What is New in "A New History of Management"?

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Abstract:

Purpose

This paper evaluates the contributions of the so-called "Historic Turn" in Organization Studies through the attempt by Cummings *et al.* (2016) to offer a new and alternative approach to teaching and researching the history of management ideas. *A New History of Management* is intended to be a provocation rather than a practical plan, and by their own admission Cummings *et al.* (2016) prefer controversy to detailed analysis.

Design/methodology/approach

This paper offers a comment and reinterpretation of a single contribution to highlight deficiencies which are symptomatic of the postmodernist research agenda around the "Historic Turn" in Organization Studies. The argument develops through a critical reading of Cummings *et al.* (2016) to determine whether theirs is a thoughtful and serious piece of work.

Findings

Cummings *et al.* (2016) invite us to revise and re-evaluate the genesis of management ideas available across textbooks. This by questioning some of the beliefs regarding the origins of management thought within textbooks aimed at both general management and the history of management thought. The premise of Cummings and colleagues is a timely and welcomed suggestion. So is their attempt to broaden the debate to alternative epistemological positions. They can potentially help to improve the emergence of conceptual and theoretical understandings of the history of managers', business and management thought.

Although far from being exhaustive, the paper points to the large number of inconsistencies and poor historiography in Cummings *et al.* (2016). This is in line with other contributions to the so-called "Historic Turn" in Organization Studies. The central argument presented by the paper is the myopic and technically poor approach of the "Historic Turn". It is the case that Cummings *et al.* (2016) fail in their attempt to offer an alternative to established textbooks or explain the development of different approaches to construct systematic studies that, over time, consider the evolution of management, managers, and those who have conceptualized their performance.

Research limitations/implications

The paper does not present new (archival) historical evidence.

Originality/value

The central contribution/ambition of the paper is to incentivize an advance of the current understanding of the origins and evolution of systematic thinking on management, managers, and business organizations. The ambition of the paper is in line with Cummings *et al.* (2016) aim to incentivize research into how textbooks address the origins of management and management thought. Textbooks in both general management and the history of management thought, and the story told in them are important tools that speak directly to the ability of historical research to help advance the different disciplines that form general studies in business and management.

Keywords:

Historiography, Textbooks, Historical Turn of Organization Studies, Critical Management

Type:

Revision Paper

Introduction

In their editorial introduction to a special edition of *Business History*, Űsdíken and Keiser (2004) noted an increasing trend in the body of work around organization theory which increasingly engaged in historical methods and explicit historical perspective to explore business organizations and their management. This apparent divorce and subsequent reunification, they said, rooted to the ahistorical character of organization studies during its development as a subdiscipline of the broader research agenda to better understand business and their management the in second half of the twentieth century. Mills *et al.* (2016) also recount how "mainstream" studies of organizations of the 1980s and 1990s were replete with examples and references to events in the past as a form to legitimize factors and explanations of the importance of a given theory, concept, or study. Zundel *et al.* (2016) concurred while noting that such practice was not limited to publications in peer-reviewed outlets but was a widespread within industry as, for instance, the use of historical narratives to establish and maintain identity claims. Zundel *et al.* (2016) also stated that the use of such narratives was problematic while their impact on how scholars engage with the past was unclear. For instance, historical narratives have been used to perpetrate common myths and serve the interest of those in power while effectively silencing and marginalizing weaker people (Kenny, 2013).

Űsdíken and Keiser (2004) noted that the re-emergence of a historical bent in the study of organizations and their management in no small measure came about because of "newer and influential research programmes" (p. 321). The first full paper of the special issue edited by Űsdíken and Keiser (2004) was the now infamous contribution of Clark and Rowlinson (2004), in which the latter summarized research based on a postmodernist agenda that had had great influence in "other branches of social sciences and the humanities" (p. 331). Clark and Rowlinson (2004) called for the use of alternative epistemologies such as that proposed by Michael Foucault as a "historic turn" in the research agendas of those other branches of social science and proposed a similar transformation was wanting in studies of organizations and their management.

One response to practices as those described above was a debate leading to the establishment of a dedicated peer-reviewed outlet called *Management & Organizational History*. In the maiden article of this outlet, Booth and Rowlinson (2006), then executive editors, outline a ten-point research agenda to guide a "a more historical orientation in management and organization theory" (p. 5). This was then to become the template for future studies. As editors of a celebratory special edition of the same outlet ten years later, Mills *et al.* (2016) claimed that this research agenda had been successful in increasing "historical sensitivity .., empirical illustrations.., and impressions of the past" as well as "..contributed to uncovering new conceptual relationships .. [and] opened the door for the use of unique and innovative historical methods" (p. 74). The same source further contended that the "Historic Turn" in Organizational Studies was instrumental in revising and revisiting "…much of the folk wisdom about people and corporations -indeed about 'history' itself- .." (p. 74).

But the various approaches and contributions have not been limited to enlightening organizational studies. According to their proponents they have also influenced business and management history as the "Historic Turn" has:

> "..produced interesting and insightful historical accounts, narratives or tales from the field. In other words, each of the various approaches, although critical of historical approaches – particularly of factual accounts and chronicles – produced interesting and plausible accounts of the past." (Mills *et al.* 2016, p. 73; see also Rowlinson and Hassard, 2013).

But in spite of these claims and as suggested by contributions such as Bowden (2018), Bucheli and Wadhwani (2014), or Mussachio Adorisio and Mutch (2013), the reminder of this paper will show that the contributors to the "Historic Turn" still need schooling on the methods to deal with the past. However precise their acute observations may be in identifying shortcomings of current textbooks and/or current practices, the tendency of such critiques to pontificate consistently refuses to articulate viable alternatives.

The reminder of this paper looks in greater depth at the nine chapters that comprise *A New History of Management*. Each of the chapters of this book maps closely at least one item on the ten-point research agenda outlined by Booth and Rowlinson (2006). A such it provides a reference from where to start a broader and more systematic critique of the body of work around "Historic Turn" in Organization Studies. My review thus invites others to engage in a similar critical discussion of both the "Historic Turn" literature as well as a revision of established notions on the emergence of business and management thought but one that leads to better and more tangible outcomes than those in Cummings *et al.* (2016).

A New History of Management – A Flawed Perspective

The book by Cummings *et al.* (2016) encompasses an introduction, nine chapters and a conclusion (which should have been called "summary"). Chapters one, two, three and nine are entirely novel. The other half the book is a reprint of previous work, namely chapter four draws on Cummings and Bridgman (2011); chapter five on Bridgman, Cummings and MacLaughlin (2016); chapter six on Hassard (2012); chapter seven on Cummings, Bridgeman and Brown (2016); and chapter eight on Rowlinson and Hassard (1993).

This earlier work was not subjected to peer review outside the critical management genre. One could argue this was a way to expand the boundaries of the field. Likewise, one could argue authors have been shy of exposing their methods and historiography to specialists. The latter may be poignant and unkind, but similar criticisms have been made to the contributions to the so-called "Historical Turn" in Organization Studies literature. For instance, Bowden (2018) states that this literature and specifically the book by Cummings *et al.* suffers "...from a tendency to make 'factual' assertions that have little basis in objective reality. Proof of this is also easily found. In their recent *A New History of Management*, for example,..." (p. 215). As document in greater detail by Bowden (2018), there is little attempt by these authors to integrate their ideas with earlier frameworks.

Since the pioneering contributions of Pollard (1965) and Wren (1972) to the more recent text by Witzel (2011) and Bowden (2018), the history of management thought draws a direct arc between today's management disciplines and the work of among others Elton Mayo, Henry Fayol, Frederick Taylor, Adam Smith and even Sun Tzu in medieval China. As the title of the book by Cummings and colleagues suggests, the reader should expect a manuscript that maps an innovative (if not disruptive) approach to research and teach the genesis of management. But it is only the emperor's clothes that are new.

The thrust of chapter one involves a bibliometric analysis of diversity while comparing architectural and medical history outlets versus those on management history. As you can anticipate, the result is that the latter was found to be narrower than the former. This quantitative assessment is used as an apparent "objective" justification for the authors to propose "a way of showing discontinuities and how things had been different, and so could be different again.." (p. 37). Here one finds at least two other themes that permeate subsequent chapters and one the "Historic Turn". The latter views the construction of historical fact as a subjective phenomenon. A narrative that is socially constructed. There is a declared hostility to objective truth as opposed to the promotion and advance of methodological relativism. Yet, as this first chapter shows, authors within the "Historic Turn" are ready to compromise their ontological and epistemic underpinnings and, when it suits them, draw on "objective evidence" (in this case, bibliometric analysis) to promote and further their research agenda.

As for *A New History of Management,* a symptomatic deficiency in the analysis throughout the book is an incomplete and partial awareness of extant literature. This is not methodological relativism but poor scholarship. For instance, in this first chapter authors seem obtuse to bibliometric studies published in *Management & Organizational History* namely Eloranta *et al.* (2010; 2017).

The second concern that permeates the book roots in the definition and scoping of management history as subject area and research category. Authors readily admit there are two peer-reviewed outlets specializing in management history (*Journal of Management History* and *Management & Organizational History*). Yet they want to portray management history as the encompassing category for systematic research into business organizations, industries and managerial practices in spite of the diversity, higher standing, greater impact factors, and patterns of publications in alternative outlets such as business history proper (*Business History Review, Business History, Enterprise & Society, Economic and Business History*), accounting history (*Accounting Historians Journal, Accounting History Review, Accounting History*) or marketing history (*Journal of Macromarketing, Journal of Historical Research in Marketing*). Cummings and colleagues rarely (if ever) publish in any of these journals. Only a couple of them were considered in their quantitative study (namely *Business History* and *Business History Review*). Their scoping of the subject area is thus severely construed. On a more positive light, there might be some currency to their conclusion, namely that there is greater "homogeneity and agreement rather than diversity and debate" (p. 312) in the study of the long-term evolution of management and business.

Chapter one also introduces Foucault's archaeologies and genealogies to revisit a number of events and institutions which, according to the authors, are primarily responsible for a partial and biased view of the history of management thought. It is worth noting that in line with the "Historic Turn" literature *A New History of Management* excludes any critique to Foucault or indeed references to other alternatives to Positivism such as the work by Bordeou, Karl Marx, Derrida, White, Latour, and others. *A New History of Management* is therefore unable to clearly present is conceptual and epistemological positions. Lack of evident intellectual roots is not only poor scholarship but questions the validity and reliability of their critique.

The possibility of a new "deeper and more critical historical understanding of moments that have been defined as key in management's development." (p. 313).

Here appears a third theme of this book, namely the tedious and repetitive reminder of Foucault's grand method which, as chapters two to eight show, is far from novel and has been used by the authors themselves in the study of management history for the last 20 or so years. In a nutshell, chapter one is full of good intentions that the rest of the book fail to deliver.

Chapter two questions whether the ideas of division of labor and efficiency were a central concern for Adam Smith. This is important to revisit because, the authors allege, textbooks in management and management history lead us to believe they were important for Smith when it was not the case. This point is well taken and so is their questioning of the narrative presenting a linear evolution in management thought. I fully agree with this claim. This as those made by the likes of Witzel (2011) that draw a direct line between the building of Egyptian pyramids or nineteenth century warfare in Prussia with modern day business are dubious and built on a rather loose conceptual framework (based on what managers do rather than what they are) and a lack of systematic empirical evidence.

The effort of Cummings and colleagues to revise Smith's texts is commendable. Yet their failure to position Smith in historical context is disappointing as little accords to either textual or historiographical evidence. There is no reference, for instance, to the work of notable Smith scholars like Paul Tonks and Reinhard Schumacher, let alone to the philosophical links between Smith and John Locke or John Stuart Mill, either of which is essential to understand today's conceptual thinking in business and economics.

Equally important is the work of Davidson (2013), who pointed out how Smith's writings and views were strongly influenced by the civic politics of mid-eighteenth-century Edinburgh in particular and Scotland more generally, and the corruption of plutocrats. Davidson convincingly (and wittingly) claims that Smith's reference to charter companies and retainers is an allusion to people like the Sir Lawrence Dundas (1710-1781), first Baronet of Kerse, governor of the Royal Bank of Scotland and Member of Parliament (Larnak Burghs 1747-8; Newcastle-under-Lyme 1762-8; Edinburgh 1768-81; and Richmond 1780-1), who made much of his money trading arms during the seven-year war against the Jacobites in Flanders and used loans to tradesmen and others to buy votes as well as manage the local labor market. He was subsequently ousted by Henry Dundas (1742–1811), first Viscount Melville, and Francis Scott (1695–1751), second Duke of Buccleuch. Henry Dundas was the first Secretary of State for War and in 1806, was impeached for misappropriation of public money. Using evidence of his perfidy Lawrence Dundas refused to finance the Duke of Buccleuch when his business failed but then went on to do exactly the same. Smith was responding to this political chicanery, as well as the economic uncertainty at the time. Edinburgh was also alive with discussions of reviving Athenian democracy, ridding themselves of a corrupt monarchy, in light of the madness of King George III and subsequently against the backdrop of the American Revolution.

Cummings et *al.*'s narrative simply ignores this context. Moreover, Cummings and colleagues fail to explore in depth Smith's views on efficiency of slave labor (see further Bowden, 2018: 6-9 and 77-78). Instead, we are told Smith was an abolitionist (which he was not), early day environmentalist (which is doubtful), while treated to superficial generalizations.

Another concern around Cummings *et al.*'s conspicuously impoverished narrative and willingness to make ungrounded assertations is that it lacks a proper historical perspective to the early stages of capitalism and the market economy. Here it is worth considering the work of two-time Pulitzer prize winner Antony Lewis (2014), who sheds light on the feudal working conditions in Scotland during Smith's life. Specifically, the process and period that Marx calls "primitive accumulation" (see further Wood, 2002 or Perelman, 2000 & 2001). Colliers (miners), for instance, were emancipated (from serfdom) between 1775 and 1799. This suggests that the liberalization of labor markets described by Smith should be seen in the context of slave versus free labor, and freeing workers from patronage and feudalism; rather than Cummings *et al.*'s narrative of sustainability and conservation.

In chapter three, Cummings *et al.*'s claim that the advent of liberalism offered a new form of governance where salaried managers were required as a new form of control that replaces the authoritarian ruler. This is interesting and can help to place the emergence of managers in a wider debate around totalitarianism and democracy (see further Fukuyama, 2012) as well as different forms of control in capitalism and planned economies (which could help to better understand and contextualize contributions and contributors of the mid-twentieth century such as Alfred D. Chandler, Jr.). But it is unclear whether we are to believe that managers are the reincarnation of feudal despots. This as Cummings *et al.* sidestep a detailed explanation of the role of managers in capitalism, the rise of financial accounting (as a form of external control), management accounting (as a form of internal control), and accountants (as a group that effectively influenced the design and timing of rules and regulations); while offering no comment to the fact that today we continue to observe authoritative and authoritarian managers and management practices that co-exist with other leadership styles and practices within an organization and across organizations and national boundaries. Indeed, the casual observer would point to practices in Africa, Asia and Latin America where managers behave as if their area of responsibility is a fiefdom and the treatment of employees as if they were serfs. Cummings and colleagues have no answer to this.

Chapter three also puts forward the idea that greater efficiency was a central concern only after World War II, when management sciences were born. This argument is often repeated across the book. Yet there is no explanation as to why and how these phenomena took place when it did and where it did. It is the latter which can offer a solid base to understand the rise of management, managers and managerialism, as opposed to the rather superficial argument by Cummings and chums that the likes of Pollard (1965), Wren (1972) or even Witzel (2011) are an exercise to give gravitas to discourse in management textbooks.

In chapter four, a re-print of Cummings and Bridgman (2011), authors explore and re-evaluate the contributions of Max Webber to the study of organization. As in the previous discussion around Smith, the point that management textbooks have oversimplified Webber's work to the point of distortion is well taken. But again, the lack of appropriate historiography is evident.

Chapter five looks at the institution of the business school and the emergence of the teaching case as a pedagogy. The point of departure for this chapter is that "... management textbook histories almost always make no mention of the development of business schools." (Cummings *et al.*, 2018: p. 149). This point here is well taken. My experience teaching undergraduate and graduate British students is that they are clueless as to the late twentieth century establishment and development of business schools in the UK (for an introduction see further Brech, 1999; Larson, 2009; Locke 1998; Whitley *et al.*, 1979 and 1981; Wilson, 1992).

The curious history of business schools could easily draw on their earliest incarnation in France at the end of the eighteenth century, when the notion of technocrats ruling society with the best interests of everyone in mind (rather than inherited right to rule) was truly radical. Instead, following their US-cantered view of the world, Cummings and colleagues offer a couple of pages worth of anecdotes (pp. 149-152) telling of developments in the anti-bellum period. Their focus is then a long critique of the institutionalization of the case method as a pedagogy, its relevance, and legitimacy within modern day business schools. Here there was a lost opportunity to discuss differences between "school of management" and the now pervasive "business school" – as between the late 1960s and the mid-1990s the former was envisioned as a strictly academic department while the latter as having strong ties to industry and executive education (distinction that has become blurred an immaterial at the time of writing).

Also lost was an opportunity to discuss how the content of teaching cases in business and management perpetuate a US-based vision of management and the priorities of US-based multinationals (Bátiz-Lazo, 2013). Cummings and colleagues' US-centric worldview not only lacks appropriate justification, repudiates the heterogeneity of management ideas and different forms of capitalism but also ideas, practices and management priorities emerging from Europe and Japan. Cummings *et al.* thus exclude any consideration as to how US-style of management was "tropicalized" elsewhere around the world.

A New History of Management is also blind the rise in the pecking order of firms such as Mexico's Cemex, India's Tata Group or Spain's Santander and BBVA. All of these as notable examples of multinationals originating in countries which up to the late 1980s were net recipients of foreign direct investment (e.g. Bátiz-Lazo, Blanco Mendialdua and Urionabarrenetxea Zabalandikoetxea, 2007). From the perspective of Cummings *et al.* one would assume that the rise of these companies is thanks to the proverbial might of US-style management and management education.

To be fair there is a widespread bias within business scholars to US management styles and education. This is sad and neglects the possibility of indigenous management styles and the literature around varieties of capitalism (e.g. Hall and Soskice, 2001; Kase *et al.*, 2011; or Locke, 1996). There us thus an unmet challenge for scholar of management history to better understand the emergence of native ideas in Africa, Asia and Latin America throughout the twentieth century, as well as the absorption of Western ideas and practices in China and eastern Europe in the early twenty-first century.

Chapter six offers valuable details on the context of the Hawthorne plant before and after the arrival of Elton Mayo. It aims to minimize the contribution Mayo's studies and highlight the known bias in his results. Cummings *et al.* present evidence to argue that before Mayo arrived Hawthorne was a model plant, run by group of paternalistic capitalists who were aiming to forestall the formation of a union. This mixed with the tight knit Polish community around the plant, and later on, the impact of the depression. All this context, Cummings *et al.* argue, was important not only because it was ignored in the report of the studies but also to understand the results Mayo and subsequent groups got when they did.

Chapter seven reignites the attack on management textbooks and how "current views of management may be limited by the way textbooks arrange 'seminal ideas' into the conventional historical narrative." (p. 277)– a theme that is repeated often enough by Cummings *et al.* to make it hollow. Here authors attempt a re-examination of Kurt Lewin's "changing as three steps" and Abraham Maslow's "pyramid of needs". Notably absent are all but one of the contributions of Bill Cooke, a scholar of management of change and more relevant, someone who has documented in detail the work of Kurt Lewin.

Although their selection of textbooks is debatable, this chapter does show the usefulness of their selected method to trace the transformation and distortion of key concepts. This as Cummings *et al.* show how key concepts are oversimplified to the point of distortion through different editions of the same textbook. This is not only quite positive but could be useful to work constructively with colleagues elsewhere in business and management to better inform both scholars and students as to the genesis of management ideas. Explaining why and how such distortion came about could be an interesting exercise in an on itself. This provided that said transformation can be documented as resulting from, say, the evolution of the research agenda of a particular peer-reviewed outlet or subdiscipline.

Chapter eight uses the term "business history" indistinctively to describe the systematic study of managers, business organizations and industries with corporate and celebratory studies, as well as the fact that most of the former are produced by academic research and the latter commissioned by the firms themselves. References on this chapter have not been updated since the text was originally published in the early 1990s, let alone keep abreast with discussions in the broader business and economic history literature on the nature and scope of the field and its method (for instance, de Jong *et al.*, 2015; Jones and Friedman, 2011; O'Sullivan and Graham, 2010; Popp, 2009; Wilson and Toms, 2011).

Chapter nine makes a case to disregard "thinking about management's past because it makes us feel good about our heritage … and [instead] encourage questioning the present state of management." (p. 311). Authors then recap on the arguments and themes presented throughout the book but, as I have argued before, offer little in terms of a way forward. Chapter ten is entitled "conclusion" but is not more than a three-page summary of the book.

Discussion and Conclusions

To summarize, if this book opens the thoughts and interest of others in the history of management, it would be a positive force. Another positive aspect is that Cummings *et al.* have a valid point that there is a need to revisit management history in textbooks. Their claim that the first texts in management history appeared in the early 1970s (or too close to the formation of the field in the decades that followed the end of World War II), is timely and should provoke the revision and questioning of the straight-line, incremental narrative that stretches an arc from Sun Tzu and Adam Smith to the writings of Henry Fayol, Frederick Taylor, Mary Parker Follet and Elton Mayo, to present day scholarship as well as reassessing the contributions of industrialists, management consultants and management in the public sector. But if we are to produce a "new history of management" then we ought to include other neglected constituencies (such as women, LGBTQ+ community, ethnic minorities, different forms of socio-economic racism, or management and managerialism in recently-industrialized countries) and topics (such as ethics, the formation of standards, globalization or as the authors suggest, sustainability).

This text does little to help us better understand how the success of multidisciplinary research initiatives during World War II (such as that in Bletchley Park or leading to the formation of the operations research group), led to the creation of academic disciplines, their monopolization by academic cartels, and the intensification of a "bunker mentality" in the generation of academic knowledge.

This book is not the work one would expect of one of the top academic publishers nor of four, well-established, senior academics in universities of international reputation - one of which makes the humble claim to have "authored the landmark article" on the subject. Authors are obsessed with the Harvard-strawman and attacking the textbooks by Daniel Wren (1972) – while ignoring that it has gone to a seventh edition which includes substantial changes to reflect international developments (see Wren and Bedeian, 2018). This at the expense of theoretical elegance, new evidence, appropriate historiography, and providing a real alternative to established textbooks (such as that of Witzel, 2011). Most of this book adds little to the research it builds upon.

Their critique to the established textbook narrative, which seeks a unified perspective on the subject, is spot on. But like Donald J Trump's: "Nobody knows better than I about [fill in the blank]", the authors believe that their method and approach puts them above everyone else - an audience who has been blind to the truth before their benevolent enlightenment. Yet Cummings and colleagues' approach is no substitute as they offer a fragmented, incomplete, and narrow view on the history of management, managers, and management thought; which builds on fragile empirical evidence and partial understanding of current debates.

Cummings and colleagues raise some valid and timely questions. But they fail to provide answers. For instance, and as mentioned above, they question underlying assumptions as to the roots of ideas of efficiency and division of labor. Yet they do not account as to why they become important and even dominant from the mid-twentieth century onwards. We are no clearer as to why management ideas are so dependent on economics and psychology, why there is a preoccupation with scarcity, or why there has been a drive to prize quantification over understanding and impact. Obviously, there is no reference to bigger questions such as what legitimizes the role and power of managers or why business schools and business academics typically offer unchallenging and capitalist-servient teachings.

Cummings *et al.* rather naïve and poor excuse is that they are only trying to incentivize others into new and fruitful enquiries and perhaps even to persuade others to act differently too. In this way Cummings *et al.* played into the arms of one of the most common short-coming of "critical management" research, namely leaving a conceptual and empirical void after an elaborated critique. To my mind, constructive criticism is more than telling people what they have done wrong but indicating clear pathways for them to do better. I found naught of the latter in this book. Moreover, the narrative of Cummings and colleagues is limited to explaining and considering developments around organizational behavior and management of change, ignoring developments elsewhere in business and management while boxing themselves within the disciplinary straight jacket they sought to condemn.

In short, it will be great if Cummings *et al.* and their *A New History of Management* stimulate interest in the subject among curious scholars and even a wider audience who wonder "what is new" and are pulled in to examining the book. However, this book is poised to do more harm than good – very much along the lines of tabloid charlatanerie linking vaccination to autism. As far as serious scholars and their students are concerned, unless you crave doses of theorizing and jargonizing scarcely related to reality or proper historiographical debates, you would be better served reading the articles from the authors listed below first hand and saving your pennies for a better alternative. As for the readers of this journal are concerned, it is timely we embrace the issues raised by Cummings *et al.* while addressing myths that have developed around the genesis of management ideas. But it is also about time we stop further disruption and misinformation by addressing with robust arguments and firm empirical evidence the misconceptions, hyperbole, fallacies, and general charlatanerie of the "Historic Turn" .

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