THE ONE BEST WAY? ‘SCIENTIFIC’ RESEARCH ON HRM AND THE THREAT TO CRITICAL SCHOLARSHIP

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Abstract

HRM research has followed the broader field of management studies towards ‘formulaic’ research in recent years (Alvesson and Gabriel 2013). It would appear that ‘quality’ HRM research is characterised by a series of distinctive features. First, a positivist methodology, with an emphasis on hypothesis testing, statistical rigour and incremental theory building is dominant. Associated with this, researchers employ increasingly complex, often multilevel, statistical models and there is more and more emphasis on technique. Second, it is dominated by correlational theorising (Delbridge and Fiss 2013), informed in particular by social psychology. Third, the research generally generates incremental advances in theory. Finally, research findings are represented in journal articles in the form of ‘science’ (Corbett et al 2013).

This should be of concern to critical scholars because the increasing dominance of a single, narrow approach has the effect of marginalising alternative, critical scholarship. Labour process critiques of HRM have tended to challenge the underlying theoretical focus of HRM, but what they appear not to have done is to engage sufficiently with methodological developments. In particular, labour process scholars appear to have failed to notice the extent to which the scope of HRM research has narrowed in recent years, with convergence towards a ‘one best way’ (Mingers and Willmott 2012). While this approach has long been prominent in HRM research, what is new is the almost total dominance of this kind of research, which has largely crowded out alternative approaches. A variety of institutional factors has contributed to this shift in HRM scholarship and these place significant obstacles in front of those who seek to reclaim space for critical voices. There is, however, room for greater methodological pluralism and researchers, academic institutions and journals have roles to play in promoting viable alternatives.
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Introduction

Recently, a number of authors have commented on the extent to which the field of management studies has narrowed, with creativity in research increasingly being supplanted by ‘formulaic’ research (Alvesson and Gabriel 2013) and the increasing narrowing of academic publishing activity to ‘the one best way’ (Mingers and Willmott 2013). From a variety of perspectives, a number of authors have observed some of the same trends in the field of HRM (eg. Harley and Hardy 2004, Kaufman 2012, Godard 2014). This paper seeks to add to this body of knowledge by discussing the increasing narrowing of HRM research and the threat which this poses to critical scholarship. It is intended to form the basis for an ongoing piece of empirical work, involving the systematic coding of HRM papers in ‘top’ journals as a means to develop a clear picture of changing patterns of publication. This in turn will provide a means to develop a fuller theorisation of the reasons for the apparent narrowing of the field in recent years.

The paper begins by briefly mapping out the emergence of HRM as an academic discipline and the pervasive concern with finding the ‘holy grail’ of a clear link from HRM to organisational performance. In doing so, the paper argues that since the 1980s, the research has become increasingly narrow in its theoretical focus and increasingly obsessed with technique. In the second substantive section, the paper presents a picture of the current state of mainstream HRM research, characterised by: quantitative method, increasingly involving very complex statistical models; a theoretical underpinning taken from psychology; the pursuit of incremental advances in theory; and the presentation of results in the form of ‘science’. The third section argues that this approach is problematic because it marginalises research which does not confirm to the tenets of the dominant approach, with the result that the field is becoming increasingly narrow and divorced from practice. Discussion then turns to the prospects for a more pluralistic approach to studying HRM and a series of tentative suggestions is put forward, while noting the challenges faced by scholars who wish to challenge the dominant approach. The paper concludes with a brief discussion of the next steps for this project.
HRM Research and the Quest for the Holy Grail

The concept of human resource management (HRM) can be traced back at least to the 1960s in the United States (Kaufman 2007: 37), but the genesis of our contemporary understanding of the term was in the 1980s. One influence was the economic success of Japan, in comparison to the US, which saw some scholars look to the east for inspiration. Based in part on their observations of successful Japanese companies, these authors reached the conclusion that the key to success was to treat employees as valuable resources to be nurtured and developed and whose commitment must be won by humanistic and developmental management practices (Thurow 1992). Of course, these ideas were not exclusively Japanese in origin and to an extent they had their roots in earlier approaches which emphasised developmental humanism, most notably the Human Relations School.

This view of the key to organisational performance formed the basis, in the rhetoric if not always the reality, of HRM as it is commonly conceptualised today. While much of the shift from ‘personnel management’ to ‘human resource management’ may have been as much about labelling and terminology as about practice, nonetheless the 1980s saw the emergence of HRM as a new field for both academics and practitioners. As well as an emphasis on treating employees as valued assets to be nurtured, the emergent theoretical models of HRM emphasised a strategic role for the management of people as a driver of business performance (see Beer and Spector 1984). While the contemporary model of HRM was developed in the US, it rapidly spread across the globe during the 1990s (see Kaufman 2007 for a historical account).

At the heart of attempts by academics and practitioners to establish the legitimacy and relevance of HRM was the claim that ‘good’ HR practices were associated with superior organisational performance (Ramsay, Scholarios and Harley 2000). If it was possible to demonstrate that HRM could drive business performance, then HR practitioners would have a claim to a key strategic role within organisations and HR academics would have a claim to a vital body of knowledge. Thus, HR researchers embarked on “the search for the Holy Grail of establishing a causal relationship between HRM and performance” (Legge 2001: 23) and this became the dominant stream of research in the field.

During the 1990s there was an explosion of research which sought to demonstrate the HR-performance link (eg. Huselid 1995; Ichniowski, Shaw and Prennushi 1995; MacDuffie 1997). For the most part the first wave of research on the HR-performance link was based on
large-scale surveys, which captured data about HR practices on one hand and performance on the other and which tested associations while controlling for a variety of organisational and/or industry factors using regression analysis.

A variety of theoretical models was employed to frame this research, but the dominant approach was that associated with the so-called ‘high performance work systems’ (HPWS) model (see Harley 2005). Briefly, this model asserted that combinations of HR practices, which emphasised non-Taylorist jobs, the provision of skills and the use of motivation mechanisms such as performance-based pay, would enhance employee discretion, satisfaction, commitment and motivation, as a result of which they would contribute to organisational performance. That is, this model proposed that HR practices had an impact on employees’ experiences of work and therefore their behaviour, which fed into organisational performance.

While much of the research during the 1990s and into the 2000s was informed by the HPWS model, very little of it directly tested the model in an empirical sense (for an exception see Ramsay et al 2000). That is, rather than conducting analysis which integrated employee and organisational data as a means to explore causal pathways, most research assumed that the causal path from HRM to performance was via a range of employee responses. There was a small amount of research which made employee responses a matter of interest in their own right (eg. Harley, Allen and Sargent 2007), but for the most part employees were absent in an empirical sense from the research. It is important to note that there was some research of a more critical bent, both quantitative and qualitative, which explored empirically the implications of HRM for employees (eg. Harley 1999; Danford, Richardson, Stewart, Tailby and Upchurch 2008), but the focus of this paper is on dominant mainstream research which sought to test the HR-performance link.

By the early to mid-2000s, it had more-or-less become an article of faith that (a) HRM (as conceptualised by the HPWS model) contributed to organisational performance and (b) that it did so through its effects on employees. This was somewhat remarkable considering that the evidence for the HRM-performance link, even within the limitations of cross-sectional studies, often showed at best a fairly weak link. For example, Combs et al (2006) in their meta-analysis of nearly 100 studies found a coefficient of 0.2 for the association between HR and performance. It was also remarkable given how little research had been conducted by the
mid-2000s that included both employee and organisational data. Nonetheless, it appeared that
the holy grail had been found.

By the mid-2000s, cross-sectional research attempting to link HRM to performance had
largely reached its limits in terms of being able to provide any more basis for claiming that
such a link existed. For a short time, it appeared that the research on HRM and performance
had come to a grinding halt. It was now conventional wisdom that effective HRM caused
performance gains for organisations, based on the numerous studies that had demonstrated
statistical associations between HR practices and various measures of organisational
performance. There appeared little point conducting yet more cross-sectional workplace
studies to provide further verification. Mainstream HR scholars maintained their focus on the
HR-performance link, but the research entered a new phase.

The Rise and Rise of ‘Scientific’ Research

There appears to have been something of a hiatus in the early to mid-2000s, before another
surge in publications on HRM and performance. Increasingly, publications that began to
emerge in the late 2000s focused on attempts to map causal paths. That is, they sought to
demonstrate how various outcomes of HRM – typically employee responses – mediated the
associations between HRM and performance, as a means to make causal inferences (see for
example: Beltran-Martín, Roca Puig, Escrig-Tena and Bou-Llusar 2008; Sun, Aryee and Law

As a means to advance knowledge of how the associations between HR and performance
appeared to emerge, detailed studies of apparent causal paths, were undoubtedly timely.
There are, however, a number of ways that such research might be conducted – for example,
detailed qualitative work is clearly appropriate to developing understandings of causal
processes – but the mainstream research has been more-or-less exclusively quantitative and
survey-based. It has commonly involved complex multivariate models, increasingly of a
multi-level kind, as a means to explore processes of mediation. This has often taken the form
of work in which employee-level variables are tested as mediators between HR practices at
an organisational or unit level and performance outcomes at an organisational or unit level.

As this sort of research has grown, it appears that HRM research has followed the broader
field of management studies towards ‘formulaic’ research in recent years (Alvesson and
Gabriel 2013). It would appear that there is now a standardised ‘one best way’ of conducting
HRM research, characterised by a series of distinctive features. The kind of HRM papers that dominate high impact management journals are characterised by an emphasis on methodological rigour, an almost exclusive reliance on social psychological theory, increasingly complex methods, the generation of incremental advances in knowledge and, as one would expect from the preceding features, presentation in a ‘scientific’ format. It would appear that for many scholars, the move in this direction is welcome, and that if anything we have not yet gone far enough. For example, in a recently published text Research Methods in Human Resource Management, Sanders, Cogin and Bainbridge write that what they refer to as ‘advanced research methods’ (experiments, multi-level modelling, etc.) can assist HRM researchers in their efforts to tackle the central challenges in the field. Well-specified research models that are based on sound theory, valid and reliable measures, and advanced analytical procedures enhance researchers’ likelihood of demonstrating significant effect sizes in their examination of the HRM-performance relationship. In addition, there is evidence that studies that utilize more advanced research methods have a greater impact...In turn, they are more likely to enhance the possibility of generating valid answers to questions about the effectiveness of HRM, to influence future research in the field, and to create positive perceptions of the rigor of the discipline (2014: 2).

The closest to acknowledging that anything other than quantitative research methods might be applied, is in a single chapter on mixed methods. Indeed, the conclusion that these authors reach is that in fact the problem with HRM research is that has not yet gone far enough in adopting scientific methods. While this is only a single illustration, it highlights the point that in many circles, it is assumed that the correct way to conduct HRM research is via complex statistical analysis.

One might well ask, what’s new? Hasn’t most of the mainstream HR research been conducted in this way in the past? It is certainly the case that quantitative, hypothesis-testing, survey-based research has been the dominant form of work in this field. What is new, however, is the extent to which the field has narrowed its focus and the extent to which this increasingly narrow approach appears to be regarded as the proper way to conduct HR-performance research. In the next part of the paper, attention turns to a more detailed consideration of why the increasing dominance of this approach is problematic for HR scholarship and particularly for critical HR scholarship.
What’s the Problem?

An obvious explanation for the dominance of this particular approach to HRM research is that it is simply the best way to conduct research in this field. Who could possibly argue with rigour, with the use of a well-established and credible body of theory and with the scientific attempt to test hypotheses as a means to fill gaps and advance knowledge incrementally but with a high degree of confidence? The argument that I want to advance here is that this kind of argument in favour of the dominant approach is not as straightforward as it might initially seem to be. In the next part of the discussion, I will highlight what I see as the key problems with this dominant approach.

First, there is the issue of the increasing dominance of psychological theory (see Godard 2014 for an account of the rise of psychology in HRM research). One might well argue that it makes sense to draw on a body of well-established and credible theory which is grounded in empirical work. Psychology is widely recognised as one of the more ‘scientific’ of the social sciences and to that extent we can regard theories drawn from this field to be particularly well-grounded in research. So where does the problem lie? First, social psychological theory focuses on a particular set of explanations for phenomena – the focus is primarily on how HRM and other workplace features have an impact on people’s attitudes and behaviours (for example, commitment, satisfaction and motivation). These explanations revolve around attitudes and behaviours of individuals (even if they are sometimes aggregated to the group level) and thus do not take into account phenomena which exist beyond individuals, but which are likely to be important in explaining phenomena, eg. structures of power, regulatory mechanisms and collective actors such as trade unions.

In addition, this kind of theory takes a very specific form – it comprises specific causal claims about associations between variables – and commonly seeks to make universal claims about such associations. The theoretical models which result are parsimonious, reflecting the ‘scientific’ approach, and involve making testable claims about causal associations. This could be conceptualised as being about ‘boxes and arrows theory’ or “correlational theory” (Delbridge and Fiss 2013). Delbridge and Fiss argue that:

This approach to theorising tends to perceive the social world mainly in terms of linear relationships that take a correlational form of “the more of X the more of Y”. It focuses on the net effect of independent variables, assuming that, in general, each variable by itself would be capable of bringing about the outcome of interest, holding constant the effect of all other candidate variables (2013: 328).
What this means is that the explanations which are likely to emerge are limited to a narrow range of mechanisms, predominantly psychological. The ‘story’ which emerges from the research is of a very specific kind and the narrow focus on causal claims about specific variables means that explanations of a holistic nature are not likely to emerge.

One effect of this is that more critical approaches tend to be absent. For example, it is not possible to account for collective power imbalances between employees and management within such theory, yet this seems likely to be a crucial factor in explaining outcomes of HR practices in some contexts. More generally, this approach does not deal with explaining processes, which in many cases involve multiple causal mechanisms operating simultaneously.

Second, there is a very strong emphasis on rigour. At first glance this appears unproblematic. Isn’t rigour a good thing? Of course it is, but in most of the HRM research, ‘rigour’ is conflated with ‘statistical rigour’. Strictly speaking, rigour refers to the application of consistent principles to reasoning and analysis. Donaldson, Qui and Luo in their defence of rigour define it as “the logical pursuit of an idea” (2012: 154). This would suggest that one can be just as rigorous in abstract philosophising as in statistical analysis. Similarly, any methodology can be applied rigorously – ethnography, interviewing, discourse analysis and other qualitative approaches have very clear sets of rules about rigour – but the conflation of ‘rigor’ with ‘statistical rigor’ has led to at least an implicit view that the mainstream approach is rigorous and thus credible, and other approaches are less rigorous and less credible. Thus, qualitative research appears, implicitly at least, to be a less acceptable approach than quantitative work.

Moreover, the claim to rigour is at times questionable in this kind of research. For example, most of it is based on surveys, which really involves taking qualitative data and squeezing it into quantitative categories, so much of the faith we have in the way variables are operationalized and associations measured precisely is at the very least open to question. The result of this focus on (one kind of) rigour seems to me to be that there is a narrowing of the kinds of questions that are asked and written about. Those that can be dealt with ‘rigorously’ get addressed and others do not. Papers which are ‘scientific’ are widely cited while those which may be equally rigorous, but which proceed from a different set of ontological and epistemological premises are not (see Harley and Hardy 2004).
Third, along with the emphasis on statistical rigour and on theories which apprehend the social world in terms of linear relationships, there is a growing emphasis in HR research on complex multilevel statistical models. Again, it would seem easy to justify this approach. Researchers are dealing with complex issues, so surely complex modelling is the correct empirical approach. The reason that the increasing statistical complexity of research is a matter for concern is that the more complex the method becomes the more of the energy of researchers goes into technique. The risk is that the emphasis is more and more on the technique and the precision of the results, with theory and practice taking a back seat. This has at least three potential consequences. The first of these is that the research is increasingly specialised and thus less likely to engage a wide academic audience. The second is that currently fashionable methods drive the questions which are asked. To put it another way, the methodological tail may end up wagging the theoretical dog. The third concerns the relevance of research to practice. As models become complex and specialised, there is a risk that the results are of less apparent substantive relevance to practice.

The fourth issue concerns the fact that the research appears to aim at making incremental advances in knowledge, in keeping with the tradition of the physical sciences. The dominant stream of research takes the form of reviewing the literature, identifying gaps, collecting data to test hypotheses as a means to fill the gaps, and thus slowly advancing knowledge – what Kuhn characterised as ‘normal science’ (1970) – rather than making any kind of methodological or theoretical leaps. This might involve adding a new mediating or moderating variable to an existing model, for example. This means that the research rarely generates major theoretical advances – rather, it chips away, on the assumption that knowledge is cumulative and progressive and that each new piece of research puts in another piece of the jigsaw. This is not of itself illegitimate – indeed there are compelling arguments for this approach – but the problem is that as this kind of work becomes more and more prevalent, theory is likely to advance more and more slowly. There would seem to be less and less room for major theoretical advances.

Finally, there is the issue of representation. As one would expect given the features mapped out above, the mainstream HR research which is published in high-impact journals is presented in the form of ‘science’ (Corbett et al, 2014). That is, it is presented in a form which seeks to emulate the conventions of the physical sciences and to present findings in mathematical terms and to highlight neutrality and precision. It is possible to contrast this kind of presentation with ‘stories’ – research which places more evidence on qualitative data,
narratives and interpretation (Corbett et al, 2014). The problem is not with the representation of research as science as such, but rather with the fact that it seems increasingly to be the case that ‘science’ is seen as legitimate, whereas ‘stories’ are not, in spite of the fact that much social science research can legitimately be presented as the latter. Consequently, papers which conform to the norms of scientific representation tend to be afforded greater legitimacy than those represented in other ways (Harley and Hardy 2004).

There would seem to have been little attention within the labour process tradition to engaging with these methodological developments in HRM research. The methodological critiques of mainstream HRM research that have been published have generally argued that there are such fundamental ontological and epistemological flaws in the dominant stream of HRM-performance research that it is a futile project (see Legge (2001) for a poststructuralist critique and Fleetwood and Hesketh (2006) for a critical realist one). Labour process critiques of HRM have tended to challenge the underlying theoretical focus of HRM (eg. Thompson and Harley 2007; Thompson 2011) or on the failure of HRM researchers to locate their research in the context of a capitalist political economy (Thompson 2013). The challenge that the increasing narrowing of the field poses to scholars working in the labour process tradition is that it is likely to further marginalise work which does not confirm to the contemporary norm.

The argument which I have made in this part of the paper is that collectively the features of ‘high quality’ research – the kind of research which scholars apparently to need to produce to publish in the really top-ranked journals – can be seen as problematic. The dominant approach to HRM research would seem for the most part to be devoid of opportunities to advance theory in significant and interesting ways, especially outside correlational theorising. It also appears to have limited potential to inform practice. Moreover, its rigour is of a rather narrow and limited kind and in any case can be called into question. One might well ask what is the point of conducting such research, apart from the fact that it is more likely to be published in highly ranked journals than is research conducted in other ways? For some, this might be reason enough to continue down this path, but for many there is more to scholarly work than journal rankings and citation counts. How might we seek to pursue an alternative path?
What’s the Alternative? Prospects for Pluralism

It is easy to criticise any body of research. A much more difficult thing to do is to put forward an alternative. This paper does not pretend to propose anything approaching a definitive roadmap for future HRM research, but it poses a numbers of questions and responses which form a starting point for discussion. In the discussion which follows, I consider some possible prescriptions for dealing with the problems mapped out above.

This paper started by mapping out the centrality of interest in the links between HRM and organisational performance which has characterised the development of HRM research, which was characterised as ‘the quest for the holy grail’. Perhaps it is the case that it is this quest which has led HRM research to its current state. Perhaps the focus on performance has contributed to the apparent obsession of mainstream scholars with quantification and precision. It seems likely that this explains in part the current direction of research, and indeed some critics have argued that the focus on performance is so fundamentally problematic in theoretical and methodological terms (Legge 2001; Fleetwood and Hesketh 2006) that it should be abandoned.

Regardless of the views of critics, it seems unlikely that the current focus on performance will disappear in the foreseeable future. Moreover, there are good reasons for critical scholars to engage with issues around performance. The raison d’etre for HRM as with any other set of managerial practices is the functioning of organisations in pursuit of managerially-defined goals, so it seems hard to avoid engaging with these issues. If HRM research is to be relevant to practitioners then surely issues of performance need to be addressed. From a pragmatic point of view, research which focuses on performance seems more likely to attract government and industry funding than that which does not.

Perhaps the problem lies not with the focus on performance as such, but with the fact that performance focus has been at the expense of a broader range of concerns. It seems clear that in the mainstream research, the human experience of work has become incidental – a means to an end – rather than a matter of concern in its own right. The suggestion here is not that there has been no research which has examined worker experiences of HRM as a matter of interest in its own right, but rather that it is difficult to find much such research in the more highly ranked management journals.
A second inference that might be made on the basis of the critique presented in the previous section of the paper is that the problem lies with ‘science’ and that ‘scientific’ research is not the best way to advance knowledge in the field of HRM. That is, researchers should dispense with complex analysis, with incremental theoretical gains and with the associated ways of presenting research. Again, this does not seem a sensible thing to advocate. In first place, such an approach is unlikely to be displaced in the current institutional environment and, moreover, nor should it be. Clearly there is value in ‘normal science’ (Kuhn 1970) and gap filling. A more plausible argument than the one that ‘science’ should be abandoned is that there should be wider recognition that it should only be ONE form of research in this area, not THE form of research.

The real problem here is that this approach has largely squeezed out other forms of work, to the point where it seems the normal and natural way to conduct research in the field of HRM. The methodological narrowness of the dominant approach restricts the kinds of questions that are asked – those that can be assessed through the quantitative operationalization of variables – and thus the kind of knowledge that is created. One reason that social psychological theory is so dominant is that there are well-established ways of operationalizing variables in this domain.

A third inference might be that social psychological theory has no place in HRM research. Clearly this is difficult to sustain. Of course some of the phenomena relevant to HRM researchers can be explained with reference to psychological processes. The problem, however, is that this kind of theoretical explanation has become more-or-less the only explanation, rather than one of a number of explanatory frameworks, at least in papers published in highly-ranked management journals. This has the effect of marginalising other bodies of theory and shifting the study of work and employment within the management discipline from being a multi-disciplinary one towards being a single-discipline one (see Godard 2014).

The argument which is being made here is that the problem is not with the form the dominant approach takes per se. Like any approach, it has strengths and weaknesses. Where the problem lies is with an apparent belief on the part of many that the dominant approach is the best approach and that it should be preferred to others, to an extent where a particularly narrow approach appears to have become dominant almost to the exclusion of other approaches.
What then is the alternative? The argument being put forward here is not particularly radical. It is simply an argument for greater substantive, theoretical and methodological pluralism in research. The only way that we can avoid the problems attendant upon the narrowing of the field that has been mapped out above is by broadening the range of phenomena that are investigated, the range of theories that are used to inform inquiry and the range of methodologies and forms of representation that are adopted.

That alternative approaches are possible within HR research is clear. For example, while much of the more critical research on HRM is conducted using qualitative methods and a broadly realist methodology, there is also a stream of critically oriented research which utilises survey data and statistical analysis (eg. Ramsay et al 2000; Ehrnrooth and Bjorkman 2012; Harley 1999; Harley et al 2010). Some of this research is explicitly informed by labour process theory. This suggests that there need not be a simple mainstream-quantitative vs critical-qualitative split in HRM research and that a range of approaches is possible.

To argue for greater pluralism and to find examples of papers which might challenge the dominant approach is relatively easy, but is there really any prospect of the current narrowing of the field being reversed? Surely there are forces at work which are much bigger than the desires of individuals or small groups to change the status quo. It is beyond the scope of this paper to provide a detailed account of how we came to the current situation, but a number of factors can be identified.

First, historically HR scholarship has been particularly influenced by two disciplines – psychology and economics – both of which might be seen as seeking to mimic the physical sciences and which have a strong emphasis on positivist ontology and epistemology. The legacy of this disciplinary influence is reflected in the fact that positivism is regarded as the legitimate – or certainly the most legitimate – position underpinning research. More recently, as documented by Godard (2014) the field of HRM has increasingly been colonized by industrial and organisational psychologists who have brought with them many of the practices discussed above. Kaufman (2012) also makes an argument about the deleterious effects of the colonisation of HRM by psychology, although his argument that this approach should be supplanted by one based on economics would seem likely to replace one narrow approach with another.

Second, there is the fact that management scholarship emerged in the United States and spread from there. The dominance of the US, where management scholarship has tended to
be more functionalist, positivist and managerialist than in Europe, has undoubtedly influenced what is regarded as legitimate across the world (Corbett et al 2014).

Third, there are much broader changes in economy and society, which are also reflected in the academic world, in which there is increasing rationalisation (in a Weberian sense). Associated with this has been growing emphasis on standardisation and measurement of academic performance, linked to government funding models and to systems of ranking institutions and journals (see Clarke, Knights and Jarvis 2012; Mingers and Willmott 2013). In this context, those who want to succeed in a career in academic management studies are under a huge amount of pressure to conform to the dominant norms concerning legitimate research, which largely entails seeking to publish ‘science’ (Corbett et al 2014).

This brief summary suggests that the current situation reflects quite significant institutional forces. In the face of such an environment, it is tempting to feel like there is no real prospect for substantive, theoretical or methodological pluralism in HRM research. Indeed, this would seem to be the case for those who wish to succeed according to the terms of the current dominant norms of academic success – publications in ‘A*’ journals, citations, employment in prestigious institutions, rising up the ranks, and so on. Moreover, given the power of social norms, many academics would not even question that this was the correct course of action.

While not wishing to downplay the importance of the current institutional environment, it is clear that change is possible. The dominant approach, which we now tend to regard as normal or natural, is itself a relatively recent phenomenon. It is not that long ago that many leading journals published a much wider range of papers, including essays (see David Strang’s as yet unpublished work on shifting patterns of publication in ASQ http://www.soc.cornell.edu/faculty/strang/projects/The%20evolving%20article%20web%20abstract.pdf). It seems plausible to argue that those of us involved in the academic enterprise can have some influence on outcomes, while also recognising that the impact of the actions of individuals and groups is not likely to lead to dramatic or rapid change.

First, and most obviously, there is the role of authors. As members of the academic community, if we are concerned about the apparent narrowing of HRM research, surely we have a responsibility to publish papers which provide alternatives to the dominant approach. This is challenging in the current institutional environment, where hiring, tenure, promotion and remuneration are increasingly linked to (particular kinds of) results. Indeed, it might well be deemed irresponsible to counsel early career scholars to challenge the current model, as
this could have consequences for their careers. For more established academics, however, there are far fewer reasons not to do so.

For authors, this would mean writing and submitting papers which challenged the current norms of scholarship in HRM. The challenge here is to produce papers which do not conform to the norms, but which are theoretically and methodologically rigorous and which put forward genuinely new arguments. Clearly there is a risk in doing so, to the extent that rigour appears so often to be conflated with statistical rigour, with the potential for papers which are not statistical to be discounted as lacking rigour. It is possible, however, to find papers which are theoretically and methodologically rigorous and published in highly-ranked journals, but which diverge in important ways from the dominant approach to HRM research either in terms of using statistical analysis to explore alternative theorisations (eg. Ehrnrooth and Bjorkman 2012; Boxall, Liu and Bartram 2012) or by adopting both alternative methods and theory (eg. Alvesson and Karreman 2007).

Second, there is the question of the role of editors, as gatekeepers for journals. Editors are as much constrained by institutional pressures as any other members of the academic community and few editors could plausibly argue that they are not concerned to some extent with their journals’ impact factors. To this extent, there is pressure on editors to publish papers which conform to the prevailing norms and which are thus likely to be cited highly. Having said that, it is clear that some papers which seek to ‘break the mould’ are highly influential and very widely cited, so there may be payoffs in publishing such work.

The challenge for editors is to attract non-mainstream papers and manage them through the editorial process. This means that editors must use academic forums to encourage the submission of such papers, ensure that their journals utilise editorial boards and ad hoc reviewers from a diverse range of theoretical and methodological backgrounds and be willing to publish a diversity of papers as a means to demonstrate their commitment to challenging the status quo. Editors can use their positions to challenge the status quo and to argue for more pluralism in approaches (eg. Corbett et al 2014; Delbridge and Fiss 2013).

Third, there is a role for educational institutions, and for academics in their roles as teachers and doctoral supervisors, to foster plurality in methodology and theory. There is pressure on institutions to ‘train’ graduate students for the academic job market, which at times is interpreted as ensuring that they have the technical skills to publish ‘scientific’ research in high-impact journals. Surely, however, our responsibility as educators is also to ensure that
students are exposed to a wide range of methods and theoretical frames, so that they are able to approach research from a variety of perspectives.

These may seem like rather limited prospects for change. While all of these things might have a role to play, as already noted above it is unlikely that this will lead to rapid or dramatic change. There is, however, reason to think that over time what counts as legitimate research in HRM may change in ways that lead to greater substantive, theoretical and methodological pluralism. Within the field of management studies, there is a growing body of work criticising the dominant approach in management research (Alvesson and Gabriel 2013; Delbridge and Fiss 2013). There is also evidence that ‘frame-breaking’ papers attract readers and citations.

It might be argued that many frame-breaking papers are just as narrow, esoteric and removed from practice as highly technical statistical papers, but by shifting the boundaries of the discipline they may well open up the field. This kind of process can be observed in the growth of organisational discourse analysis over the past two decades, during which it has moved from the margins of management scholarship to now commonly being published in mainstream journals. While there have been attempts within HRM research at shifting the boundaries of the field (see Delbridge and Keenoy 2010) there has been relatively little progress, but if the broader field of management studies embraces a return to pluralism, there may be hope for HRM.

Next Steps

At present, this paper is framed more-or-less as an essay, with a slightly polemical bent. As a means to develop it, further empirical and theoretical work is required. On the empirical front, it is important to note that the claims about the trajectory of HRM research are based on observations, as well as claims made by other authors (eg. Godard 2014), rather than on a systematic analysis of patterns of publishing. While we can be confident about the broad contours of the changing field of HRM research, it seems likely that when patterns of publications are explored via a rigorous analysis of HRM papers published in ‘top’ journals, the picture which emerges may be more complex than the one presented here. Thus, until my current project of coding and analysing papers is complete, the picture which I present of the field must remain to an extent provisional.
In theoretical terms, the paper currently attempts to describe developments and provide some proposals for addressing the situation. What it does not do is to provide a well-developed theoretical account of *why* the field of HRM research appears to have moved in the direction it is currently taking. Some arguments are put forward about the disciplinary and national heritage of HRM research, as well as broader changes in economy and society which have permeated higher education. This is far from providing a compelling theoretical explanation of the apparent narrowing of the field. Thus, the second task which remains, once the empirical analysis is complete, is to work to develop a compelling theoretical explanation.
References


