rowlinson’s call for this Historic Turn to be based upon “such as the ‘discursive turn’, deconstruction and post-modernism. Within history itself this transformation is associated with hermeneutics, the ‘linguistic turn’, and the revival of narrative.... (pg. 331).” Their declared social purpose was also different from that of people such as Wren and Bedeian in that it sought to find new foundations of management history, seeking to make the profession less supportive of management and more supportive of radical ideas.

This broadly postmodernist approach to history has challenged the dominant empiricist model as expressed by Wren and Bedeian. As Novicevic et al. (Novicevic et al. 2015, p. 13) accurately observed in a chapter published within *The Routledge Companion to Management and Organizational History*, “a significant number of management historians” now reject the idea that management history can be written on the basis of “positivist factual truth-claims.” Elsewhere in the same volume, Munslow (2015, p. 136) declared that “meanings are fictively construed. There are no lessons in the past.” Although such “critical management” perspectives draw on multiple sources – most particularly Michel Foucault, Hayden White, the “amodernist” French theorist, and Bruno Latour – their influence within management history has grown exponentially since 2004 when Clark and Rowlinson published, in *Business History*, their call for a “Historic Turn” in organizational studies. Admittedly, as Bowden (2018, p. 204, 212) notes, there is a “disingenuous” character to much of the literature that the call for an “Historic Turn” has spawned

in that it frequently uses Foucauldian and postmodernist frameworks without acknowledging their source. The original Clark and Rowlinson (2004) article, for example, only mentions Foucault on two occasions and postmodernism on three – despite being couched in what we are clearly Foucauldian ideas. While the movement is inchoate (and contradictory – see Bowden 2018), there is, nevertheless, a deep and resounding skepticism of the empirical history that Wren and Bedeian have published.

The postmodernist approach has its focus on power and, as Richard Evans (1997) expressed it, the ability of those in power to create sustaining narratives that maintain the current power relationships. As Evans has noted, this approach has some of its roots in the New Labor history, where scholars have attempted to provide a voice for the voiceless and attempt to reconstruct a past that had been lost. Yet, E.P. Thompson and Herbert Gutman were empiricists, who used many different techniques to cobble together a forgotten past. However, postmodernists – even those who are not completely devoted to it – are skeptical of historical fact. Advocating what they refer to as ANTi-history, amodernists Durepos and Mills (2012, pg. 5) argue that all knowledge is inherently subjective and that research should be primarily informed not by empirical discoveries of past realities but by considerations “as to who benefits from a specific interpretation of the past.” A similar opinion is expressed by Hassard, Rowlinson, and Decker (Rowlinson et al. 2014, p. 257) in an article entitled “Research Strategies for Organizational History,” where it is declared that there is a “fictive” component in all historical writing. Seen from this perspective, it is power – and the uses and abuses of power – that is decisive in shaping and distorting the nature of evidence, be it in the cause of the oppressed or the oppressors. Postmodernism is skeptical and indeed, sometimes outrightly hostile to most forms of historical evidence (Appleby et al. 2004). Its proponents note that official documents are flawed sources of evidence because they either do not consider all relevant factors or because those individuals in power seek to control the record. Similarly, oral history and memoir are flawed due to the fact that memories are social creations. Within organizations, therefore, it is only the constructed memories of the powerful that endure. Other types of evidence, such as demographics, etc., are deliberately ignored in that reference to such sources mitigates historical explanations built around power and oppression, which postmodernists regard as the real drivers of history. Time periods are also considered artificial constructs that detract attention from the immediacy of power and oppression. In short, there is no evidence, no way of getting to the past, which does not reflect the manipulated social creations of one or more power interests. Accordingly, the narratives that historians create as an intellectual framework are mere fictions that typically support the dominant framework and are examples of power.

These postmodernist ideas *do* have a deep benefit to the profession in that they force us to consider both our methodological roots in a deeper way and the foundations of the field. For example, in Cummings et al.’s (2017) book on management history, it is argued that management historians willfully ignore the contributions of figures such as Louis Brandeis and Theodore Roosevelt and their commitment to liberalism, focusing instead on the production efficiency of

Frederick Winslow Taylor. Elsewhere, Cummings et al. argue that Adam Smith was not a neoliberal icon but rather a believer in the welfare state; that the use of the term *laissez-faire* in association with Smith is a misuse; that Max Weber was not primarily focused on bureaucracy; that management ignores its deep relationship to chattel slavery; and that historians (most likely Wren) have cherry-picked the evidence to provide legitimacy to the field of management.

At its best, therefore, the postmodernist approach forces the traditional scholar to focus on questions and issues that we have ignored. One of the particular strengths of postmodernist research in management is that it forces us to puncture many of the myths that surround our profession such as the sanctity of the scientific method and the peer review process. The myth goes that management scholars hypothesize based on theory, collect data, analyze data, and then provide a write-up. This is often not the case. Hypothesizing after the results are known is more common than we care to admit; we cannot even define one of key terms, theory; and the peer review process probably rewrites manuscripts more than we are prone to admit. We also tend to believe that methodological problems only exist when they appear to be a myth or a gross overstatement; we perform statistical analysis when none of our survey methods are random; we often do not test the theory as intended. This is to say nothing about the lack of common definitions on basic terms such as job satisfaction and strategy.

Yet, despite the above strengths, postmodernism turn is rife with problems. One of the issues that the new management history does not solve and, indeed, exhibits the same sin is how management history is taught to students. To really make the past alive, to make it relevant to the business student, is one of the most difficult aspects of education. I believe that despite the changes they make to history, they do not bridge this issue. Indeed, their embrace of postmodernism would appear to be anathema to the business student who prefers empiricism. From my observation, undergraduate business students typically want facts and practical application. They do not want long debates on the issue of subjectivity and power. Instead, the vast majority appreciate the essential role that management plays in both the individual business and the economy at large, matching supply with demand and instigating the efficiencies that engenders increased wealth from fewer resource inputs. Accordingly, it is not only a matter of the obscurantist postmodernist writing style annoying the typical undergraduate and frustrating the master's student with concepts that are difficult to relate to practical problems. It is also the case that postmodernism seeks to actually destabilizing management as a concept. Although some may concur with such a "deconstructionist" action, I take an opposite position, agreeing with Bowden (2018) that "modernism" (i.e., our modern industrial society built around free enterprise) and management (i.e., the mechanisms for maximizing the potential of free market societies) need to be defended as both sources of wealth and freedom.

Another problem is that there is really nothing new or original within postmodernism. Rather, it is a simply more radical reiteration of the relativism practiced by Carl Becker and Charles Beard, two past Presidents of the American Historical Association, during the 1930s. As with today's postmodernist, these two historians "mocked the notion that the facts speak for themselves" and dismissed the "objective

reconstruction of the past” as “a vacuous ideal” (Novick 1988, p. 293). These relativists from the 1930s also resembled today’s postmodernist in arguing that the constraining influence of wealthy donors limited the objectivity of history; that activist practitioners dominated the field in the interests for the status quo; and that the field was dominated by Anglo-Saxon Protestants who left little place for the “professional historians of recent immigrant background, none [for those] of working-class origin, and hardly any [for those] who were not Protestant” (Novick 1988: 61; 68–69).

Looking back, it is not surprising that relativism would come to dominate after the First World War as God died in the mud in Flanders Field. Much like the post-Civil War generation, this generation found all Gods dead and that there were no absolutes and little to die for. *Goodbye to all That* was the mantra for those that survived the war. It is not surprising that postmodernists such as Foucault would reject both Marxism and capitalism and in the process adopt a largely nihilist philosophy. The post-Cold War world has become the age of fracture, where shared meanings and viewpoints are falling apart, where structures no longer seem to hold, and where – despite the best attempts of society – racism and sexism remain constant and permanent (Appleby et al. 2004). There are no sunny uplands. A “postmodern” view is also found in the work of Thomas Kuhn, who suggests that science is driven by social factors and not scientific facts (Appleby et al. 2004). A “Kuhnian” view can thus be seen throughout the postmodernist literature in that historical narratives are socially constructed.

Another problem with the new management history is that it ignores the most salient force of history: time. One particular question if we eliminate time periods, how can we organize history? This would be akin to eliminating chapters in a book, stages in a play, or yards on a football field. Time periods make history understandable and approachable. Likewise, whereas from a certain (generally accepted) perspective, we perceive two boxers in the ring as two separate entities, an alternate reality, using quantum mechanics, suggests that the boxing match is merely a single concept, as they are merely two sets of molecules colliding in a vast empty vacuum. Of course, if we say that they are the same, and that Ali and Frazier were the same entity, we would be laughed at any sports bar or pub. One way to consider is whether the First World War and the Second World War existed in a meaningful sense. From one perspective they are the same – featuring a revisionist Germanic state – seeking to overturn the old imperial world of Europe with a new imperial world. Therefore, from this perspective, the Kaiser was merely the dress rehearsal for Hitler. Of course, there was a vast difference between the Kaiser and Hitler. Ignoring the difference between the two wars is to say nothing of the relative rise and decline of powers such as Great Britain, the United States, France, and Russia/Soviet Union. Despite the artificial nature of time periods, they are useful and accurate devices to describe the past.

The largest problem with postmodernism is that it can lead to the denial of known facts. Gordon Wood, winner of the Pulitzer and Bancroft prizes, one of the great historians of his generation, and the top historian of the early American Republic, has stated that a postmodernist would have a difficult time stating that a Holocaust denier is wrong, because one historical opinion is as valid as the next (Wood 2008).

Wood's argument is thus similar to that of George Orwell, when he points to the fallacy of an insistent belief that $2 + 2 = 5$. Rather than leading to a broadening of approach, such arguments merely result in error. As Appleby et al. write, "Postmodernism renders problematic the belief in progress, the modern periodization of history, and the individual as knower and doer." History becomes nothing more than another measure of control. Worse still, since history is subjective, we can change history to suit our needs since there is no "reality," only our poor ability to describe it. Hence that is the reason why Taylor is now socially responsible.

ANTI-History

ANTI-history is one of the most ambitious attempts by management historians and historians in general to develop a framework to describe how history is made (Duperos and Mills 2012). Their ambitions are justified in many ways. I have a real appreciation for it – even if I feel that take their arguments too far. ANTI-history argues that knowledge is the product of actors and networks, whose actions determine how people see the world. Therefore, since history is a product of actor networks, it is necessarily subjective. Accordingly, written history is nothing more than a story that supports the needs of the actor networks. This line of argument suggests that as the actor network changes, therefore, history changes – making the past immutable and history subjective. Consequently, there is little in the way of facts and evidence. Instead, what we have are merely the mental images and justifications of those in the network.

Duperos and Mills have written a very interesting and useful framework. Unlike other postmodernists they address some of the key problems with postmodernism by positing that history is necessarily subjective and that the historical record is a fusion of the authors network – a network which includes archivists, other historians, and the public. In addition, the historian has within their network the actual historical figures who impact upon the historical record on the basis of their actions. For instance, in their chapter on the removal of German emigres from an airline that Pan Am had control over, Durepos and Mills correctly demonstrate that historical actors *can* transform the narrative to fit their political needs. I believe this framework has great utility for use in certain types of archives, most particularly corporate archives. In fact, I believe that the approach advocated by Durepos and Mills is a necessary and essential one for those examining the inner workings of how corporations save records.

Yet, despite its strengths I do have two issues with the ANTI-history. Firstly, I find the level of cynicism to be extremely high. I would not say that Duperos and Mills have a conspiratorial view of the world, but it is close. Namely, I would dispute their view that archivists can influence the historical record in a negative fashion. My experience of archivists is that they wish to maintain the historical record as it was – the warts and all. Perhaps, my experience is shaped by dealings with government or academic archivists (rather than corporate). But I believe that it would be a gross dereliction of their responsibility and professional ethics for any archivist to manipulate the archival record. And I doubt that any significant number resort to such unethical behavior. Secondly, I believe that the handling of certain issues by Durepos

and Mills – such as the removal of Germans from Pan American’s subsidiary – does not fully consider the historical record. I could buy into the fact that Juan Trippe would act opportunistically by removing the pilots to maintain control. Yet, I believe there is a shade of gray: American military leaders were concerned, even during the 1920s and 1930s, with the rebirth of German might. Given how closely aligned air industry was to the military (and how closely Trippe worked with the government), he would have acquiesced (one suspects) to the viewpoint of the American government and military.

Perhaps more troubling is that historians have been using this type of framework (i.e., one that gives primary to actor networks and interests) for years. When I was an undergraduate at Gettysburg College many years ago, we read two differing frameworks of the Reconstruction written by two different authors. The first was written by a Hilary Herbert that was highly critical regarding many Reconstruction policies; the second was written by Kenneth Stampp that was favorable and wished that Reconstruction had worked. Herbert had fought in the Civil War on the Southern side, was from South Carolina, and had opposed racial integration. Stampp was a Northerner from Wisconsin, who came from a family with strong progressive tendencies. He also wrote during the 1960s, which some have called the Second Reconstruction. The lesson was when reading history, consider the historian. I never read history the same way again. However, scholars have been aware of this type of technique for years. But, Durepos and Mills do provide a theoretical framework.

But even more troubling is that there are times when the actor network theory does not hold. In fact, there are numerous times when people go against their network (Lukacs 2011). One particular example would be the experience of Ronald Radosh (2001). Radosh was what is known as a “red diaper” baby, meaning that his parents were members of the Communist Party of the United States. Party members were devoted to the party: they lived together, worked together, and socialized together. They also had articles of faith. These articles of faith could not be questioned. To be a communist meant that your life’s values were determined by Moscow. One of those articles was that the Rosenbergs, the atomic spies, were innocent of the charges brought against them. Radosh’s memoir recounts how this fact was pounded into him as a young man. But Radosh rebelled: he abandoned the party and became a member of the New Left. When Radosh started writing the major study on the Rosenbergs (with Joyce Milton), Radosh came to the conclusion that they were guilty. Radosh wrote this knowing that it would completely lead to him being ostracized in the academy. How would ANTi-history explain this? There was little in Radosh’s network that would suggest either his level of honesty or his willingness to break with previous beliefs. However, these are mostly quibbling. I think this framework could be used to describe how traditions emerge.

Other Histories

Two additional histories I would like to discuss are Gerard Hanlon’s *The Dark Side of Management* and Morgen Witzel’s *A History of Management Thought*. Hanlon’s (2015) book is written from a critical even Marxist perspective and builds on

Reinhard Bendix's work, *Work and Authority in Industry*. Like Bendix, Hanlon proposes that management thought is highly ideological and contentious in nature. But Hanlon goes further in that he argues that the central purpose of management is to alter social relationships so as to shift money from one class to another. Management is not about wealth creation, efficiency, or effectiveness. Rather it is a "political endeavor" whose purpose is to modify social relationships so that market-based solutions, competitiveness, self-care, and individualism run paramount. Hanlon also argues that management is necessitated by labor's refusal to bend to capitalists, necessitating managerial action to circumvent or undermine this refusal. It is from this perspective that Hanlon contemplates the policies and understandings of Taylor, Mayo, and Ford. This is an interesting perspective and one that I am sure will gain great esteem in management critical studies.

Let me state that I disagree with Hanlon's framework. That being said, he does such an artful job that I could see and understand his perspective. For example, I agree with him that concepts related to market wages, utilitarian work values, monetary incentives, and individualism were not originally part of the typical worker's perspective. It took political and social movements (one of which management is one) to bring workers to the perspective of modernity. I also agree that we should consider worker perspectives more fully when we write about various management interventions. I would also agree that management is a form of social control – something that management scholars are often not aware of, if they are aware, something that they prefer not to be aware of. I also believe that he has some apt things to say about the relationship between Taylorism and Mayoism: in that Mayoism could be seen as an extension of the former.

I find, however, his arguments to be unconvincing. In large part this is because Hanlon views every action taken by a manager with a degree of hostility. It has long been noted that managers were often racist and unwilling to hire African Americans. Henry Ford did that – he seemed to be unbiased in hiring even if he did primarily hire African Americans to dangerous jobs. Yet Hanlon seems critical of this, noting that Ford hired immigrants and African Americans due to the fact that native-born Americans were generally reluctant to work on Ford's assembly lines. In addition, Hanlon is selective in his use of evidence. He cites with approval Bell's and Moore's criticisms of the Hawthorne studies, but he never cites or discusses whether or not those criticisms were fair or not. It is difficult to see, in reading Mayo's writings, a justification for Hanlon's view that Mayo was really adjusting "man to machine." What Mayo was more likely trying to do was to discover what managers could do, besides contracts and government action, to promote spontaneous cooperation and reduce the amount of labor upheaval at the time. Likewise, it is difficult to envision how worker resistance leads to wealth. Anyone familiar with the work on counterproductive work behavior would understand how false that statement is and that worker resistance leads to exceptionally high costs to business. In addition, it is a facile argument that management is blind to the dark side of management – there are a consistent series of books, articles, and other materials about management shortcomings.

Lastly, Hanlon is critical of two aspects of management. The first is management's claims to scientific objectivity, and the second is the claim that management promotes hierarchy. In terms of management's claims to scientific evidence, it is true that Taylor's work, although it had the residue of science, was not as scientific as he claimed. Likewise, scholars have noted for years the problems of the Hawthorne studies. Yet, research is driven by empirically testing theory, measurement, and evidence-based research. Although, there are quibbles regarding management research (we could be much better on replication), we do try to use scientific methods in gathering research. If anything, management scholars are aware of the vast complexity of research – hence the strong field of contingency management. Likewise, I am baffled by the claim that management is based on and promotes hierarchy. Yes it does, but I could not think of a society, except for maybe some nineteenth-century utopian societies, where there was not a hierarchy. In fact, modern management probably is a better hierarchy since there are systems that promote meritocracy. I am not sure if Hanlon promotes a romantic view of the past or the future in that there are no power differences.

Morgen Witzel is the author of *A History of Management Thought*, a work which, I believe, provides the best survey of management thought outside of Wren and Bedeian. Witzel is in many ways a bit of an outlier. He is more of management consultant and practitioner than he is an academic, although he is a fellow at the University of Exeter. I find Witzel's work to be an interesting addition because he asks the question that I have been wondering for a long time – when did management move from an applied science to one based on theory building, data mining, and nonpractical results? Witzel has proposed that beginning in the 1890s, when management started to emerge, the new profession of management was desirous of the professional respect enjoyed by law, medicine, or accountancy. This societal respect has, however, not happened. It has not emerged. Instead, management finds itself disrespected, without honor. Even some of its best thinkers, such as Jeff Pfeffer, decry the lack of respect of management in the field of social science. Witzel argues that part of the problem is that management is ignorant of its past, it does not cite seminal articles of the past, or, if we do, we do not read them.

Witzel also proposes that management started to lose its way during the 1940s when the Carnegie School (i.e., a school of management thought identified with academics from the Carnegie Mellon University in Pittsburg) emerged to challenge classical management. What is notable is that the Carnegie School provided one of the first, sustained attempts at theory development by academics for academics. While Elton Mayo was a member of the Harvard Business School, he addressed actual managers more than he did in academics. His associate Fritz Roethlisberger was similar, although he had more of a foot in academia. George Homans, another of Mayo's students, moved over to sociology, where he developed theory. Yet, Carnegie was firmly entrenched in academia and taught business, as William Starbuck noted, as one would teach physics. Despite all the praise that Herbert Simon, Richard Cyert, and James March gained, however,

there is also some justification in the criticism that they started us down the road of abstruseness.

I like Witzel's approach as he writes like someone who is heavily engrossed in the field in terms of both research and practitioners, unlike Wren and Bedeian, who write in a more scholarly way. I disagree with Witzel. I believe we do not cite historical arguments, because it would reveal that the emperor has no clothes – most of what we research today is impractical or similar concepts restated. Modern management is more precise and scientific, but have we, in any way, advanced the tools that manager's actually use? Would someone who is a manager notice a real difference between self-determination theory and two-factor theory? Likewise what would a manager say about some of the insights that we have produced? For example, one suspects that most managers would say that the supposed theoretical insight that resources are a source of competitive advantage is something that is self-evidently obvious. Similarly, would a practicing manager care about the various amounts of vocabulary/theory building and item analysis that we do? I am not sure a manager would be impressed by this. Nicholas Butler's famous statement rings true: "An expert is one who knows more and more about less and less until he knows absolutely everything about nothing." Significantly, those management thinkers, like Michael Porter, Tom Peters, and Peter Drucker, who have attempted to move past and offer applied evidence, are often attacked.

I do have a couple of quibbles with Witzel's framework. Firstly, he does not cite many articles from the *Journal of Management History* and other academic journals. For instance, if he wrote about John Hassard's work on the Hawthorne studies, he might have a differing perspective. Likewise, his perspective on certain issues would change if he read more about the contextual history in which management occurred. For example, was the Progressive Era more about constraining markets or about setting them free? Probably it was both, depending on what vision of progressivism someone adhered to. Likewise, while he mentions the importance of the Second World War on management, he fails to sufficiently reflect upon the esteem which wartime managers were able to garner. If he did, he would note that management's current low status was not always apparent. Likewise, he overstates the amount that Henry Ford borrowed from scientific management. Indeed, some Taylorites did not believe that Ford used scientific management at all. In fact, given that Ford did not read much, it makes sense that he may developed something similar on his own. Lastly, I believe that Witzel protests too much about management's status. Again, who are we less prestigious compared to? Management academics typically make more than economists, sociologists, and psychologists. Although we are not as highly compensated as accountants, the demand for accounting has exploded over the years due to the regulatory environment. Accounting research is also difficult to undertake. Likewise, the demand for operations management has waxed and waned. Although management as a discipline maybe on the downside, new challenges are always emerging to provide opportunities for those scholars who are intent on solving them.

Myths

We consistently hear the term myth regarding management history. We hear about the myths of Taylorism and Hawthorne studies. In fact, some scholars concede that both myth and historical reality can often contend with each other. Mainstream history is characterized by a similar dichotomy. Our memory of Lincoln is often very different from what actually occurred. One particular reason is that we use Lincoln as a perspective to determine an answer to the question: What is America? What America should be? What should we as a society to be based on the ideals of Lincoln? As David Donald (Donald 1956) has noted, politicians often try to get right with Mr. Lincoln. In this case, history serves as a common framework, a useful myth to rally society (Peterson 1994). In scholarly professions, we often use myth as a means to provide us with a sense of legitimacy. Management has sought – over the years – to seek legitimacy through several different methods, the use of theory building, the scientific method, and practical application to provide us with a sense of legitimacy (Pfeffer 1993). As a new field, we have status anxiety and envy toward other more established fields. This is a common trend throughout science. Economics has envy of physics and sociology as an envy of economics (Steinmetz 2005). In fact, scholars such as Jeffrey Pfeffer (1993) have bemoaned the lack of a clear paradigm in the field of management, noting that we are losing ground to other business fields.

We have also used myths to provide legitimacy as well. Perhaps, it is the reason why managers start the process of management with Frederick Winslow Taylor, rather than such flawed figures as Henry Ford, whose violent anti-union policies and anti-Semitism made him a comprised figure (Lacey 1986). Perhaps, had Ford died in 1918, soon after his electoral defeat, when he was compared to Moses, we would have used him as a model. Yet, we also focused on Taylor for reasons that were historical as well. Namely, that Taylor appeared at the right time and place for management to emerge as a distinct field. A check of how Taylor inspire thinkers is clear. While there were other efforts to understand social man, the Hawthorne studies were the study that inspired scholars to reexamine the field to consider the role of social motivation in terms of economic motivation. Likewise, checking a listing of citation numbers would reveal what contemporaries believed about the Hawthorne studies and how it inspired thinkers (Muldoon 2012). It is important to note, therefore, that the complexities of history usually do not lead to a ready reduction.

To debunk myths, management historians often create strawmen. Wrege and Perroni argued (pg. 26) that scholars should “think to look at the idol early enough in the game to discover that it had feet of clay before it was hoisted onto a pedestal.” I am curious as to how we answer the question in management: who do we idealize? One of the favorite targets has been the Hawthorne studies. Firstly, scholars have, for the last several years, pointed out that the Hawthorne studies were poor methodologically, that they proved nothing, and that Mayo had a biased viewpoint (Gabor 2000; Hassard 2012). Secondly, we have also been told that that the Mayo did not discover “social man” and that its discovery had been researched many years before.

These concepts were not therefore alien to people who wrote at the time the studies gained fame. In addition, the argument that welfare capitalism and human relations were similar concepts is something that is objectively false (Wren and Bedeian 2018). If anything, welfare capitalism encouraged worker revolt – which human relations attempted to solve. The question should be: why did the Hawthorne studies gain prominence?

Of particular impact on the field has been the work of Charles Wrege. Wrege is the one historian that postmodernists and traditional historians can all say a good work about. There are many positive things that could be said about his work and him as an individual. He was never too busy to help younger scholars and never too satisfied to just accept established facts. As a historian, Wrege was an empiricist who would be willing to cross an ocean to verify a comma. Postmodernists respect him because he challenged norms. Wrege was, in some aspects, a model scholar. In other ways, he was not. Wrege spent too much time trying to gather truth that he saw the world in black and white and in the process produced, in my opinion, non-judicious history. Wrege was so driven to criticism that he not only lost sight of the forest, but of the trees as well, focusing on branches. I am not sure if Wrege was the one that introduced the practice of calling history myth or the ongoing hunt for originality – but he certainly contributed to these trends in the field.

Wrege's work on Taylor exhibits this sin. Whenever there is an issue with Taylor's account, Wrege automatically denounces Taylor as a scoundrel. For example, Wrege and Stoka call Taylor a plagiarist, but they overplay their hand. Morris Cooke did write some of the *Fundamentals of Scientific Management*. But it was based on a speech that Taylor gave. So it was Taylor's ideas, with Cooke as a contributor or to use a phrase, a ghost author. Taylor also published his book with Cooke's knowledge and consent, even offering to provide royalties to Cooke. Hough and White point out that it was Taylor's ideas and published under Taylor's name. Taylor was the author in the "true sense" in that he assumed responsibility and was published using his authority. Taylor was not publishing an academic paper but was relaying a story using memory devices that make a connection with previous knowledge. Martha Banta makes a note that Taylor's stories served as a way of passing the principles of scientific management through a narrative device. They were a selling point – not a scientific paper.

Often times scholars point out that there is no Kuhnian paradigm in management history, noting that scholars have had different takes on issues related to Taylor (Novicevic et al. 2015). Some view it as a myth; others view it as a good viewpoint. Of course, neither history nor science ever reaches a consensus on many issues. Despite the protective powers and validity of intelligence testing, there are still scholars that challenge the conventional viewpoint. Likewise, despite the evidence, there are still people who believe that the earth is flat. In fact, scholars often impose consensus on other scholars through control of journals and funding in the field. Yet, even if there is a consensus, there can still be divergent voices.

The question of originality has been an ongoing issue since at least the time of Hoagland (1955), whose article pointed out that Taylor's work had been preceded by Charles Babbage. We are also pointed to the fact that there were many

antecedents to the work of Fayol and others over the centuries. One of the most recent tendencies has been to try to find origins of concepts – even among those long forgotten figures who were not management thinkers. While this searching for intellectual roots has long been the domain of traditional scholars, the postmodernists have taken this issue to the extreme. If we believe that history is completely fictive, then we can cut history to fit any fashion we desire. Meaning that we can select historical figures that fit what we regard as modern values, distorting the historical record. This would exactly the thing that Winston Smith did in Orwell's *1984* when he changed history by distorting the war records of Oceania and eliminating unpersons.

Conclusion

At present we are at the crossroads in management history. The dominance of the LSU-Oklahoma is ending. Both Wren and Bedeian have retired from teaching, meaning that there are no new students. The postmodernist school, with bases in Europe and Canada, seems to becoming more and more popular. What the postmodernist school is doing is ripping up many of the established nostrums, challenging many of the standards of traditional management history. In part it does this by noting that history goes beyond the great man model and by considering different voices. Of course, such postmodernist approaches have been tried in mainstream history – with disastrous consequences. As postmodernist influence has increased, so has the field of history in the United States (and elsewhere) witnessed a gradual decline of majors, interest, and relevance to modern life. Most academic history today is jargon-laden works of figures of little historical importance that has little relevance to ongoing debates in politics and the society at large. By contrast, the 1950s witnessed the likes of Arthur M. Schlesinger Jr., C. Vann Woodward, and Richard Hofstadter, each of whom provided works that informed the political culture of the 1950s. If we cannot understand the past, then why should we research it? If the figures of the past, with their brilliance and stupidity, nobility, and venality, have little to teach us, why study it? Winston Churchill, himself a great historian, always urged aspiring statesmen to study history so they could understand the present and the future. Churchill himself understood that his historical writings provided himself with a unique understanding of the present. For example, his writings on his great ancestor the Duke of Marlborough provided him with an understanding of two things. One, a person could be dead politically (as the Duke and Churchill were), and events could resurrect them. The second was that Marlborough's struggle against one would-be tyrant (Louis XIV) provided Churchill an understanding of Hitler that his contemporaries did not possess.

This is the crux of the matter – perhaps the one issue modernists and postmodernists can agree upon – that management needs to understand its past in order to improve its future. History teaches us that commonly agreed upon ideas – the best wisdom – are often wrong and that there are few Solomons out there. It also teaches us that history often repeats itself, especially in the social sciences, where

common models wax and wane upon importance. If professional maturity is a gift, according to Bedeian, then it is treated like a pair of socks. We need to understand our field's past to understand its future. Yet, the inability of postmodernism to consider even basic facts damages the prospect for consideration.

At present, we are witnessing some contentious debates about management history. This is a good thing. It suggests that as a field we are growing, becoming mature, and perhaps gaining a greater degree of acceptance in the field. In many ways, management scholars do not study the past as much as they should. What they do learn is often inaccurate, simplistic, or an outright canard. Right now there is a dearth of management historical education in the United States, perhaps in other countries as well. When scholars do study, it is usually just Chap. 2 in principles of management textbook. Perhaps this is why we have so much of the "Columbus syndrome" in management in that we often rediscover what has been researched. The phrase old wine in new bottle is an apt description.

However, there are better and worse methods to research the past. Some such as postmodernism seem to offer some theoretical or reasoned approach. However, postmodernism is actually in decline in mainstream history and even literature. Would this approach gain increased respect in a field that is devoted to empiricism? I have serious doubts. In addition, this approach is dangerous in that we can play with facts to create narratives that support our own understanding of the past. In the *NHM*, we have learned that Taylorism was about conservation, a point that has been missed by a generation of historians, who have written extensively about the efficiency movement and scientific management. The *NHM* did so, because they wanted to make it seem that management has always been socially responsible. If they could make that change, what else could they do? History is not mad libs. We are constrained and bound by evidence. We cannot speculate or stretch the evidence to fit our world view. There is a reason why George Orwell made Winston Smith a historian of sorts.

Likewise, writing history without timeframes is difficult and perhaps even impossible. Timeframes do not emerge from the blue. Rather they emerge from a historians' consideration of events. Usually, these emerge with little difficulty. For example, if we were to write a world history, from 1945 until 1991, we would dub this period the Cold War, due to the confrontation between the Soviet Union and the United States. Likewise, in management history, we have clear lines, periods in which certain ideas are in fashion and others are not. That being said, one of the minor weaknesses of the Wren framework is that the current period is not as well developed. I believe examining Witzel's work would be beneficial, in that the obvious divided would be the separation between academics and practitioners.

Some scholars have urged that we adopt a Marxist perspective of management history. This type of approach would downplay biography and focus on structural and situational cues that are usually economic in nature. For example, a Marxist would analyze and argue that management is the result of various economic contingencies that have impacted society. The spread of the industrial revolution and the demand for coordination meant that managers were needed to handle the size of various corporate behemoths. While this analysis is correct, it does not explain

either the innovator or why nature of the discovery. For example, Taylor tended to focus on the most menial tasks with the dimmest of workers. “Schmidt,” an ethnic and social stereotype created by Taylor, is not just a story device; it is also emblematic of a man with a deep bias against the lesser type. There are numerous disparaging remarks about Italians, Hungarians, and African Americans throughout his work. Taylor’s heavy handedness prevented him from getting greater acceptance. Had he been more accepting and flexible person, his critics would have been lessened. Likewise, Mayo’s inability to write for the academics, the lack of clarity in his thought, and his unwillingness to explain himself to his critics limited his appeal. Scholars in management history are focusing on critical biography and focusing on how factors influence scholars. A pure Marxian approach would have limited the appeal.

Cross-References

- ▶ [Debates Within Management History](#)
- ▶ [What Is Management?](#)

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