**Phenomenal differences: varieties of historical interpretation in management and organization studies**

I am grateful to Albert Mills, Jean Helms Mills and the rest of the editorial team at QROM for giving me the opportunity to write this article in response to Bradley Bowden's piece, titled "Empiricism, and modern postmodernism: a critique" (Bowden, 2019). Though I am writing in response, I am not a postmodernist, and any defence here of postmodernism will be qualified. The overarching argument I want to advance is that hostility to differences of approach in historical research in management and organization Studies (MOS) is a result of hermeneutical and, to some extent, aesthetic preferences. These differences are, I argue, *phenomenal* in nature; that is, they refer to the phenomena about which the different approaches wish to generate knowledge. ‘Phenomenal’ is also sometimes used as a synonym for ‘extraordinary’ or ‘exceptional’, and so the title of this article also refers to the apparent Sturm und Drang of academic discourse–but which often mistakes light for heat. My argument is that we should be more appreciative and understanding of these phenomenal differences, and at the same time less concerned by them. What matters is to ask important questions, and to see the richness and variety of possible (and different) answers to those questions.

Bowden’s recent contributions (Bowden 2018; 2019) provoke important questions that challenge the nature of the historic-turn in MOS, and the related methodological impact that the historic-turn has had on cognate subject areas: particularly the field of management history, and its close relative, business history. Bowden’s critique of the historic-turn s that it is based on an underlying postmodernist paradigm, and that there is little or no benefit to be had of historical knowledge generated in this way. However, Bowden does not engage in a defence of historical research methods in response. Rather his position is a counterattack *against* postmodernism. Inadvertently this reproduces and concretises antagonistic differences between different sub-communities within a broader research community, and is likely to decrease the levels of dialogue in what already a long-lasting debate about which methods are best for undertaking historical research with reference to business, management, and organization.

Bowden's (2019) article in QROM has to be set in the context of–and alongside–his recent book, *Work, Wealth and Postmodernism: the intellectual conflict at the heart of business endeavour* (Bowden, 2018). This book not a straightforward text–it needs to be read in multiple ways. First it is a history of postmodernism and its influence on MOS. This in itself is a substantial endeavour and achievement. Notwithstanding the critique developed in this article, Bowden’s book should be regarded as a scholarly and ambitious text that reveals much about the intellectual currents that run deep beneath the surface noise and crescendo of everyday academic practice. At its best it reveals how such currents, in subtle and often unheralded ways, shape practice at the intersection of MOS and business/management history. Second, the book is a broadside against postmodernism itself. And third, the text is a polemic for a particular vision of capitalist modernity. On the one hand it lionises modernity and its achievements; and on the other–contradictorily–is a declinist text, exhibiting both nostalgia and elegy for a particular form of modernity that has vanished. In so doing it situates postmodernism as both responsible for, and a symptom of, decline­–which is also seen contemporarily through the post-industrial (and essentially postmodern) economy, and forms of employment that do not map to Bowden's aesthetic and to some extent ethical preferences for those which provide a "vendible commodity" (Bowden, 2018: 98-99; 284-285). He associates this with a turn away from Enlightenment values.

Despite, or perhaps because of, Bowden's commitment to modernity and a particular vision of the Enlightenment, he ultimately reaches pessimistic conclusions similar to those espoused by intellectual neoconservatives who see postmodernism as both source and symptom of wider cultural, political, and economic decline (for an outline of this intellectual heritage, see Peters, 2008). As I argue here, this is problematical because–far from seeing Bowden’s book/article as championing mainstream approaches to historical research (in business and management history, especially) it instead undermines such approaches. It does so in three ways. First, it develops false antagonisms that in turn are likely to feed negative critiques and ‘straw-man’ understandings of historical research for being unreflexive and only, merely, empirical. Second, with its polemical mode it advocates for a particular vision of modernity and its desirability, with which many historians and social scientists (and historical social scientists) might quite reasonably disagree. And third, it limits the possibilities for debate and dialogue between sub-communities by rejecting outright the methodological, ontological, epistemological, axiological, and hermeneutical choices of substantial sections of those engaged in the historic-turn in MOS and its cognate areas.

The article is structured as follows. The first section is an extended critical exploration of Bowden's (2018) book. This establishes that Bowden's article (2019) in QROM is part of a sustained intellectual project opposed to postmodernism. I examine (some of) the central arguments of the book, and critically evaluate the argumentation that underpins those points. The second and third sections then turn to Bowden's (2019) article in QROM. These sections focus on postmodernism and historiography, and Hayden White’s influence as a historiographer. The fourth concluding section then discusses the "phenomenal differences" that I argue inhibit academic dialogue between the different academic communities that are interested in the history of business, management, and economy.

*1. Work, Wealth and Postmodernism: the intellectual conflict at the heart of business endeavour*

Bowden's (2018) book of the title above is a substantial work that traces the origins of postmodern thought and its influence on Management and Organization Studies, and the treatment therein of the history of management thought and management practice (Bowden, 2018). It is one of a number of recent books which have addressed this subject, notably those by Cummings et al. (Cummings *et al.*, 2017) and Hanlon (Hanlon, 2016) (for my thoughts on these two books, see Mollan, 2019).

Bowden's book is divided into three parts ("Intellectual Heritage"; "Postmodernism"; and "Sociology and Reflections"). In the first of these, consisting of three chapters, Bowden frames the debate at the heart of the book, by examining the intertwined intellectual and material(ist) histories of modernity, as he sees it. In the first chapter, Bowden identifies modernity as the economic and intellectual change that stemmed from the Enlightenment, while noting that from the outset, the nature (and desirability) of this change has been both questioned and subjected to critique. For Bowden, material (or economic) modernity is synonymous with the industrial revolution and the emergence of market capitalism and its commercial activities. He writes, '[t]he transformative success of Western industrialisation and modernization–which has seen humanity rescued from the "poor, nasty, brutish and short" existence that Hobbes identified as the historic norm–always rested on ideas as much as technology, economics, or social and political organization' (Bowden, 2018, 62-63). This modernity is ‘associated with a market economy characterised by respect for property, individual rights, and a free labour force’ and was, in the nineteenth and twentieth centuries, ‘clearly economically and socially superior to other forms of social organization. (Bowden, 2018, 104)’ In chapter 2 Bowden identifies what is, for his argument, a key antagonism between empiricists and idealists. The concern of the empiricists was with material progress and human experience, so favouring modernity; the concern of the idealists was with 'matters of the spirit', which led (via Rousseau and then Nietzsche) to rejection of modernity. In time this second intellectual tradition was to spawn post-modernism.

The third chapter then traces the development of economic thought, and the sceptical critiques that have been developed by postmodernists of economic narratives of material progress, economic development, and rational intellectuality. Bowden's axiological position emerges from this discussion as personally committed to a broadly positive (but not wholly uncritical) vision of capitalist modernity. 'My prosperity', he writes, 'rests first and foremost, on the supply of material goods, electricity, drinking water, and associated infrastructure (i.e., roads, walkways, railroads, hospitals, schools, etc. (Bowden, 2018, 99)’ Bowden is clear that the decline in employment rates in the developed world among the working age population is a concern from an economistic point of view. Bowden's conclusions are supported by the use of statistics which are generated by positivist techniques of measurement, and draw on economic history. Though Bowden is sceptical of the public policy conclusions that owe to the analysis of Thomas Piketty and similar authors [=(Bowden, 2018, 65), he reveals a preference for debates founded on empirical evidence, which nonetheless commit to broad notions of seeking an improvement in material conditions and reducing poverty and inequality. Part of Bowden's argument –with which I have sympathy–is that postmodernist inspired discourses draw on methods and use sources that cannot always (and more often than not do not) capture macro-social and macro-economic phenomena, or, indeed, problematize *those* phenomena adequately. But, as I discuss below, qualified support for capitalist modernity and macro-economic level analysis and exploration, might also have very little to tell us about the collective or individual experience at meso or micro levels, or be able to tackle the issues that postmodern discourses can and do problematize. Here again is a phenomenal difference. For Bowden, much of his concern is essentially economic, concerned with outcomes of the economic structures, rather than the *individual* experience of management or organization.

Chapter 4 of Bowden’s book then turns to management history. The chapter begins by exploring the connection between management knowledge and the rise of an industrial economy. It then moves to discuss management-labor relations, and the relationship between management and markets. Here again the conceptualisation is chiefly economic and industrial in nature. The focus is on management as an economic and organizational process, and to some extent as a profession.

It is in this chapter that Bowden begins to outline the challenges for management researchers (as he sees it), in a context of remaining positive about modernity:

If modernity is to be defended and advanced, this defence must necessarily involve a review of the management principles that have got us this far; a review that must brutally discard old and irrelevant maxims (Bowden, 2018, 104).

He further notes that

….in defending modernity, however, one will do a poor job of the task if one denies fundamental truths that the paths of modernity have often been troubled; that capitalism has at times willingly availed itself of unfree labour; that no single model–be it based on classical economics, Keynesianism, post-1970s neoliberalism–has operated without failings (Bowden, 2018, 135).

The failings which Bowden notes are–perhaps surprisingly–similar to the failings that Cummings et al, discuss in their recent book which adopted a Foucauldian discourse approach. In contrast Bowden asserts that from ‘the main intellectual traditions of the Enlightenment–reasoned and logical scrutiny can lead to solutions for current and future woes’, going on to state that ‘this is not something that postmodernists can ascribe, drawing as they do on different traditions of Enlightenment thought (Bowden, 2019, 135).’ Bowden’s argumentation here turns on what he considers to be the epistemological difference between empiricists and idealists/postmodernists. Postmodernists believe ‘that knowledge is highly individualised, tied to individual consciousness and feeling’ and so ‘matters involving economics and a society’s material advancement must always be–at best–a secondary concern (Bowden, 2018, 135).’ Empiricists underlying concerns are, for Bowden, with material advancement.

This argument depends upon an agreed-upon understanding of what material advancement constitutes, and whether it is desirable–which are both ethical and aesthetic judgments, as much as qualitative or quantitative ones. Material advancement is contested. Opening up a new oilfield, for example, might constitute material advancement by ensuring a continuing supply of cheap fossil fuel energy, but might also hinder material advancement by retarding the development of market demand for renewable energy alternatives, while also contributing to catastrophic climate change. The appropriation of territory and resources, the displacement of people that might involve, and the destruction of ways of life, and being–with all the human costs (material, cultural, and spiritual) that this would entail–might also be to one sense of material progress, but also to the more general degradation of society and planet. For example, such acts of the exertion of power, of course, are replete within acts of imperialism, the studies of which can take multiple methodological and theoretical modes, all of which might come to a similar conclusion that it was undesirable (see Mollan, 2018 and articles in the attendant Special Issue of *Management and Organization History* for examples). Similarly, just because postmodernists might not share Bowden’s preference for truth claims and methodology does not necessarily mean that material advancement might be a secondary concern: rather, they might simply disagree as to what constitutes material advancement and the extent to which it is desirable, either in part or at all.

The next section of the book focuses on postmodernism as an intellectual and academic practice, its origins, as well as its critics. Here Bowden displays his wide and deep knowledge of Postmodernist writing. In these chapters (5&6) Bowden argues for the post-modern character of the historic-turn in MOS and criticises the use of postmodern ideas within that broad field of research. The issue here is that his argumentation is sometimes questionable, which in turns calls into doubt the validity of the conclusions that are subsequently drawn.

His first target in this respect is to criticise Cummings et al. (2017) for not applying Foucault’s ideas correctly in their recent book. For Bowden, any Foucauldian approach is incompatible with any act of interpretation or causal inference, writing that this is ‘methodologically impossible … The reason for this, as Foucault explained in *The Archaeology of Knowledge* is that Foucault had “no great liking for interpretation” (Bowden, 2018, 140).’ This rigidity of interpretation of Foucault is evident throughout these chapters. What is lost through this is that–at best, perhaps– postmodern ideas are useful as a means of problematizing and destabilising existing orthodoxies. How they are applied in academic practice seems to me to be open to–no pun intended–interpretation, unless one assumes that Foucault’s words are beyond exegesis. Second, and perhaps more importantly, the approach by Cummings et al. is–by their own account–based on Foucault’s *genealogical* approach, which does expressly seek to examine the processes that produce knowledge, in relation to power.

Foucault’s writing was complex and sometimes ambiguous–perhaps one of its strengths, in a way; and it shifted over time. His archaeology is not quite the same thing as his genealogy. Foucault described genealogy as ‘grey, meticulous, and patiently documentary. It operates on a field of entangled and confused parchments, on documents that have been scratched over and recopied many times’. It ‘requires patience and knowledge of details, and it depends on a vast accumulation of source material’ and ‘relentless erudition’ (Foucault, 1984, 76-77). The technique for Foucault, even, was difficult. He wrote of his early attempts at genealogical history that it was ‘tangled up into an indecipherable, disorganized muddle. In a nutshell, it is inconclusive (Foucault, 1980, 78).’ Contrary to the idealism that Bowden senses in postmodernism more generally, Foucault is clear that this method is, at least in part, quotidian in nature, writing that ‘if the genealogist refuses to extend his faith in metaphysics, if he listens to history, he finds that there is "something altogether different" behind things: not a timeless and essential secret, but the secret that they have no essence or that their essence was fabricated in a piecemeal fashion from alien forms. (Foucault, 1984, 78)’ The example that Foucault uses is that of the concept of liberty which he claims was invented by the ruling classes. The focus is therefore with the sources of institutional power that enable the creation of bodies of knowledge and ideologies that then shape actions. The treatment of management thought as one such body, in the manner of Cummings et al, seems to me to be an application that is reasonable to make using a genealogical approach. Hanlon managed to approach his similar topic through the use of Labor Process Theory and what can be described as form of intellectual history. Each has employed a different method and approach to cover similar ground and reach both similar and different conclusions (Cummings et al. 2017; Hanlon, 2016).

Chapter 7 of Bowden’s book, then develops the essential critique of the historic-turn that is also present in the article in QROM . This chapter begins by noting the prominence of certain authors who have contributed to the MOS historic-turn as institutional gatekeepers, and then begins to trace how the historic-turn occurred, under the influence of postmodernism (Bowden, 2018, 203-204). Bowden argues that within the historic-turn there is opacity whereby ‘overt references to postmodernism … are eschewed, even as postmodernist concepts and methodologies are advanced.’ (Bowden, 2018, 213) For Bowden, this is especially the case with the work of Hayden White, which I discuss below.

Chapter 7 is relentlessly hostile to both postmodernism and the historic-turn in general. Historic-turn contributors are criticized for not acknowledging the post-modern roots of their investigations (Bowden, 2018, 214). When they do acknowledge their commitment to, or merely use of, postmodernist ideas, they further fail because they are unwilling ‘to carefully explain the theoretical roots of their intellectual positions’ (Bowden, 2018, 215). ‘What is conspicuous by its absence, Bowden writes, ‘is any sustained attempt by advocates of the Historic Turn to explain either their relationship with pre-1800 traditions of Western thought or their stance in relation to the very significant differences that exist *within* postmodern thought (Bowden, 2018, 216).’ The first part of this supposed unwillingness is a high bar to place. Very few practicing historians (or social scientists in general, for that matter) are either able or willing to discuss their practice in respect of Enlightenment philosophy. While Bowden clearly does undertake such an situative operation relating to the Enlightenment in *Work, Wealth, and Postmodernism*, he does not do this in his most recent previous book (Bowden, 2009), not–by the way–that this would have been expected. There is a further irony that the kind of methodological reflexivity that Bowden demands here is somewhat similar to a call for reflexivity in the practice of historical research that several authors in the historic-turn have made (notably Durepos, 2015; Rowlinson, Hassard, and Decker, 2014; Taylor, Bell, and Cooke, 2009).

Bowden's criticism to authors working in the broad oeuvre of the historic-turn leads him to see twisted hand of postmodernism even where it might seem to be less obvious. After taking to task the openly Foucauldian approach of Cummings at al, for being inexact in their historical understanding of British industrialization, he then turns to others who similarly show a 'willingness to make ungrounded assertions' that lead to 'alarmist hyperbole'. He states:

Among the postmodernists associated with the Historic Turn, such hyperbole is evident in Bernard Burnes and Bill Cooke's "Review Article" in the journal *Human Relations*. In arguing in favor of research that addresses "fundamental issues" and "the big questions", Burnes and Cooke–who happily acknowledge in their article their adherence to "postmodernism"–assert that urgency is indicated due to the fact that "[t]he world is running out of natural resources and food." Given the prestigious nature of the journal in which this assertion was made, and the rigorous review system that it would have presumably have undergone, I was tempted to take this comment seriously–as, no doubt others, have (Bowden, 2018, 218).

Bowden then rebuts this with reference to population and food production data. However, the article by Burnes and Cooke is *not* about natural resources and food; it is about the field of Organizational Development (OD). The specific comment to which Bowden refers is given in full below:

Yet there has never been a time when the big questions were more important. The world is running out of natural resources and food, climate change is threatening to destroy the planet, and organizations and in large and important sections of our economy seem to have decided that ethics, morality, and laws to not apply to them (Burnes and Cooke, 2012, p. 1416).

References are given in the text to support this statement. Bowden is probably literally correct that food is not running out (at least not yet), but the broad thrust of the comment above by Burnes and Cooke (which serves as a peroration in the conclusion to the article) is to enjoinder a research agenda that embraces a wide vista–and the case that natural resources are being depleted and climate change is a significant social, economic, and organizational challenge seems to me to be unarguable. However, *even if* Bowden was unarguably correct about food and natural resources not running out (noting that climate change was not mentioned by Bowden, which rather concedes the point), this would still not invalidate the rest of the article which is about something different, nor would it invalidate any underpinning commitment or use of postmodernist ideas in the article (if there were any).

However, the article by Burnes and Cooke is not an obvious example of postmodernist inspired research, being as it is a historical exploration of research in OD covering the period from the 1930s to the present, taking the form of a discursive literature review. After 11 pages, the article does refer to the postmodernist influence within OD from the 1980s, but the treatment is relatively brief and largely descriptive, opining that 'for postmodernists reality is socially constructed and for this reason they reject what they see as the modernist-rational approach to change by Lewin and the OD movement' (Burnes and Cooke, 2012, p.1406). Burnes and Cooke then note that though postmodernism (as well as complexity theory and process theory) have ‘proved very attractive to organization scholars, they have proved much less attractive to practitioners’, going on to observe that such approaches are seen as being ‘strong on analysis and weak on application; in essence they are approaches to understanding organizations and not approaches to changing organizations (Burnes and Cooke, 2012, p.1407).’ This is not a ringing endorsement for postmodernism. Burnes and Cooke ultimately conclude by expressly *rejecting* the postmodernist critique of OD. They argue that it (the critique) was that OD was 'about making organizations more effective, i.e., profitable'. Instead Burnes and Cooke stress the original intention of Kurt Lewin (operating with a *modernist*-rational methodology and episteme; i.e., he was not postmodernist) that OD was to help resolve social conflict, 'extending democracy, and defeating totalitarianism in order to build a better world', and that now, in the present, OD should ‘promote ethics and democracy in order to build a better, more sustainable, world’ (Burnes and Cooke, 2012, p.1417). I am not in a position to comment on the historical veracity of Burnes and Cooke's argument; but it is clear that Bowden has interpretatively inverted their position to suit his need to find the influence of postmodernism more widely.

Bowden then turns to what he describes as the 'Rhetorical School', the issue of memory studies and ANTi-History (Bowden, 2018, 220-234). In this section Bowden comes closest to achieving his intention of critiquing the use of postmodernism in historical research by pointing to its limits. He notes that the attempt by Durepos and Mills to delineate a form of historical research that bridges between empiricism and postmodernism (later described by Durepos (2015) as “amodern”, following the intellectual lead of Bruno Latour, on whose work on Actor-Network Theory (ANT) was an original inspiration) is “the most significant attempt to address the internal failings of postmodernist thought in management and organization studies” (Bowden, 2018, p.231). Bowden critiques the ANTi-History mode of investigation as follows:

Like a person astride a barbed wire fence, the attempt by Durepos and Mills to thread an *amodernist* route between modernism and postmodernism lands them in some uncomfortable positions. Despite their stated support for “empiricism”, Durepos and Mills still see knowledge as subjective, a social construct of the socio-political actors. This makes the understanding of how knowledge is constructed more important than the knowledge itself (Bowden, 2018, 233)

Notwithstanding the colorful barb, this is an acute assessment. At the heart of the ANTi-History approach is, indeed, the desire to understand how historical knowledge has been constructed, and, following that process, a means of researching and thinking through the veracity and analytical traction of the prior approach by undertaking historical research anew, on that same topic. What Bowden implies here (but does not particularly explicate) is that as such ANTi-History can only proceed from an *existing* text or corpus from which to work through using its methods: a far narrower starting point than that used by mainstream historians, postmodern cultural theorists, or, indeed MOS researchers who might wish to research something previously unresearched. In a sense, the deconstructive requirement pinpointed by Bowden, may identify some kind of conceptual limit for the ANTi-history approach, boxed in by the primary requirement to explore the subjective construction of knowledge, and so limiting the bounds of ‘the [historical] knowledge itself’ (Bowden, 2018, 233) that might otherwise demand attention.

Bowden also argues persuasively that memory should not be confused with issues of fact, in particular when dealing with the horrors of genocide. Here, Bowden reminds us that it is a misreading of Ricoeur to suggest the 'testimony of memory' and history are incompatible, or that divergences between the two represent epistemological or ontological alternatives that must be decided between. Attempting to understand how individuals and communities remember and forget events, people, places and so on is a different historical operation from acknowledging that those events, people, and places had a real existence. Commendably Bowden refers here to the Holocaust as an example of historical fact, about which the objective reality of the historical experience is irreducible (Bowden, 2018).

Bowden then moves to a defense of Ranke over objectivity in historical writing. The issue of objectivity, and what different researchers understand to be the meaning of a slippery concept, appears to me to be at the heart of considerable misunderstanding (quite probably on all sides) between the different sub-communities that make up the historic-turn in MOS, and business and management history. Ranke is usually understood to represent *the* expression of the possibility of a recoverable objective account of the past–something usually regarded by historians as a naïve impossibility. However, instead of conceding that Rankean objectivity has now been largely abandoned by professional historians, or arguing the opposite, that Rankean objectivity is desirable, Bowden instead argues that unsophisticated facticity (along the lines of naïve objective realism) was never Ranke's understanding of objectivity in history to begin with. Here I must tread lightly, because, I am not sufficiently versed in Ranke's writing to be able to comment on that interpretation. But from the point of view of argumentation, it aligns Bowden with historians–and postmodernists–who say that there are limits to the possibility of objectivity. Bowden even writes that Ranke 'believed that the structure of language impedes full understanding of objective reality' (Bowden, 2018, p.229), *almost* implying that Ranke was an (albeit unlikely) intellectual precursor of Derrida and post-structuralism. So when Bowden attacks historic-turn authors, and also historians (such as Mordhorst and Schwarzkopf, 2017) for quoting Ranke "out of context", the point that Bowden makes is that while they may perhaps be getting Ranke wrong, they remain correct about the basic point–that there are limits to objectivity. If Ranke, many professional historians, postmodernists, and Bowden all broadly agree on this, the debate they might seem to be having collapses.

The final section of Chapter 7 then turns to the influence that postmodernism has had on accounting studies, including accounting history–especially in respect of the impact of Foucauldian thought on that sub-discipline. Here Bowden's argument is essentially that the accounting journals have bifurcated into those with a critical/postmodernist orientation, and those that 'promote a highly technical and largely acritical research on financial and business practices'. 'It is hard to see', Bowden writes, 'how such an outcome assists either the accounting discipline or modernity's continued advance (Bowden, 2018, p.246].' But if both critical *and* uncritical/technical studies of accounting both have this effect, what is the alternative?[[1]](#footnote-1) We are not told.

The final section of the book (Chapters 8 and 9) then argues for the 'good news story', that the world is wealthier than ever before and global poverty has been reduced, tempered by worries about macro-economic issues such as household indebtedness, investment rates and capital investment patterns, and declining employment rates in advanced economies. 'Everywhere one looks, in short, one find evidence of multiple constraints on future wealth creation', Bowden writes [Bowden, 2018, p.271]. What emerges is that ultimately Bowden doesn't see a utilitarian or materialist purpose for postmodernism. For Bowden 'postmodernism is essentially mute when it comes to economics' (Bowden, 2018, p.261). It is 'not interested in collective social experiences and outcomes', except one: power (Bowden 2018, p.280). And despite his belief in modernity and his stated optimism for it, Bowden is also deeply pessimistic:

[W]hat concerns me most is the fact that so many are disengaged not only from debates about wealth creation, but also from any meaningful participation in sectors of the economy that produce material wealth. ... Among those [in the workforce], an ever increasing number are sectors where we can delude ourselves that concepts such as national productivity and market forces have no meaning .... Their spread within the modern world threatens to act like a wasting disease, paralysing the intellectual and economic vibrancy that has long been a hallmark of modernity [Bowden, 2018, p.287].

This is perhaps the most disturbing feature of Bowden's argument. For not only is postmodernism an intellectual ill, but it is also a product of, and perhaps also responsible for, the rise of jobs that do not produce a ‘vendible commodity’, in particular the public sector and service sector. If there is a causal mechanism to explain how this supposedly parasitical employment detracts from the economy it is (we must assume) that Bowden thinks that jobs in in the services and public sector do not ‘produce material wealth’–a highly questionable assumption to make given the non-materiality at the heart of wealth creation in many sectors of the contemporary economy (the digital economy and finance being two notable examples). Bowden's argument is that for the growing number of well paid and well educated professionals in the public sector and business and financial services (to say nothing of those on more modest incomes in those sectors) has led to less personal meaning for 'the old collectivist ethos that favored class solidarity, labor market regulation, and higher minimum wages' (Bowden, 2018, p.285). There is much to be debated here, but this argument reads like an elegy for a vanished world of industrial workplaces, which is rather at odds with the broader argument for the onward march of modernity.

Bowden makes some use of economic history in his book, but does not acknowledge the well-known sectoral transformation of economies over time–away from primary and secondary activities and towards tertiary activities, in particular service activities in both the public and private sector.[[2]](#footnote-2) Indeed, Bowden repeatedly identifies two features that concern him about advanced economies: that there is declining labor participation and in some cases–such as the hospitality sector–chronic low wages. Yet these are both outcomes of capitalist modernity which prioritizes highly skilled knowledge workers and capital intensive economic activities. He makes an argument about productivity without also acknowledging that one of the reasons that professionals command high salaries is that they are highly productive. Capital intensiveness in manufacturing has raised productivity dramatically–while also reducing the demand for industrial workers. Bowden's concerns about the decline in labor participation may be further compounded by the impact of technological changes that may reduce or eliminate the need for a whole swathes of both blue and white collar workers, in particular the impact of robotics and Artificial Intelligence. These impacts are a direct consequence of modernity and the operation of market capitalism. Notwithstanding this, Bowden's observations about the macro-economy have little to do with the historic-turn in MOS, and instead form a polemical stance which seeks to harness what is essentially a debate about theory and methodology in history and MOS to an argument against the economy of post-industrial capitalism.

There is then a tension Bowden's book between the munificence of capitalism and modernity, and a somewhat pessimistic view of it prospects for its future. Here Bowden draws on the (sometime) neoconservative academic, Francis Fukuyama. Bowden aligns his critique of the transformation of the advanced economies ('...that condemns increasing numbers to insecurity and needless want' Bowden, 2018, 258) with the work of Fukuyama, writing that a 'deep sense of malaise is also indicated in recent reflections by Fukuyama ... pessimistically Fukuyama now believes that the principles upon which modernity's success has been built–market economies, democracy, and legal systems that protect individual rights–are confronting existential threats (Bowden, 2018, 258).' Later, reflecting on Fukuyama's observation that all societies eventually decay, Bowden asks the question, 'is the social and economic order that emerged in Western Europe during the Industrial Revolution–and built around market economics, liberal democracy, and respect for individual rights–in its death throes? (Bowden, 2018, 285-6)'

Fukuyama's thought is complex, and over time his position has changed somewhat. The presence of neoconservative declinism in his considerable academic output is tempered by a sober analysis of the causes of political development and stability. That Bowden should draw on Fukuyama reflects these shared interests. In *The End of History and the Last Man*, Fukuyama is somewhat hostile to postmodernism, or at any rate relativism, as seen through the prism of Bloom’s *The Closing of the American Mind* (Fukuyama, 1992, 306-307; Bloom, 1987). In *Political Order and Political Decay* (and also in *The Origins of Political Order*), Fukuyama develops a historical interpretation about the importance–and fragility–of the institutions on which modern political and economic development has been based, which is echoed in Bowden’s own advocacy for the Enlightenment values and institutions, and an account of economic development that is founded on them (Fukuyama, 2011; 2014). Yet the question of the fragility, viability and the imminent decay of contemporary society is left frustratingly open by Bowden (2018). If our contemporary modernity (or postmodernity) is in decline, is that the fault of postmodernism, or deeper flaws in the political and economic systems that have created and sustained it?

Bowden’s book closes in an unusual way, that demonstrates the tension between optimism and pessimism. The final two pages of the book are devoted to discussing the 1967 equal pay act in Australia which forced employers in Australia to pay Aboriginal Australians the same as white Australians. Bowden argues that this ‘is now regarded as the worst employment relations disaster in Australian history (Bowden, 2018, p.314)’ because agricultural employers adopted new technologies that removed the need for agricultural workers and evicted the Aboriginal employees who had previously worked on the land. Bowden argues that ‘the Aboriginal population in the Australian interior is still living with the devastating effects of their eviction from gainful employment. Hard at work one day, dependent on welfare the next, the Aboriginal population in various camps and “Top End” towns slid into a social abyss that blights the Australian democracy. [Bowden, 2018, p.315]’

Skyring–a historian of this episode, upon whom Bowden draws–has questioned whether the pre-1967 pastoral economy in Australia ‘was ever a modern normal economy’, noting that it ‘had long been identified as feudal or based on slavery … it functioned only with subsidies that were huge and largely unacknowledged and, in later years, illicit. (Skyring, 2012, p. 166)’ She went on to conclude:

Without acknowledging the enormity of the value of Aboriginal people’s underpaid or unpaid wages and their stolen pension cheques, contemporary policy responses miss a central point. Aboriginal poverty and the economic dysfunction of many remote Aboriginal communities did not start with welfare dependency in 1969; they were created through dispossession of Aboriginal land and of the value of Aboriginal labor across generations (Skyring, 2012, p. 166)’.

Bowden acknowledges that Aboriginal Australians, along with other colonized peoples ‘suffered terribly from the advance of modernity’ [Bowden, 2018, p.313]. And, indeed, it is well known that slavery, indenture and forced labor has long been part of the history of capitalism, the outcomes of which were both historically and contemporarily terrible. But this leaves Bowden’s argument stranded. The form of colonial capitalist modernity that exploited Aboriginal Australians at the outset doesn’t map to Bowden’s sense of the free-exchange of free-labor as a hallmark of modernity itself; but the form of market capitalism and technological innovation that Bowden argues made higher wages for Aboriginal Australians in the pastoral economy supposedly uneconomical *does* fit Bowden’s sense of the shape of capitalist modernity. So, none of the destitution that Bowden describes has anything to do with postmodernism, and everything to do with colonial modernity in the first place, that was replaced by market capitalist modernity in the second; neither of which–as Bowden states–have benefitted Aboriginal Australians.

In conclusion, then, *Work, Wealth and Postmodernism* should be read in multiple ways. It seeks to establish the intellectual basis for a particular vision of modernity and at the same time criticizes postmodernism. Readers may judge for themselves whether this is consistently sustained, but above I point to a number of problems of argumentation in that respect. The book is also polemical, with a significant tension between positive hope for capitalist modernity, and also a pessimistic sense of where it might lead. Bowden ascribes to postmodernism–both the intellectual practice and also the largely post-industrial conditions of advanced contemporary economies–the blame for what has gone wrong with ‘work’ in particular. This then spills over into a methodological dislike (which is ethical and aesthetic, as much as anything else) for post-modern approaches to the study of the past. This, then, provides context for the article in QROM.

2. Postmodernism and historiography

Of the things that struck me about Bowden’s article, and also his book, was a feeling of déjà vu. For like it or not, Bowden retraces a terrain that has been fought over before. This is not itself a criticism of the craft or quality of the critique that Bowden develops, but is reminiscent of the historiographical debates of the 1980s and 1990s that raged as historians responded to the postmodernist critiques of historical research (see Iggers, 2005). Indeed, an observation that could be levied at multiple parties in the debates about the historic-turn in MOS is that they are, in different ways, all repeating debates that were had decades ago in the historiographical journals and elsewhere; and in the process, often talking past each other. The critiques developed by postmodern approaches to historiography have been absorbed and reflected on by professional historians, whether or not they are committed postmodernists or not. This is reflected in courses on historiography in both undergraduate and postgraduate history programmes, as well as in many of the standard historical methods textbooks on this issue (See, as examples: Jordanova, 2006; Berger, Feldner and Passmore, 2003; Marwick, 2001). As Jorma Kalela (a postmodernist historiographer) writes, ‘scholars in fields closely aligned to the study of history tend to harbour anachronistic ideas of the nature of the historian’s work–in spite of the likelihood that the paradigmatic change within the discipline of history has a counterpart in their fields too’ (Kalela, 2012:6). Quite so.

Following this, there seems here to be a misunderstanding of the impact of the literary critique of history (following White and others), which was to wither away the last vestiges of Rankean objectivism, leading Evans to be able to observe (in 1997, over two decades ago) that ‘few historians would now defend the hard-line concept of historical objectivity’ (Evans, 1997: 3). Even *before* that impact Marwick (writing in the early 1970s) was to argue that historians should, in their historical accounts, be as true "as possible" (Marwick 1970, p.21) , the qualifier being of critical importance. The limits of historical research practice have long been known (see Evans’ chapter “Objectivity and its Limits.” Evans, 1997, 224-253). A blunt and undifferentiated, perhaps even oversimplified, version of the postmodernist notion of the *impossibility* of objectivity does not contradict that historical research should aspire to be as objective as possible, even while the conventional view is that this is unattainable in practice. The benefit to the practice of history of the literary turn/critique was to *enhance* (not abandon) the rigor with which historians approach their sources and methods. In turn, the ongoing commitment to rigor and reliability of historical methods has helped historians to challenge the pernicious and unethical misuse of history (the case of Irving-Lipstadt being the most famous, perhaps. It established that David Irving was, indeed, a Holocaust denier who deliberately misused sources and evidence to deceive) (Lipstadt, 2006).

Yet the subtle influence of the literary turn is often, or usually, unacknowledged. Instead, for example, Durepos wrote in 2015 that the "belief in the possibility of objectively and impartially reconstructing the truth of past reality as it happened" is "celebrated to varying degrees by positivist, scientific and *business historians*" [Durepos, 2015; my emphasis]. This, in its own way, is similarly problematical to Bowden’s attempted excoriation of the postmodernist influences on historical work, because both positions build into antagonisms simplified portrayals of professional research practices. These portraits are rendered much too definitively, and without enough understanding of the messy complexity and nuance of research as it is actually conducted, and the multiple modes of identity and practice than an individual researcher might adopt. Bowden’s defence of Rankean objectivity (however considered) is therefore problematical, because it perpetuates the myth that the practice of history is a claim to writing “objective history”. This portrait is common in the historic-turn in MOS where “realist history” (Vaara and Lamberg, 2016) and “history as fact” (Suddaby and Foster 2017) are presented as key types of historical research, and yet barely reflect historiographical developments over the last thirty years, to which business historians have not been immune–even while they have been largely silent on the issue.

This silence also may be source of tension. Where Bowden conjectures that "if intellectual maturity rests on the ability to defend the fundamental epistemological premises of their work" (Bowden, 2019, p.2), I disagree. Being able to defend the epistemological premises is not the same thing as actually defending them, or seeing a need to defend them (though for a defence, see Toms and Wilson, 2010). Much historical work takes place in relatively settled paradigms for research which do not require them to be re-assessed ab-initio over and over. Such re-assessment is done by methodological/theoretical specialists–and not every subject-specialist will also undertake this task. To be clear about this point: history is a broad research discipline, in terms of topics and techniques. Topic specialists will usually have a grounding in the methodological techniques of the discipline germane to their practice (variously: archive methods; palaeography; historiography; cliometrics, and so on), but might not develop expertise or interest enough to want to advance the *techniques* of historical research in historiographical journals–because their focus is the *topic* of research (say, the industrialisation of an economy, or the impact of the Cold War on management thought), not the tools used for the task. For those historians, the techniques are merely tools to undertake the primary task of history: to know more about the past, for whatever purpose–in the present–that the topic is deemed important. Venturing into the debates about historical methodological justification is a complex task that most historians leave to methodological specialists. And, indeed, it is a potentially fraught process. An example of this can be found in the 1990s where Arthur Marwick engaged Hayden White in the *Journal of Contemporary History* about postmodernism (Marwick, 1995). Marwick was against; needless to say, White was for. Marwick’s piece was rebutted by Hayden White in a brutal and caustic way (White, 1995). However, it was the subsequent rebuttals of Marwick’s contribution by fellow non-postmodernist historians that were telling (see Kansteiner, 1996; Roberts, 1996; Lloyd, 1996; Southgate, 1996). Marwick was late to the party and was stumbling into territory to which he was neophyte. Others had engaged in much more eloquent considerations of the postmodernist and post-structuralist critique of history. This, itself, was now more than a quarter of a century ago.

What is a little ironic about Bowden’s claim about the need to defend one’s methodological approach is that the kind of methodological reflexivity that he demonstrates and wants others to demonstrate is often also called for by postmodernists, who are–as Bowden demonstrates–interested in discourses, power-relationships, and the various intellectual and ideological commitments that individuals bring to their research practice. For postmodernists such as Kalela, for example, there should be such an operation:

The task is not only to set down the actions and thinkers of those people under investigation: the historian must also outline his [sic] own discourse in setting the parameters of the study [Kalela, 2012, p.15].’

Kalela describes this as "double detachment". This is similar to the view of Durepos (2015) (drawing on White) when she writes that in order to be reflexive historians should "acknowledge their chosen mode of emplotment as one of many alternatives." So, it should be noted that most historians *do not* do this explicitly. But then nor do most MOS scholars, or, indeed, researchers in many disciplines, including critically oriented ones. To illustrate this, take the article by Banerjee (2007) which appeared in the journal *Critical Sociology*. This article arguably constitutes part of the historic turn in MOS (it contains reflections on the historical origins of corporate social responsibility), and was published in an obviously critical sociological journal. Banerjee is a substantially cited author in the field of Organization Studies. The article itself is one of the most cited pieces to have appeared in that journal. At no time does Banerjee explicate his ‘own discourse in the setting of the study’ (pace Kalela), or discuss the ‘emplotment as one of many alternatives’ (pace Durepos). The proposed need for explicated personal discourse as part of the research process is a preference that reflects a phenomenal difference. In this case, that difference is a desire to know about the position of the author/researcher in the research process–as well as the object of research. Once again, this is a perfectly reasonable thing to want to understand, but it also should be recognised that not every genre of social science research–including MOS, and the historical variants of social science–requires such an operation. To discount research that does not require such an operation, or to insist on it as a pre-requisite, is to homogenise and limit what is possible on the basis of an axiological preference.

3. Hayden White’s influence

One of the mains charges that Bowden levies is that–within the historic-turn in MOS–Hayden White is lionised as *the* historiographer par excellence and usually referred to without adequate authorial criticism (see also Bowden, 2018: 188). Bowden notes that White is often presented a “leading philosopher of history”, without noting that he was a postmodernist, and, in addition, other important historiographers are omitted or ignored. Of course, Hayden White *is* (*was*) a leading philosopher of history, but this doesn’t detract from the essentially accurate observation that in *some* of the writing on the historic-turn, the postmodernist nature of White and others is not explicitly noted (some, but not all. See Durepos, 2015 as a counter example).

For Bowden the presence (and sometimes absent presence) of Hayden White seems to be evidence of an opaque and deliberately hidden commitment to postmodernism. Yet the very fact of citation of a well-known postmodernist historiographer is, in and of itself, an indicator of a kind, as it points to engagement with that writer. This does not necessarily make a historian (or social scientist) a “postmodernist”, but it probably does indicate that, at the least, they have reflected on the craft of historical research and writing in light of postmodernism. That is to be welcomed.

However, the issue of White’s pre-eminence causes interpretative difficulties, because his work is not universally accepted as the state of the art on historical method. Take this observation by Suddaby on a presentation given to a group of US business historians:

I spoke about my current research, which explores the various ways in which large multinational corporations use history strategically to market products, manage their brand, improve stakeholder affiliation, and enhance their human resource practices. I suggested, somewhat flippantly perhaps, that for the corporate historian, history was more than brute facts; it was a rhetorical resource used to construct meaning, identity, legitimacy, and authenticity in the corporation. History, I suggested, was a powerful and largely unexplored symbolic asset of the firm.

The reaction was not positive. I made the clearly naïve error of citing the American critical historian Hayden White’s 1975 book *Metahistory: The Historical Imagination in Nineteenth-Century Europe*. The crowd seethed. “That was not history,” I was told, “and Hayden White was not a ‘real’ historian” (Suddaby, 2016, 47).

There are three things about this account that are noteworthy. The first is that (most) business historians are not corporate historians, and most business history is not corporate history, if corporate history is that which seeks to explain that organization in its own terms, from a naïve realist perspective. The conflation of an imagined realist/objectivist corporate history with the research of most business historians is problematical and unhelpful because it does not reflect the field of business history. Any issue of the leading journals in the field of business history[[3]](#footnote-3) would give evidence of the range, diversity, and sophistication of business history research (see also Scranton and Fridenson, 2013).

The second is that though Hayden White is a historiographer, he *was* (as Bowden says) a postmodern historiographer. It seems reasonable that some business historians might have little identification with White’s approach for that reason and/or because the narratological approaches of White might have little utility to the types of research that they undertake. That doesn’t mean that that audience wasn’t expressing a reasonable academic position in disliking White’s work, or that he isn’t a “major philosopher of history”. Both things can be true at the same time.

And the third is that the kind of "uses of the past" research that Suddaby has developed through the use of what he describes as "rhetorical history", is not the kind of historical research as practiced by most professional academic historians. Notwithstanding differences of *method*, the “uses of the past” literature it is not primarily about the past itself (however conceived), but primarily about how an account of the past is *used* by organizations, or indeed, how such accounts *might* be used by organizations (Suddaby and Foster, 2017). These are perfectly reasonable research topics, of course, and might have analogies in studies of how organizations use images and art as a means of establishing a brand or identity (that might not be art-history or aesthetics as art-historians or aestheticians might understand their research fields), or a study of how organizations might use spatial modelling devices (maps, GIS) to perceive the organization and its operations geographically (but might not be engaged with geography as an academic discipline). And so on. But what is absent in much of the work on “rhetorical history” and “uses of the past” is much sense, or interest, in how social, economic, political, and cultural forces pervade over time. Or, put another way, those organizational theorists are not as interested in how historical forces shape economy and society, but are more narrowly confined around the idea of historical representation within an organized setting. That is, indeed, an area over which post-modernist inspired research on narratives (drawing on White, perhaps) might well be appropriate.

In this, then, Bowden makes a valid point when he calls into question the way in which temporality is conceptualised and integrated into the interface of the historic-turn MOS and historical research. Though this is something of a blunt generalisation (for the sake of argument), for historians it is events and structures in the flow of time that form the objects of research, and the accounts that are written thereof. For organization theorists interested in only (or mainly) representation, these events and structures (and the effects they have) may be of a much lesser importance than the account that is created, and how it is then used. For organizational theorists interested in using history as means of organizational theorising more generally, however, business, management, and organization history are fertile terrains to understand how organizing occurs in time and over time.

For Bowden, evidence on which theoretical or empirical generalisation might be made in that regard is rooted within his commitment to the positivist/empiricist tradition. There is nothing particularly to take issue with this, while noting the relative limits of that method of knowing. His opposition to postmodernism is on the basis that he dislikes the Whitean critique of the “fictive” construction of historical narrative, and see the dangers of relativism where “anything goes”. Yet there is a difference between an understanding of relativism on the one hand, and on the other claims of false moral equivalence or, worse yet, historical denialism. So a critical awareness of relativism is necessary for any researcher to *attempt* see beyond themselves and apprehend the multiple ways of knowing, the bounds of cultural, social and linguistic anchoring, and the limits of seeing the world only from one’s own perspective. This is not to decry “facts”, but is to appreciate that “facticity” is a process of the construction of the most basic research objects from texts and artefacts, into which the object text’s original author, the socio-cultural matrix from which they came, and were writing, and the context in which they were writing, all shape how a “fact” can be rendered by the historian, and how reasonable that rendering has been. Historians are taught to read sources with the grain, and against it; to look for witting and unwitting evidence; to engage in the most basic forms of source criticism (who, where, when, why, where, whom for, how). Creativity and imagination play a role in this interpretive process. One value, then, in White’s *Metahistory* is to allow the historian to hold a mirror to their practice; and to reflect, on personal biases, preferred aesthetics, and ethical axioms, and to reflect on how their narration proceeds. But this is not the only historiographical text that it would be useful to reflect on, and if there is a case to be made, it is that Evans (2001), Iggers (2001, 2005) , Jordanova (2006) , McCloskey (1998), Marwick (1970), Kalela (2012), Bloch (1992), Le Roy Ladurie (1979), McCullough (2002) and others should all be read as well.

A thorough grounding in historiography is essential to be able to differentiate between the object of historical research and the method used to explore that object. Differences will, of course, occur if the object of research is to understand historical forces, rather than the ways in which those forces are accounted for in historical narratives. Again, this reflects the differences of phenomenon to which the researcher is concerned. Reconciliation between phenomenal concerns may be essentially impossible­–and that is absolutely fine! But dismissal of the modes, methods and phenomena to which others spend valuable research time, and commit to writing, does not advance any understanding of history, or the role that history plays in shaping the present.

4. Closing remarks: phenomenal differences and mutual recognition

One of the more controversial aspects of Bowden’s article is the focus on a number of scholars interacting with the MOS-historic who are ‘regular contributors who constantly cite each other’s work’ (Bowden, 2019, ?). The first thing to say is that care must be taken not to ascribe unitary holism to a list of academic authors who intersect in their research interests. It is not surprising that a community of practice would cite each other’s work; that is how progress is made in any field. It would be incorrect, however, to suggest that the research output from any such group of an academic community would constitute a unified body of work, or, even, that in the messy world of academic research that identifies and modes of research do not shift even for an individual author between outputs and different projects. Furthermore, It is a puzzle that where Bowden sees these authors as being advocates for (or the unwitting vessels of) postmodernism, the journals[[4]](#footnote-4) edited by some of those individuals he names are not really postmodernist; if anything, they are influenced by a variation of institutional theory, the origins of which are in empirical economic and organizational research (see North, 1990; Powell and DiMaggio, 1983). It seems doubtful if such a large and diverse group could all be crypto-postmodernist while maintaining and developing academic reputations and research based on institutional theory, or otherwise untroubled by plural approaches to historical research (see Decker, Kipping and Wadhwani, 2015 as an example of a commitment to plurality). A better question may be to ask how postmodern thought is integrated into the empiricist and positivist origins of institutional theory as it is practiced in MOS/historic-turn, and whether the implications of such a combination have been adequately worked through.

The debate about the "historic-turn" in MOS seemed to focus–at least in its initial stages–around an intractable and sometimes impenetrable debate about methodological and philosophical validity and legitimacy. However, one feature of the proliferation of methodological explication around historical research in MOS is that a methodological scaffold–as a basis for research (see, especially, Rowlinson, Hassard, Decker, 2014)–has now been sufficiently erected to mean that the disputations about methodology might recede somewhat, it is to be hoped. This may depend on mutual understanding that that different sub-communities have different interests, objects of research, methods, units of analysis, hermeneutics, as well both ethical and aesthetic differences–*and yet*, may still be fundamentally connected by their mutual interest in the past, in history, and the ways in which history both shapes and is understood in the present. And, if we can accept and be accepting of the very notion of phenomenal differences in the first place, what should come next?

Bowden poses the question as to why postmodernism has enjoyed the success that it has (Bowden 2018, 141). His answer is that its success reflects its intellectual origins in the Enlightenment and a divergence between idealists and empiricists. Perhaps so. But it seems equally plausible that there is a pragmatic and more prosaic reason for postmodernism’s success–it is an approach which has been borne of our age, and speaks to it. And, equally, I think there is something to be said for the stimulating effects of postmodern inspired writing, the playfulness of the concepts, and their ability to provoke and stimulate meaningful and important academic questions. Postmodernists and positivists alike may find that the present socio-economic condition is unsatisfactory, and only then proceed along different routes of investigation. Bowden considers that “postmodernism is ill-suited to economic analysis, thereby marginalising postmodernism from the key debates of our time. (Bowden, 2018, 17)” But this demands–at the least–that we evaluate what the key questions of our time actually are, and–even–whether we must share with Bowden the underlying axiom that the chief issues of our time are best understood through the lens of economics, or indeed to question which version of “economics” and economic thought might yield the most fruitful results. These questions will always be debatable, of course, but of the main secular changes of our time with some connection to business, management, organization, or the economy (which include, for example, austerity; Brexit; climate change; economic (under) development; financial crises; inequality and discrimination in work, pay, and advancement based on gender, ethnicity, race, or sexuality; labour precarity; nascent protectionism; technological change; the rise of the emerging market economies–to name only a few) it seems credible that postmodernist concerns with power and its distribution, how knowledge is constructed and used, and how ideas shape social and economic relationships, are all potentially valuable avenues for research, or starting points for reflection and critique. This is not to decry economistic approaches at all; but it is to suggest that it is deeply unhelpful imagine that those with different phenomenal concerns, different methodologies, and different axiological preferences to oneself are beyond understanding (especially in *their* own terms), or whose work must–by the extent of its difference and divergence from one’s own–be of limited, or lesser, value.

It follows from this that there should be, instead, a willingness to engage communities of practice on their own terms, and to attempt to create a genuine dialogue that might shed light on old topics with new insights, or help formulate better questions and better answers to new problems. As Marwick wrote of historiographical controversy, ‘[i]t is out of the clash of different interpretations and conflicting hypotheses that a new, profounder, more rounded version of some historical problem finally emerges.’ (Marwick, 1970, p.214) It is perhaps time to call for a clash over argument and interpretation of history and the past, to debate and discuss the events, periods, people, organizations, structures and narratives of history, and of the past, that we consider (for whatever justifiable reason) to be of importance in the present, rather than to persist with a potentially endless digression as to the legitimacy, or lack thereof, of the multiple ways of obtaining historical knowledge.

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1. An interesting historical account of the divergence between positivist and critical accounting approaches is Baker (2011), who applies a Foucauldian genealogical technique. [↑](#footnote-ref-1)
2. The literature discussing and debating this is, as to be expected, vast. The work of noble prize winning economist/economic historian Simon Kuznets is especially important in establishing sectoral structural change as a characteristic of what he referred to as ‘modern economic growth’. See Kuznets (1966) and (1973). A review of Kuznets’s contribution to economic history is Easterlin (2001). Broadberry (2016) provides useful and recent overview of ongoing debates. [↑](#footnote-ref-2)
3. Especially *Business History*, *Business History Review, Enterprise and Society, Essays in Economic and Business History, Journal of Management History*, and *Management & Organization History*. [↑](#footnote-ref-3)
4. See *Academy of Management Review* (previously edited by Suddaby), Academy of Management Learning and Education (edited by Foster), or *Business History* (co edited by Decker and Wadhwani among others). [↑](#footnote-ref-4)