Editorial

This is my second solo editorial as editor of the *Journal of Management History*. Like my predecessor, Brad Bowden, did in his first editorial, I have laid out my views of what management history is and what this journal should pursue as research. For this editorial, such a philosophical overview is not needed. Rather, I would like to develop the importance of management history through the articles in this issue. Each of these articles is excellent and drives home the importance of history in the practice and research of management. As such, they are in-line with my first editorial – whereby I laid the parameters of the journal. Please note that these essays were also accepted under my coeditorship with Brad Bowden.

The lead essay is by Gabrielle Durepos and it is a response to James Reveley's criticism of ANTi-History. First, both the criticism and response are exemplars of scholarship and professionalism. Debate is important because it is the mechanism through which any academic field grows. Second, ANTi-History is one of the most important and influential contributions to management history over the past 10 years. In fact, ANTi-History has featured prominently in the literature, and we have had such major figures, such as Jean Helms Mills, declare themselves ANTi-Historians. Even more flattering is Suddaby's (2016, p. 46) comment that ANTi-History "holds high potential for bridging business history and management theory." Third, ANTi-History has become so prominent that, as Durepos admits, it has gone well beyond what she and others had intended, suggestive of widespread adoption.

Based on these percepts, Durepos sets out to rebut some of the claims offered by Reveley. First, this article cuts down to the idea that the criticisms offered stem from different epistemological understandings of what constitutes both history and, even, reality. Second, issues occur from a partial understanding of what ANTi-History is. For example, others and I have pointed out that we "have observed that ANTi-History sees all knowledge of the past as subjective, and consequently, there is 'little in the way of facts and evidence'." However, Durepos offers the following criticism:

> [. . .] to be fair to Bowden, Mollan, and Muldoon, these opinions were intended as criticisms. However, there are dangers in presenting matters of opinion as fact, and in this case, what is factually incorrect about ANTi-History.

Mostly, the question that Reveley most expertly raises is that a subjective perspective may ignore objective truth. Dr Durepos argues:

> [. . .] rather, it (ANTi-History) seeks to show how knowledge pegged as "objective" and "truthful" assumes those labels. Specifically, ANTi-History exposes the process in which histories are made and situates the outcome (what) as related to, or arising from, the process (how). The objective is to show that versions of the past-as-history assume their plausibility in everyday, ordinary practices.

One may wonder that both sides are talking past each other, but in both the criticism and response, both sides are expertly laying down their case.

Because I feature prominently in the article, I would like to respond in that I agree with many of the percepts of ANTi-History and it is something that historians have done for years, namely, when considering history, consider the historian. What is noteworthy about this approach is that Durepos and others have created a deft method and theory to explain this practice. Accordingly, two of my papers (Muldoon, 2012, 2017) can be interpreted through the lens of ANTi-History in that different groups of scholars have very different

understandings of the human relations movement. Therefore, ANTi-History should be in the toolbox for any historian. However, I also stand by my criticisms; it can be taken too far. The argument that knowledge is subjective is itself a potential objective argument. But this should not cut too deep. When used properly, ANTi-History can lead to truly illuminating results.

Dr Linkletter and Dr Tabesh find that Peter Drucker was highly analytical in his use of language, and that he was neutral in terms of polarity, sentiment and subjectivity. They argue that others have made these criticisms of Drucker, but no significant evidence to support these assertions is found in this study. The authors acknowledge the fact that Drucker's work has been considered unscholarly and unanalytical because he never embraced extensive sourcing and citations; however, they pointed out that his evidence sources are drawn from multiple disciplines (history, psychological, the social sciences, the arts, etc.). Thus, although not analytical in the sense that managers may expect, he is drawing larger arguments and conclusions from a vast array of evidence. The authors also find a balanced attention to social and economic phenomena in Peter Drucker's work.

More than the writings on Drucker, what this article does is to provide a quantitative method to examine the use of language in helping us to understand seminal works and thinkers in management literature. Accordingly, this approach can lead to some interesting findings and, when used with a theory (maybe actor network theory), helps to explain some of the findings, facts and assumptions in our field. For instance, Drucker was once highly esteemed by academics because of his accessible writing and humanistic values. But as the field became more scientific, his prestige diminished, suggesting a changing zeitgeist.

Mark E. Mendenhall, Frank C. Butler, Philip T. Roundy and Andrew F. Ehat have teamed up for an excellent paper on The Church of Jesus Christ of Latter-day Saints and top management. This paper again demonstrates the importance of history as a tool of management research, as these authors use this case study to further theorize on top management teams, one of the most important topics within management. This article is noteworthy for several different reasons. First, it uses a religious organization as its focus and in doing so, makes a return to the focus of Chester Barnard, one of the forefathers of organizational theory, whose exemplar of an organization was the Catholic church. Second, it illustrates the ongoing importance for securing behavioral integration (BI) in top management teams and the processes that foster and preserve it. The formation and continuation of the church is a remarkable story given the tremendous amount of discrimination that its members suffered over the years, including its founder, Joseph Smith, who was brutally murdered.

This paper makes several important contributions to the literature. First, based upon the study's findings, the authors derive a process model of BI formation and preservation, addressing a long-standing gap in the BI literature. Second, it highlights the importance of iterative education and socialization in producing and maintaining BI over long time periods. Third, in addition to iterative education and socialization, the authors found that top managers must engage in conservation strategies to preserve BI culture in top management teams. Accordingly, the authors expand a field of research that has been a popular and important one for about 40 years. As such, this paper demonstrates the importance of history for theoretical contributions in management.

Chelsea Sherlock, Erik Markin, R. Gabrielle Swab and Victoria Antin-Yates have provided a literature review of family business, an important field of research, but one that has not received the attention that it is warranted. It should be noted that most of our theories on firms, such as agency theory, have been based upon publicly traded corporations, rather than family firms, despite the family firm being the most common type

of business around the world. The authors do not use historical methods that have been common in management history. Rather they use a literature review, a common technique in science, to demonstrate the changes of a field throughout a period of time. Because they are examining how a field has changed over time, they are using a technique of history to uncover changes in a field of research in management.

In this case, the authors use the literature review to accomplish several tasks. First, in the literature review, they use this technique to demonstrate the importance of family business as a major topic within the field of management that has witnessed a five-fold increase since 2007. Second, they found six core topics with 21 different subtopics. Third, they also demonstrate how these categories have grown. Fourth, they show the impact of key scholars, influential articles and journals in shaping the field. Based upon these contributions, this paper is destined (and should be) read in doctoral seminars and by established scholars who seek to understand research on family business.

Yuwei Sun and Jon Billsberry's paper on value incongruence demonstrates the ongoing relevance of viewing articles from the perspective of the past as well. One of the common occurrences within the management literature is that there is a mismeasurement and misuse of a construct. There are several reasons for this occurring: a change in theory, a recognition that the content domain has been misapplied and a lack of understanding due to a focus on making arbitrary decisions. These are all common issues that emerge in research as scholars sometimes become too focused on their paper or a field of research. However, taking a perspective from history allows trends to emerge and scholars to see whether there are issues with a construct.

Sun and Billsberry offer an excellent article in this regard in that they examine perceived employee misfit (PEM). They find, based on their view of the literature, that "in the absence of alternatives, researchers have taken low scores of value congruence as a measure of misfit." They also found that most studies of PEM demonstrate the importance of "values, supplementary conceptualization or interactions with the organization (rather than interactions with other employees, tasks, etc.)." In addition, there is a measurement issue in that measures focus on similarity rather than dissimilarity. They use their findings to produce four separate quadrants that could help drive research: researcher-determined studies of perceived molar misfit, researcher-determined studies of perceived molecular misfit, employee-determined studies of perceived misfit as an experience and employee-determined studies of perceived misfit as an identity. I believe that given both the complicated nature of PEM (is it misfit, identity, etc.), this paper is an important one.

Our final paper, by Klaus Brockhoff, again demonstrates the benefit of viewing business from a historical eye. One of the common tenets of historical analysis (to the point of becoming a cliché) is the idea that what is revolutionary is probably not, as it is nothing more than a fad (slightly different than something previously). In addition, there is considerable lack of understanding of the cost involved. As Brockhoff writes:

> [...] it means that a concept may appear new to a person (and thus become the subject of an evaluation and adoption decision) although it has been around already for quite some time. While early adoption research employed self-referencing models, it later drew on criteria that co-determine the adoption decision.

In fact, some of the best and most important business techniques are as old as history.

The most prominent example of this is compound interest, which dates back (perhaps) to Sumer, 2300 BC. Of course, people could not compute it easily. In fact, computing compound interest by hand might be extremely difficult or even impossible. However, the emergence of the financial calculator or spreadsheet software has made it possible for even people

uneducated in finance to be able to compute this figure. Another example would be the Black–Scholes option pricing model. Whatever the merits of the work of Black, Scholes or Merton (and they have been controversial for a while), the Black–Scholes option pricing model has changed the stock market and executive compensation. However, Brockhoff demonstrates that option pricing has been known since at least Louis Bachelier. But a ruling by the government (speculation is gambling), probability not being important to business education, and a lack of hardware prevented options from being an important topic. In fact, I still have a headache from trying to calculate Black–Scholes by hand as a finance student.

Based on these cases, Brockhoff produces a brief framework:

- An effective problem solution is presented.
- A controllable and efficient solution process or software is presented.
- A type of hardware to apply the software to the problem is made available.

Although I slightly disagree with the limitations of the article, certainly a case study can be limited in its generalizability, but it can lead, as did the article by Mendenhall *et al.*, to new theory being developed. I believe the framework that Brockhoff proposes may be of interest to students of management as well. For instance, it could well be that human relations and motivation research became more common because of the creation of measures with a Likert-based response scale.

I hope the reader of these articles enjoys reading them as much as I did.

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