**Management and business history – a reflexive research agenda for the 2020s**

**Purpose:** The author reflects back over his career as a management and business historian so far to consider opportunities for the future of management and business history as a disciplinary area.

**Design/methodology/approach:** The paper consists of two segments – the first half is an auto-ethnographic personal reflection looking at the author’s research journey and how the discipline as experienced by the author has evolved over that time. The second half is a prescriptive look forward to consider how we should leverage our strengths as historians to progress our discipline forward.

**Findings:** The paper demonstrates opportunities for management and business history to encompass new agendas including the expansion of the topic into teaching, the possibility for the advancement of empirical contributions, and opportunities for findings in new research areas, including the global south and public and project management history.

**Originality:** The paper demonstrates that historians should be more confident in the disciplinary capabilities, particularly their understandings of historic context, continuity, change and chronologies when making empirical and theoretical contributions.

**Reflection**

I was intrigued to be approached to write this paper at a time when the very form and purpose of the academy has come into question like never before.[[1]](#endnote-1) At the time of writing the world seems in flux. Our universities have been blindsided by the Covid-19 pandemic, which will come to define historical understandings of the year 2020, and we have spent a lot of time thinking about what teaching will look like in the 2020-21 academic year and beyond. In this paper, however, I want to think about what our research in management and business history might look like in the future. I do this firstly by reflecting on my own experience in the subject area as a lens through which to examine its broader practice, and secondly by using this experience to reflexively think ahead about what we might want to do as a scholarly community to increase our impact in the coming decade. There has been a broader awareness of management and business history’s relevance in the last few years, and perhaps in particular since the financial crisis, but at the same time we still remain unsure of our legitimacy in the broader academy, an unsureness of footing that we need to be more assertive in tackling. Aside from Covid-19, the broader global crises and wicked problems of inequality, democracy, human rights and the environment (Camillus, 2008), many of which relate to the complex impacts of business and management activity, seem to be problems which few are willing to listen to the academy to try to solve. In some ways the academy has gradually deprived itself of legitimacy in cocooning and distancing itself from society through its self-legitimating and self-perpetuating systems, characterised by the building of disciplinary boundaries through the interlocking system of discipline specific conferences, journals, rankings and general DOI accumulation practices. Yet we as historians know that the world has long been characterised by crisis and what might be considered insoluble problems, and we have an opportunity to provide perspective, unsettle and challenge in a fashion that might be refreshing for the overall management studies discipline.

 Firstly, a brief methodological note. I offer first a reflection on my past experience. This account is auto or self-ethnographic (Alvesson, 1998; Hayano, 1979) in form, being heavily constructed from my own recollections together with those of colleagues – I did not do active historical research to reconstruct this, but rather draw on my own (limited) memory and perception. Where primary documentary sources are used, it has been to jog my memory. The second account is less personally narrated, but consists again of a discussion constructed from my own personal viewpoint of current practice, and offers prescriptions from my own perspective – agreement is left to the reader’s discretion.

*Management and Business History: The Struggle for Legitimacy*

 Management and Business History (MBH)[[2]](#endnote-2) always seems to have been struggling for legitimacy in some form, making entry into it an economically risky premise for any scholar. Leslie Hannah (2019) illustrates how MBH managed to evolve a semi-distinct existence for itself within the LSE’s Economic History department, in which I did my PhD between 2005 and 2009. Hannah locates the struggle as a product of the contested ground between ‘monks’ and ‘technocrats’ – or those who wanted to pursue pure economic (and other social science) theory versus whose who wanted economics to engage with the wider world. In this environment, such stratospheric figures as Coase and Penrose, who were interested in the practical and empirical application of theory had moved on from LSE, as government research funding for theory driven research had increased. Hannah locates the Business History Unit (BHU) that he served as Director of from 1978-1989, firmly within the technocratic tradition. The BHU, though part of LSE and, initially, Imperial College, was strictly self-funding, reliant on fundraising from business to survive, but 1980s London proved a fertile market for business history research of various directions, with early research by figures such as Geoffrey Jones (now Isidor Straus Professor of Business History at the Harvard Business School) and Jonathan Liebenau contributing research on such growth industries of the time as banking and pharmaceuticals respectively. Further, the BHU was a place of considerable institutional entrepreneurship, with the building of the MBH academic subject area ecosystem that happened under Hannah and his successor, Terry Gourvish. Hannah took over the editorship of the journal *Business History,* the Director of the Unit took a seat on the Business Archives Council (a charity which works to preserve the records of British business), and the BHU held predecessor conferences to those later held by the Association of Business Historians and the European Business History Association, both organizations that Jones had taken a leading part in founding. The self-funding nature of the BHU made it vulnerable as funding from business dried up in the 2000s, and a further blow was the change to the UK Research Funding landscape after the election of the Conservative-Liberal Democrat coalition in 2010 with its ‘austerity’ agenda, and so the BHU closed when Gourvish retired. Yet the entrepreneurial activity undertaken meant that a considerable international legacy in terms of the structures for MBH research was bequeathed.

 This provided an ideal environment for the fostering of my own PhD research agenda into Scottish foreign investment. Hannah’s intellectual contribution, for instance in his landmark work *The Rise of the Corporate Economy* (1976)*,* had been to develop comparative work that was based on a broader synthesis than the work done by Chandler, especially in *Strategy and Structure* (1962)*.* This style of MBH attempted to gain social science legitimacy by subverting narrative to the cause of thematic explanation, for instance exploring systematically the factors behind the rise of large firms by exploring the influence of common factors such as management structure, technology and productivity. This theoretical-comparative approach, later labelled ‘analytically structured history’ by Rowlinson, Hassard and Decker (2014), allowed for abstraction and the construction of theoretical explanations out of the messy webs of evidence left by past business activity. It was also somewhat broader in focus than the other main genre of MBH, the longitudinal and multi-volume ‘corporate history’ as identified by Rowlinson, Hassard and Decker, which focused on a single organization which had often funded the work, perhaps epitomised by the work of Coleman (Coleman, 1969a, 1969b, 1980) on Courtaulds, Charles Wilson (1968) on Unilever, John F. Wilson on Ferranti (2003, 2007, 2013) or Gourvish on British Rail (2008, 1986, 2004) (though it should be noted that this genre remains historically rigorous, as it encourages studies of admirable depth). More recent corporate histories (Durepos, Helms Mills, et al., 2008; Durepos, Mills, et al., 2008; M. Deal et al., 2018) have aimed to deconstruct and reconstruct this style somewhat. The analytically structured approach also allowed for an alternative mode of explanation permitting a dialogue with yet circumvention of those proponents of the ‘New Economic History’ who privileged the total abstraction of the past through statistical estimation, otherwise known as the Cliometric School. This school of thought privileges method over historical and theoretical explanation, and enjoyed hegemony over economic history research in the 2000s, but narrowed its focus to the very abstract. While it might be considered to have ‘theoretical fluency’ as Maclean, Harvey and Clegg (2016) would call it, it has come to be criticised even by its own creators for taking neither history or economics very seriously, ignoring historical context and theoretical integrity all at once (Fenoaltea, 2020). Most importantly for those like myself interested in studying the historical development of specific organizations, when I came to LSE hoping to focus on the motivations of late 19th century entrepreneurs and investors for pursuing foreign direct investment I found in the Economic History department outside of the BHU an antithesis to the study of the role of individuals and intra-firm dynamics. Though I quickly learned some basic quantitative methods, this was not a field of study that really interested me nor suited my aptitudes.

 The BHU seminars, which focused on papers from the analytical and corporate history approaches, provided me with an alternative vision of how to be a ‘legitimate’ scholar in the historical social science field. The vibrancy of this series was the fruit of the discipline building efforts of Hannah and Gourvish. Papers in the series were diverse in topic and method – just a glance at the 2006-07 academic year schedule reminds me of sessions involving Janette Rutterford and the late Josephine Maltby on women investors, Andrew Godley and Bridget Williams on the role of Sainsbury’s in the emergence of the chicken industry, and Sir Geoffrey Owen on the historical decline of the British Courtaulds conglomerate (LSE Business History Unit, 2007a). The BHU seminars were a gateway drug for me to the Business History Conference and the Association of Business Historians conferences, which proved to be more encouraging and fruitful outlets than those of the Economic History Society. The unit also hosted an intriguing one day conference in September 2007 on the business history of sport involving Dilwyn Porter, Tony Collins and Stefan Szymanski among others, which was key for my later career as it made me aware of the potential of sport history research (LSE Business History Unit, 2007b). But back in the 2006-09 period these sessions taught me how to structure MBH research and emboldened me to abandon my first approach at my thesis, which had been to look at the free-standing company phenomena in Scotland on a population level, and rather to focus on a case study approach. The work of Mira Wilkins (1986, 1988, 1998) on foreign direct investment together with the debates she had with her opponents (Casson, 1998; Chapman, 1998; Miller, 1998) provided a useful framework to situate my work on Scottish registered firms which had not just invested in but also actively managed and developed farming, property development, mining and logging businesses in Australasia and North America between the 1860s and early 1900s (Tennent, 2009).

 At this time my theoretical orientation remained common to that of the orthodox MBH school which had inspired my work, which continued to privilege (sometimes implicitly) economic determinism largely based around transaction costs assumptions of internalisation and principal-agent theory. This ‘economic history of business’ approach concerns itself heavily with the interplay of profitability, productivity and corporate structure, as well as sometimes considering the impact of technology and state intervention (an excellent example of this genre is Higgins and Toms (2011)). To some extent, the works of Chandler and Williamson were still considered to be the major comparative frame in terms of understanding the structure of business. This was how I cast the work on popular music, which I undertook at the BHU with Terry Gourvish and Richard Coopey. Finding ourselves unable to access the archives of the major record companies, we initially carried out research into market concentration in the industry using the pop charts as a proxy for market share, using the Herfindahl-Hirschman index to analyse concentration. We were able to provide a transatlantic comparison with the US by recalling the work of Peterson and Berger (1975; 1996) on the rise and fall of independent record companies in the US, showing that in the UK independents appeared later, and until the end of our sample period, seemed to erode the hold of the majors (Gourvish and Tennent, 2010). On the other hand, it was also clear from quantitative research using the industry press that the majors had only ceded partial power to the independent companies whose output they themselves distributed (Tennent, 2013a). This was a piece of work entirely oriented around understanding the historical interplay of business within the markets, set amongst the backdrop of the British post-war business environment which had began to interest me through teaching.

*Moving into the Management School*

 As much as MBH had managed to assert its independence from economic history, a broader interface with management studies started to emerge which threatened to see the discipline become subject to different hegemonies. Almost imperceptibly through the early 2010s the momentum expanded away from the economic history of business, partly through the inescapable influence of the historic turn in organization studies (even if this was only ironically meant in Clark and Rowlinson (2004)). I also started to become more aware of the potential of the allied fields of management and organizational history as distinct from business history, which tend towards an understanding of the functional concerns of business as well as the history of management theorisation rather than overall enterprise performance and strategy. This field saw rapid expansion in the UK especially after the establishment of the journal *Management and Organization History* in 2006. I also participated in a broader shift in the UK, which has seen business historians generally relocate themselves to the business school, somewhat stretching and challenging the ecosystem built by Hannah and Gourvish. This brought new opportunities including involvement in the British Academy of Management, the Management History Research Group, and later on the Management History Division at the Academy of Management. Also important for my own research, however, were opportunities which arose for cross-disciplinary conversations with sociologically oriented historians working in fields of study which interested me including sport and transport, as well as legal scholars working on corporate governance concerns, and broader conversations with strategic management and organization studies scholars. Perhaps the most exciting development was the opportunity that emerged for historians to publish in more mainstream management journals by engaging with theory (Maclean et al., 2016), and the additional plurality that came with the realisation that multiple business histories were possible (Decker et al., 2015). Bucheli and Wadhwani’s (2014) book, particularly the later chapters on methodology, also provided a range of intriguing possibilities for speaking to organizational research audiences.

 At the same time, some historians saw the historic turn and the proximate calls for history to be located within theoretical rather historiographical literatures as quite threatening. From the point of view of a then emerging scholar the call of Taylor, Bell and Cooke (2009) for historians to embrace enhanced reflexivity and visibility of method in an effort to reduce their isolation from the rest of management studies seemed a reasonable one, but it drew an angry rejoinder from the editors of *Business History* (Toms and Wilson, 2010)who explicitly rejected the possibility of relativism being valid in business history. This seemed to be followed by something of a crisis of confidence in the integrity of the subject area, with a perhaps misplaced call for more scientific approaches to the formation of a ‘new business history’, with increased application of economic theory, quantitative methods and hypothesis testing (de Jong et al., 2015). These responses by leading scholars seemed problematic to me, because earlier literatures in the field had already started to warn of the ‘Whiggish history’ problem inherent in abstractive work (Lamoreaux et al., 2004). Hannah (2009) had also warned of the difficulties intrinsic in comparative historical approaches. Historians surely are not a neutral conduit for archival information as Toms and Wilson seem to portray it, because historians, bounded by practical concerns such as limited archival opening hours and access arrangements clearly make active decisions on what to record in the archives and how to emplot these recordings into narrative or abstraction. As in the example of the music research referred to above, the sources used and the limitations of working with them (for instance exhaustive availability or scarcity) can shape what it is possible to find, and to some extent this requires the historian to pursue research strategies to optimise their time with the sources. While I do not fully agree with postmodernism’s critique of textuality it is clear to me as a reflexive practitioner that what historians write is shaped by the conscious and unconscious choices they make around source use, interpretation, and emplotment. Farge (2013) provides some reflections on the messy process of interpreting the overwhelming volume of historical sources using a government archive at the same time as realising that while we cannot fully know the context in which something was originally recorded, we also often do find ourselves emphasising with the people that the archives bring us into contact with. This very process seems unlikely to be one of dispassionate objective discovery and replication, therefore I would identify myself with more of a relativist position.

Classical MBH, in the sense of using archival research to investigate the economic or even social history of a business seemed to be in retreat to some extent. Around the same time, I became aware of the rise of the ‘uses of history’ literature which, while interesting in terms of the relevance of archives and historical evidence to practicioners, used somewhat static or ahistoricist conceptualisations of organizational histories that conjured notions of pasts that were somewhat simpler than might have been constructed if the history of the organization were itself the research question (Anteby and Molnàr, 2012; Brunninge, 2009; Suddaby et al., 2010). This blurred Marwick’s (2001) conceptually clear history/past dichotomy in a way that seemed, to me, difficult to process. If anything, ‘uses of heritage’ might have been a better title, for often the ‘history’ discussed was how received memories of the past or heritage were used rather than the conclusions reached by historians about the past, which by necessity is a very different entity. These blurrings of longstanding understandings and traditions, and the relative lack of discussion with them from the established MBH field seemed disorientating. On the other hand, later work in this literature, particularly that of William M Foster which stressed the use of ‘tradition led narratives’, seemed to reflect the history/heritage nuance more carefully, and unlocked the potential of this approach to me (Foster et al., 2011, 2017). Meanwhile, Lubinski (2018) brought historical context more carefully back into the discussion, using a carefully researched history to try to move the discussion beyond the pure organizational exploitation of the past.

For a while leadership and guidance seemed difficult to find, and publication of the sort of holistic MBH work that I wanted to do which meshed together the strategic, financial and organizational seemed to become more difficult (though I am sure some of this was due to my hubris as a young scholar underestimating how difficult it would be to get published).

*New Opportunities*

 However, these difficulties did not persist for long, as I was soon able to find new space and new confidence by engaging with a range of new influences, including the rhetorical history concept. The historic turn brought thought provoking and new ideas, and provided new models and opportunities for publication, in particular for me by opening up the possibilities for dialogue with institutional theory as well as with notions of historical awareness and consciousness. This meant there were new opportunities for research outputs, and indeed to ‘dual output’ research projects by publishing both ‘historical’ and ‘theoretical’ research. Critical to this was being willing to keep learning, including the crafting of papers for management audiences, and engaging with broader intellectual themes and theories. In the process of writing Tennent (2017), for instance, I learned that it was possible to propose a refination of the theory of Mees (2010) around public transport policy using empirical historical evidence. In my work on the 1966 FIFA World Cup with Alex Gillett, we wrote a historically oriented book about the preparations for the tournament (2016), but Gillett and Tennent (2017) used the same evidential base to propose the historicisation of Flybjerg’s (2012, 2014) project management theory. This conceptual idea, ‘dynamic sublimes’, has now been taken up in the study of sporting megaprojects (Andrew et al., 2020). In a similar vein my work with Andrew Smith and Jason Russell on the militarising and managerialising impact of Berle and Means (1932) opened up intriguing possibilities for discussions with legal and industrial relations scholars, allowing for dual publication in these fields (Russell et al., 2017; Smith et al., 2018, 2019). This also signalled a shift from the possibility of just working on the histories of organizations towards the history of management ideas in themselves, a theme I explored further in my chapter on the development of strategic management (Tennent, 2020)*.*

A further positive field of development inspired by my broader involvement in teaching in the business school was the opportunity to contribute towards devising a confident pedagogy to introduce history in a meaningful sense (beyond the Chapter 2 history alluded to by Cummings, Bridgman, Hassard and Rowlinson, (2017) to the business and management curriculum. Since I began teaching Strategic Management in 2010, and then the closely allied field of International Management, I had felt that these fields were intriguing (if not always conscious of their roots in historiography) but yet the case study method of teaching them to students was sometimes quite one dimensional. Alongside the destructive disconnect from reality which Mintzberg (2004) identifies as a consequence of case study teaching there was a strong sense that students somehow lacked interest in cases over which they felt they had no ownership. Further, case study teaching using fictionalised versions of real firms (often picked because they were ‘famous brands’ familiar to students) did not seem to equip students well for the social science research methods they were subsequently expected to use for dissertation research. It seemed instead that for at least some teaching exposing learners to business archives (rather than just history in itself) might instead provide an opportunity to wrench students away from this comfort zone by giving them the opportunity to see the traces of past business activity in practice. So alongside launching a third year teaching module attempting to do this we wrote a paper for *Management Learning* which attempted to encourage such an agenda, essentially attempting to bring the gains of the historic turn to the classroom (Tennent et al., 2020). A positive contribution to teaching and learning in the business school that goes beyond the mere learning of historical fact, building reflexivity as a key skill, must surely also be crucial to the future development of the management and business history topic area.

**Looking Ahead: An Agenda for the 2020s**

In the 2020s as management and business historians we will need to retain our identity as a sub-discipline – while remaining both ambitious and confident in the sub-discipline. While we should contribute to the mainstream of management and organization studies discussion we need to be wary of our identity becoming subsumed within it, much as the generation led by Hannah were wary of being subsumed by quantitative economic historians. While the historicisation of the management discipline is certainly desirable, to maintain dual-integrity in the mode of Maclean, Harvey and Clegg (2016), we surely also need to maintain our distinctive historiographical and methodological capabilities. This includes the unique understandings of historical context, chronology, continuity and change that we bring, and can perpetuate by fostering new generations of historians. An involvement in management pedagogy is undoubtedly a critically important way to achieve this, in addition to continuing to build and indeed reinforce our research ecosystem. This will involve being confident in our position in order to oversee the inclusion of history in pedagogy and perhaps involves moving into administrative positions within business and management schools which allow us to influence these processes. Further, we will need to play the soft political game that positioning our journals to gain esteem requires, alongside publishing relevant material in them, assuming the practice of assessing the output of academics by journal publication persists.

*Teach history to reinforce research*

 We further need to be confident in history’s potential to stimulate managerial reflexivity (Alvesson and Sandberg, 2020; Cunliffe, 2016) and historical consciousness (Rüsen, 2017) in view of the on-going 21st century crisis in the various forms that it takes. We are in a tumultuous historical period, which demonstrates well the risks inherent in the gnomic mode of thought, re-enforced by a teleological viewpoint assuming economic, social and technological process to be automatic, which is normative in much management and business school pedagogy. To engage with some of the 21st century’s ‘wicked problems’ all of society needs to think about crisis and change (as well as continuity, indeed) more reflexively. This includes to some extent encouraging the next generation of managers to think about their role in creating historical change. Understanding change, whether say in markets, society or practices, is one of the central themes of our topic area. While perhaps avoiding the construction of reductionist dialectics we can engage with how change as a historical process has happened in the past, for instance when industries grow or decline, causing impacts onto broader organizations and society. We can also conceptualise how the broader historical context interplayed with these changes.

Further, while the idea that there are ‘lessons from history’ may be controversial with some historians we can provide some applied teachings from the past experience of business, management and organization in terms of events and strategies. This might involve building empathy with historical actors to understand their emotional decision processes rather than simply teaching austere hypothetical theories about what might happen, a potentiality discussed by Popp and Holt (2012). While historical context means that the same event can never happen twice, the very understanding of historical context which itself is typically comprehended through reference to dissimilarities from the present means that we can attempt to interpret and evaluate the processes affecting past events. Even more powerfully, we can teach students to identify these historical processes themselves through the interpretation of archival material and the construction of an understanding of how we might interpret causality. Students can further learn and become confident about building interpretation skills as they realise and become comfortable with the idea that competing explanations are possible for such historical processes. Becoming comfortable with interpretive epistemology is a valuable skill especially for students who have come through educational systems that privilege the positivist logic of STEM thinking. Such skills encourage students to detach from themselves the notion that topics such as strategic management have positivist logics, allowing them to become critically reflexive practitioners able to understand and critique the strategic narratives that organizations that come to employ them build.

We therefore need, as a research community to heed the call of Cummings and Bridgman (2011) for greater teaching of history in business and management schools. This requires MBH to establish itself pedagogically. One of the broader shortcomings of twenty-first century academia has been to emphasise the separation between teaching and research, which has led to an almost total decoupling between the two in the professional lives of some academics. A more holistic approach in which we more assertively link our teaching to research and vice versa would provide an opportunity to introduce history into management pedagogy. Following two main teaching strategies can achieve this:

1. *Historical context of ideas*. Historical background can be put into the teaching of ideas, so that students are aware of the context in which an idea arose. For instance, today the Ansoff (1965) matrix is still taught in strategic management, as are the ideas of Michael Porter (1980), but the pedagogy (and especially the textbooks which reproduce these ideas) rarely mentions the historical contexts in which these ideas arose – Ansoff as a product of the planning and diversification crazed 1960s, Porter as a product (or perhaps a producer) of the 1980s obsession with competition. Strategic management pedagogy thus fails its own PEST (1986) analysis, as the as the political, social, economic and technological context of these ideas is overlooked! Clearly there is a danger that such background history could be reduced to teleologically uncritical ‘chapter 2 history’ as Cummings, Bridgman, Rowlinson and Hassard (2017) call it, but a critical awareness of the temporal context of these ideas would build the historic consciousness of students. This would not only reduce the faux-scientific character of this theory but also led to students applying it to the needs of the present more critically potentially introducing ‘dual integrity’ into teaching.
2. *Historiography and archival practice.* This would involve the introduction of modules that more specifically introduce students to history through the study of historiography and the use of primary sources, in a similar fashion to the pedagogy of university history departments. This could include a compulsory first year general ‘Histories of Capitalism’ or ‘Histories of Management’ module providing a contextual overview of the historical development of business and management ideas and practices since the industrial revolution. Focusing mainly on historiography and using survey texts such as Blackford (2008) for business history, and Wren and Bedeian (2020) for management history, this could mirror the scene setting guide to previous practice received by first year students in many professions, including architecture (see for instance University of Dundee, (2020)). Some limited primary source interaction could be introduced. Such an approach would establish history as a legitimate and relevant field of study for management students and help to counter the perception frequently met by the author that few students would take an optional module in history. An option module pitched at second, third, or fourth year (depending on the national system) could then be introduced to give students a chance to continue their learning journey. This module could be more specialised around a certain time period (for instance, the comparative business history of one country after 1945), allowing accessibility and a central theme, but could also feature more exposure to archival enquiry, with students building up skills of archival interpretation across the module. This would allow students to engage with ‘unsettling knowledge’ about past business practice as discussed in Tennent, Gillett and Foster (2020) as well as building the qualitative skills of students, but might also encourage the potential for historical and archival dissertation topics to be studied. Such a module might usefully supplant some existing case study based modules too, because it would provide an opportunity for students to do the sensemaking of the messiness of ‘factual’ past business activity for themselves, rather than a comprehension exercise about a fictional abstraction as at present.

 The historicisation of the management school pedagogy would therefore go a long way towards the legitimation of our position as historians would no longer be an expensive luxury to Deans, but have practical utility in teaching a core element of the curriculum. There is also potential for such approaches to be included in and enrich the many MSc and MBA courses taught in management and business schools around the world, providing students who are closer to practice with new opportunities for reflection. This would compliment the ‘war stories’ told by practitioner tutors. Further, the broader teaching of history would rebound into the research agenda creating the scope for the broadening of enquiry in our field also called for by Cummings, Bridgman, Rowlinson and Hassard (2017), as well as incorporating history into the development of management ideas in itself. The normalisation of MBH teaching should therefore be paralleled by the continuation of ‘dual integrity’ type publication strategies by management historians where it is possible to use archival methods to refine theory. Papers that have achieved this in an applied way include Maclean, Harvey, Suddaby, and O’Gorman’s (2018) work on the institutional formation of the global hotel industry, Perchard and MacKenzie (2020) on path dependency in government-business relations, and Heller and Rowlinson’s (2019) work on the formation of imagined communities in organizations. All of these papers demonstrate a theoretical contribution, which emerges from a rigorous engagement with historical evidence. There is also, echoing the earlier concerns of Taylor, Bell and Cooke (2009) a visibility of method designed to overcome the misunderstanding between historians and social scientists that arises over the nature of secondary data as alluded to by Perchard, MacKenzie, Decker and Favero (2017). These studies also have the advantage of being teachable; being based on archival evidence, rather than interviews or other forms of research based on contemporary subjects, they feature named organizations within their contexts, making it easier for learners to relate to and emphasise with them. Their empirical nature means that they are not characterised by extreme abstraction and thus challenging for learners to get hold of; the historical examples create a sort of narrative that aids sensemaking.

*Being confident in our capabilities*

 At the same time we should not forget the potentiality of historical research in itself, perhaps getting relegated to a secondary role as scholars become more concerned with the prestige of publishing in certain journals. The danger of tilting MBH too far towards a broad theoretical focus might therefore be that some of the diversity of the subject is lost, or that the potential for future diversity to be increased, is reduced. Chasing historical and theoretical novelty by prioritising empirical research that has a clear theoretical importance therefore could lead to an overlooking of enquiry that has greater long term resonance – a sort of historical research equivalent of overlooking of the riches of the Amazon. Some exploratory research therefore surely remains necessary to discover future potential sources, topics and phenomena for theoretical explication and which should not require positioning for publication beyond the historiographical, because the topic is empirically novel on its own. Chia and Holt (2009) remind us that deliberate design is not always required for strategy – this insight can surely be applied to some extent to our own research approaches, which to some extent might be recalibrated to take the specific as seriously as the generic. Othering a research topic as too specific, or not of ‘global interest’ [particularly if the reasoning for this is that no comparator to the USA is included], especially before it is fully developed, essentially amounts to a hegemonic exclusion process. Here another new movement, which is emerging in the business history field, the ‘narrative turn’, argues for a move back and a reconnection with the broader field of history as a discipline, which as Decker (2015, p. 233) observes would require a broader historiographic focus, in which ‘theory’ might consist of considering antecedence to more ‘contemporary’ phenomena or practices. Mordhorst and Schwarzkopf (2017) and Popp and Fellman (2017) strengthen this call to return to a tacit use of narrative as the main storytelling and structuring device, arguing that such a turn might allow for an integration of the more realist and relativist positions in the discipline. Here though the novelty includes a welcoming of the potentiality of narrative as well as the reflexive acceptance that the historian is the constructor of the narrative, and an escaping of White’s (1973) identification of embarrassment that the narrative exists, which was at times a pitfall of the analytical and corporate history schools. The premises of both movements interest me but it is worth reflecting that the potential growth of rival schools represents a depending and maturing of the MBH discipline – much as Porter encouraged the rival RBV school to emerge in strategic management, giving it a platform at conferences, in order to strengthen that discipline overall (Barney, 2011).

While the rise of historical organization studies is undoubtedly important in placing historical research at the centre of management studies, there is a danger that without paying attention to our strengths that the field could eat itself just as cliometrics consumed economic history, with the cherry picking of static historical moments as case studies to justify a theoretical refination. The narrative turn allows potential for us to draw on our core competencies as historians, such as an understanding of context, processes of continuity and change and their reconstruction, consolidating that internal integrity which is surely necessary to achieve dual integrity. Knowledge in our field proceeds along two vectors, the theoretical and empirical – and it is unclear why the theoretical must always be privileged over the empirical, especially if the two are implicitly considered to have a symbiotic relationship. If theoretical knowledge can be advanced incrementally through refination then we should have the confidence as historians to also be able to work it the other way. Papers published in historical journals, if not making a theoretical contribution, should make their empirical contribution clear. Such theoretical refinations, pursued in a methodologically reflexive way, should encourage other social scientists interested in the past to pursue rigourous historical research methods based on principles of context and chronology, with a clear understanding of continuity and change. This will prevent further attempts at methodological reinvention of the wheel which seem unmindful of history’s deeper traditions, such as Gill, Gill and Roulet’s (2018) claim that historical research somehow needs to be more ‘trustworthy’. Historiography provides opportunities for greater contribution to historical organization studies too. Decker, Hassard and Rowlinson (2020) reinforce the importance of historiography and its potential importance to historical organizational studies, while also offering the possibility for archival and memory based studies to enter into conversation with each other.

The privileging of an empirical contribution is a practice which historians and historical journals have at times been weak at; MBH journals have relatively low impact factors in part because we do not cite ourselves enough.[[3]](#endnote-3) In the parallel and related applied field of sport history, Stone (2017) has criticised historians for lazily citing long published books as cannon instead of more recent revisionist historiography published in well respected journals. While such books might constitute ‘standard’ works we should remember that they were constructed by their authors, who made conscious and subconscious choices about which sources to draw upon and how to emplot the narrative and abstract out their conclusions. To become more reflexively aware and more impactful we must foster a culture of critical engagement with the work of our own peers, whether authored by an experienced and respected professor or an ‘early career researcher’. Further, a well-defined empirical contribution with a clear idea of context and chronology can aid the identification of multiple theoretical contributions. Ideally, an MBH researcher might pursue a research agenda on a topic, say the comparative history of an industry, firm or project, followed by a publication strategy entailing the production of a book and then several carefully targeted theoretical and empirical papers.

*Breaking new ground*

 Creating such a culture of critical engagement in terms of the empirical as well as theoretical development of MBH would equip us well to further pursue a broadening of the topic area, as suggested by Cummings, Bridgman, Rowlinson and Hassard (2017) including the global south on its own terms as well as the global north. Broadly speaking, to create the potentiality to contribute to the solution or mitigation of global problems we will need to use our capabilities to cast our net as broadly as possible, and one approach might be to think critically in terms of engaging with forms of management in other cultural traditions for the own terms, using the multi-polar approach of (de)coloniality as proposed by Wanderley and Faria (2012). We tend to cast our periodization from 1850 onwards, but a clear reason for needing more early modern and industrial revolution period management history would be to throw light on the origins of large scale business and management as the infrastructure for colonialism, with even imperialism itself constituting a business, thinking of early chartered corporations such as the Dutch and English East India Companies. Smith and Tennent (2016) illustrate that these developments encouraged steps towards shareholder value capitalism including the separation of ownership and control, and later limited liability. This process built into the mid-nineteenth century in the Atlantic world – Cooke (2003) points directly to the straightforward denial of the role of slavery in management and business history, especially in the formation of management cadres and the emergence of techniques for motivating workers. Imperial historians such as Cain and Hopkins (2002) and Davis and Huttenback (1987) have tended to view the development of Empire from the metropole, as a function of the political and economic strategy of the metropole. To some extent my own work on Australia and New Zealand, and the American West (Tennent, 2009, 2013b) falls into this bracket, using archives generated by companies based in the metropole to uncover their contribution to colonisation processes. The focus on the metropole is not just a reflection of existing biases but also a result of the concentration of paper records and archives about the colonisation process in the metropole, which makes research in colonised countries more difficult, a problem alluded to by Decker (2013). Cornelius, Amujo and Pezet (2019) take a multipolar perspective, looking at colonisation from the perspective of local rulers and workers in Colonial Nigeria as well as from the perspective of the British, demonstrating how the marginalised groups were able to mobilize resistance. There is also more potential for using the archives of the metropole to look at the pressures and tensions of the decolonisation process, as seen in the work of White (2019) and Mollan, Frank and Tennent (2020), as forms of soft colonial power attempted to maintain themselves through earlier investment linkages.

 Deeper resources also exist for the expansion of the topic area in countries that have already been well covered by MBH, perhaps in industries away from the traditional heavy lifters of the industrial revolution such as textiles and engineering. Here also historic accounts such as Owen (1999) have tended to focus on the economic and strategic histories of firms as well as industrial relations, while social histories and other angles such as marketing, communication and PR often remain untold. Considerable resources remain for exploitation both in corporate archives and even at archival institutions well known to business historians, including the Hagley Institute in Wilmington, Delaware. Other sectors have not even benefited from much economic or strategic history, including rigorous financial analysis, of the sort which has become less common but which might itself make more of a comeback as an analytical genre. Leisure and cultural industries are one area which continue to receive relatively little attention, despite their historic linkages to the consumer electronics industry and in the case of sport, sponsorship and philantrophic influence from local employers (Gillett et al., 2016; Heller, 2008). Gillett and Tennent (2020) have also followed Scranton (2014) in suggesting that project management histories may offer management and business historians an alternative window into the institutional processes at play in many industries from heavy engineering to the organization of sport events. The temporary organization (Lundin and Soderholm, 1995; Morris and Geraldi, 2011) as a mode of governance is not new – the free-standing companies of Wilkins (1988) which brought together lawyers, accountants, merchants, company promoters and mining engineers could be re-conceptualised in this form. Mollan (2018) has suggested that research on the FSC which sees the firm as the unit of analysis has reached its limits, an insight which can applied to a broader range of empirical cases. Many industries now and in history have relied on such P-form structures needed only during the lifespan of a project, whether building a railway in the 1830s or making a computer game in the 21st century. A topic to be further explored from new perspectives, in say the film industry, is why a project form was desirable in some times and places, but the internalisation of the Chandler or Williamson types of corporation was considered more desirable in others. Further, the study of projects as the unit of analysis forces the researcher to use sources from multiple archives, creating diversity of perspective and new opportunities for understandings of organizational phenomena not feasible when the corporation is the unit of analysis.

Even some apparently well-chronicled industries such as rail transport have new angles which can be explored, because of the sheer size and diversity of activities of the organizations in that sector - a good example of recent activity in this direction is Turner’s (2018) work on the role of railways in developing North Wales as a tourist destination. Related to transport is also the extremely broad area of management and business in the public sector, political economy and municipal governance, including municipal trading. Public institutions tend to have remarkable archival resources which historians can exploit – the accent has tended to be on those held by national government, but local government has considerable resources too, as demonstrated by Turner and Tennent (2019) on trams in London, and Fowler’s (2019) history of governance in London Transport in the first half of the twentieth century. Little has been done on other forms of public or quasi public institutions – for the universities at which we work tend to maintain excellent archives relating to their own activity which could be used by scholars interested in the history of education and professionalised organizations. An exception to this is Pérez’s (2021) forthcoming history of the hospital sector, which uses a global comparative history to examine the temporal development of these institutions. A discussion of public management history is likely to provide ammunition to historians interested in engaging with the institutional logics perspective as forms of hybridity (Powell et al., 2019) in history can be considered.

**Concluding Remarks**

 I have offered here a personal reflection on where we have been as a discipline since I joined it in order to suggest a broad, holistic research agenda, both in terms of where MBH might make epistemological and ontological as well as pedagogical progress in the 2020s. We should be ambitious and confident as historians going forward, not just in our integration of our work into the theoretical discussion of management and business studies as a whole, but also the empirical contributions that we can make to it – indeed, we can contribute empirical research expertise and contribute to teaching where other forms of social science research and pedagogy are potentially lacking. We need to take a holistic yet reflexive approach to the topic in which no one approach dominates, but in which different forms of historical research and presentation compliment and are in rigorous discussion and debate with each other – overall comparative styles of history of the sort pursed by Hannah and colleagues, and proto-Annales metanarratives are useful but should be challenged by microhistories and studies of more specific phenomena. MBH, containing a diversity of approaches, should be emboldened and seek to retain a confident, rigorous independence within management studies. Such an approach will allow us to expand the scope of the field and challenge some of the underlying premises and gnomic presentism of management studies itself, pursue research on the darker and more critical themes within management and business history, and even perhaps contribute an understanding that historical construction is a form of sensemaking in itself. Further we can assert our ownership of the study of the past within management studies through increased methodological reflexivity and confidence in the value of empirical contribution based upon our core competences of an understanding of historical context, construction and chronology, protecting our domain from more cosmetic or less critical uses of historical data. I would like to be able to emulate Hannah and Gourvish in helping to continue to construct the ecosystem which can support this kind of work. Finally while it is fun to talk and write about how to do history – but I should really get back to actually doing it now!

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2. I use Management and Business History as a short form here for the interlocking fields of management, business and organizational history that I realize do have slightly different units of analyses and traditions. However, scholars from these fields converse with each other through journals such as JMH and events so I think it legitimate to consider it a descriptive term for our broad field. [↑](#endnote-ref-2)
3. Here I am referring not just to JMH but also what I would consider the major journals aiming at a general audience in the MBH field – including *Business History Review*, *Business History*, *Enterprise and Society* and *Management and Organizational History.* [↑](#endnote-ref-3)