

In the eyes of Janus: The intellectual structure of HRM-performance debate and its future prospects¹

INTRODUCTION

The relationship between human resource management (HRM) and organizational performance has been a key challenge for HR researchers and HR professionals alike. In the last twenty years, HR researchers have been highly engaged in providing evidence for the effect of HRM on organizational performance and explaining the underlying mechanisms of this relationship. It is thus not surprising that the HRM-performance debate has been one of the most popular debates in HRM as a field of research. According to ISI Web of Knowledge around 800 papers were published with keywords including HRM and performance (or similar) from 2003 to 2012. The most influential work about the HRM-performance link clustered around the seminal paper by Huselid (1995) also influenced strongly the broader field of management and still represents one of its important knowledge domains. For practitioners, on the other hand, the main challenge was to affirm HR as a strategic function and develop HR systems that could contribute to superior organizational performance. The HRM-performance debate was highly aligned with the needs of the business and instrumental in providing necessary evidence to legitimize an HR manager's "seat in the boardroom" as well as in guiding development of practices that improved the bottom-line.

Despite this track record of influencing academic thinking and producing relevant knowledge for practitioners, the debate faces new challenges. The production of academic output

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within the HRM-performance debate peaked in 2010 with 159 papers and almost halved in 2012, down to 82 papers. The debate has also reached a reflective phase, where its capability to provide answers to some of its core questions and validity of findings are under greater scrutiny (Fleetwood & Hesketh, 2008; Paauwe, 2009; Paauwe, Guest, & Wright, 2013; Patrick M. Wright, Gardner, Moynihan, & Allen, 2005). The business reality has also changed profoundly and can now be characterized by competition through business models, embracing the fast pace of technological change, which is unsettling the current people-machine equilibrium, and the need for continuous innovation. The key challenge for the HRM-performance debate in these new circumstances is how it should develop to continue to be influential for academic thinking and relevant for practitioners. Should it continue on the same trajectory or reorient to retain its current status?

For this paper we use the metaphor of the ancient Roman god Janus, the god of beginnings and transitions, who is usually depicted with two faces, one looking to the past and one to the future. We aim to offer a perspective on the future of the HRM-performance debate and its prospects for interaction with practice by evaluating the debate's state of the art and learning from its past.

Specifically, we performed an extensive cocitation analysis of the HRM-performance literature to examine the debate's intellectual structure, and organized a symposium along with a follow-up structured interaction (quasi Delphi) with some of the leading experts in the field to gather expert insight about the future. The panel consisted of highly influential scholars, whose work has both shaped the debate, as well as served as a strong channel for disseminating core ideas from HR research to practice (in alphabetical order): John E. Delery; David E. Guest; James P. Guthrie; Antony Hesketh; Mark Huselid; Paul Sparrow; and Patrick

M. Wright. In three rounds of structured interaction the panelists shared their views on: 1) the current state of the debate, 2) their (dis)agreement with other panelists' views, 3) potential new knowledge domains, and 4) prospects for the future of research-practice interaction, which were then coded, summarized and organized into text with our commentaries.

In the remainder of the paper we first present an overview of the cocitation analysis of the HRM-firm performance debate, and continue with experts' perspectives on the state of the art, major controversies and suggested future development of the debate. We conclude with the main findings in the form of plausible scenarios of future development of the debate.

THE INTELLECTUAL STRUCTURE OF THE HRM-PERFORMANCE DEBATE

Like every scholarly debate, the HRM-performance debate is founded on an intellectual structure – existing knowledge which it drew upon to inform the development of the debate (Mulkay, Gilbert, & Woolgar, 1975). Most HR scholars would agree that the debate was initiated in the early 1980s with the so called Michigan and Harvard models of human resource management, but fewer are aware that the origins of several components of the debate's intellectual structure go back to the 1960s, with the oldest single element of its intellectual structure going as far back to *The Wealth of Nations* (Smith, 1776).

To examine this rich cumulative intellectual structure of the HRM-performance debate we performed a cocitation analysis of the relevant literature (Small, 1973; Small & Griffith, 1974). With cocitation we refer to a link between two scientific outputs that occurs when they are cited together in the same document (White, 2011). The more times they are cited

together, the stronger the cocitation link between them. For example, because Huselid (1995) and Wright & McMahan (1992) both cited Barney (1991) *and* Jackson, Schuler, & Rivero (1989), we can establish that Barney (1991) and Jackson et al. (1989) are linked by (at least) two cocitations. A cocitation analysis allows us to explore how cocitations, which represent the basic building blocks of an intellectual structure of a scholarly debate/field, cluster into interpretable knowledge domains. We adopted the island procedure (De Nooy, Mrvar, & Batagelj, 2011) in order to explore cocitation patterns of over 60.000 scientific outputs, which informed 1.011 scientific articles contributing to the debate between 1985 and 2012. The procedure was used for its effectiveness in extracting and visualizing key components of intellectual structures from complex cocitation data. The results of the analysis, performed with Pajek software (Batagelj & Mrvar, 1996), are reported below and represented in Figures 1-3.

Starting with the cumulative intellectual structure of the HRM-performance debate up to the present, we can observe a vast, highly interconnected cluster of cocitations – the dominant knowledge domain – which accounts for most of the available knowledge landscape. The dominant domain can be detected in Figure 1 as an elevation with a broad foot and a high pillar in its center.

Insert Figure 1 here

Each coordinate in the landscape in Figure 1 represents a (potential) cocitation. The height of elevation indicates the intensity of cocitation. Cocitations that are interlinked (and of similar intensity) are clustered together in the landscape. The pillar consists of approximately 200 highly co-cited scientific outputs and peaks in a cocitation cluster consisting of links between Huselid (1995), Delery and Dotty (1996) and series of related papers (Arthur, 1994;

Becker & Gerhart, 1996; Delaney & Huselid, 1996; Delery & Doty, 1996). In Figure 2 we zoom-in into the cluster of 30 most intensively co-cited papers in the HRM-performance domain – the highest central part of the pillar in Figure 1. The papers that constitute this cluster were published between late 1980s and 2005 by mostly U.S. authors. The width of lines indicates the intensity of a cocitation tie. All cocitations links have above 50 ties.

Insert Figure 2 here

The immediate knowledge landscape around the pillar features scientific output, which has received significantly less attention than the contributions in the centre, but which still remains strongly interrelated with the pillar. At first sight, this representation of the debate's intellectual structure suggests a rather monolithic debate, informed by a considerable amount of highly overlapping knowledge (i.e., intense cocitation in this case implies similarity/complementarity of knowledge elements in the eyes of those who built on them). However, to truly understand what the debate builds on, we have to take a closer look into the dominant knowledge domain and its development.

A closer examination reveals that several salient knowledge domains, which are now seamlessly integrated into the dominant domain, have played a highly visible role in the development of the debate. Some of them have been present since its inception and remain an important foundation for developing new knowledge about HRM and organizational effectiveness today. The origin of a majority of these knowledge domains is external (i.e., they originate in important theoretical discussions about organizations and their functioning in general) and goes back three decades or more. The best representatives of this part of the debate's intellectual structure include: 1) *Resource based view and competitive advantage* (e.g., Barney, 1991; Miles & Snow, 1984; Wernerfelt, 1984), 2) *Social exchange theory and*

employee-organization linkages (e.g., Blau, 1964), 3) *Human capital theory* (e.g. G. S. Becker, 1964), 4) *Institutional theory* (e.g., DiMaggio & Powell, 1983), and 5) *Critical perspective* (on personnel management) (e.g., Legge, 2005). The importance of these domains and their positioning within the dominant domain fluctuated through time, but never ceased to exist.

Another important component of the intellectual structure, now part of the dominant domain, developed by means of integration of internal and external knowledge. Specifically, *Strategic HRM* domain emerged as a combination of seminal work on organizations (i.e., March, 1958; Weick, 1979; Williamson, 1975), early conceptualizations of strategy and HRM (e.g. Dyer, 1983; Schuler & Jackson, 1987; Wright & McMahan, 1992) and consideration of the link between organizational environment and HR practices (Jackson, Schuler, & Rivero, 1989). This knowledge domain became the conceptual backbone of the debate and laid foundations for further development of internally developed knowledge domains. The dominant knowledge domain was established in mid to late 1990s when the *Strategic HRM* domain merged with the rapidly developing *High Performance Work Systems* (HPWS) domain. Since then the dominant domain has been transforming incrementally but has retained its basic appearance.

There are only a small number of knowledge domains that have informed the debate so far without being part of the dominant domain. In Figure 1 these domains can be observed as low elevations (plateaus), separated from the central elevation. Even though they were not as extensively used in developing new knowledge about HRM and performance, they provided the debate with access to non-redundant knowledge (i.e., since researchers have not co-cited them with the elements from the dominant domain, we can infer that researchers considered them different from the dominant knowledge domain elements).

Specifically, they usually highlighted an alternative perspective, suggested new insights for studying the HRM-performance relationship or enhanced potential for interdisciplinary research. For example, *Social capital* knowledge domain (Adler & Kwon, 2002; Nahapiet & Ghoshal, 1998), introduced the idea of social capital and emphasized the need to consider how HRM contributes to shaping companies' internal and external social structures. Elaboration on these ideas later resulted in the conceptualization of relational archetypes by Kang et al. (2007) and refreshed the debate. Other representatives of these knowledge domains are: *HR competencies*, *Varieties of capitalism*, and *Regional HRM specifics* (i.e., HRM in India, China, and Europe,) domains. These domains are robust enough to remain in the cumulative intellectual structure of the debate, but there are also other domains that have complemented the dominant domain only temporarily (e.g., *Knowledge creation & learning*; *Prosocial behavior & OCB*; *Evolutionary theory of change*).

An additional view of the intellectual structure, which is particularly interesting if one wants to project future development of a scholarly debate, can be obtained if we examine only knowledge domains that were cited by recent contributions to the debate. Our co-citation analysis of what informed HRM-performance contributions from 2005 to 2012 showed that due to inertia the core of the dominant domain (i.e., the pillar) is relatively stable and thus similar to what has been shown for the cumulative case – the whole period from 1985-2012 (see Figure 2). However, what is different is that we can observe a proliferation of small new knowledge domains that are not part of the stable dominant domain, which are “competing” for attention. It is highly likely that at least some of these 81 new elements, which include *M&A*, *e-HRM*, *Talent management*, *Positive psychology* and other domains, that recently became part of the intellectual structure of the debate, will leave a significant trace.

Moreover, if we examine only those knowledge components in the dominant domain that emerged recently and inform recent contributions to the debate (see Figure 3), we can observe some interesting developments. These domains grow underneath the pillar, catching up the level of cocitations in the core of the pillar. In Figure 3, the width of lines indicates the intensity of a cocitation tie; cocitation ties have between 7 and 21 cocitations (compare with Figure 2). The overall observation is that the most recent knowledge cluster informing the debate predominately consists of review studies and critical reflections on the current methodological paradigm (e.g., Boselie, Dietz, & Boon, 2005; Wall & Wood, 2005; Patrick M. Wright, et al., 2005) and aggregate empirical studies (e.g., Combs, Liu, Hall, & Ketchen, 2006 meta study; Datta, Guthrie, & Wright, 2005 industry context study) and could thus be labeled as *Reflective* domain. This situation is probably developmental and tells us that researchers are citing these studies now because they are trying to address the challenges being put forward by these contributions. In addition, we can notice some other knowledge components that reveal the most recent interests of HR scholars: 1) *Employee perceptions of HR practices & HR attributions* (Kuvaas, 2008; Nishii, Lepak, & Schneider, 2008); 2) *Multi-level research* (Li-Yun, Aryee, & Law, 2007; Takeuchi, Chen, & Lepak, 2009); 3) *HR, Knowledge & learning* (Collins & Smith, 2006); 4) *Relational architecture & social structure* (Evans & Davis, 2005; Kang, et al., 2007); 5) *The role of line managers and HR implementation* (Purcell & Hutchinson, 2007); and still 6) *HPWS* (also exploring effects on alternative outcomes such as occupational safety) (Boxall & Macky, 2009; Macky & Boxall, 2007; Zacharatos, Barling, & Iverson, 2005).

Insert Figure 3 here

With this description of the current intellectual structure based on bibliographic data let us now turn to the expert insight of the state of the art of the debate by our panelists.

STATE OF THE ART IN THE EYES OF THE PANELISTS

As can be discerned from the co-citation analysis the important knowledge domains, which are also reflected in other overviews studies are in the area of the Resource Based View (RBV), Social Exchange, Human Capital, Institutional theory, Critical Theory and the Strategic HRM domain inclusive the so-called High Performance Work Systems. According to our panelists we see later on more attention to the process of implementation and the devolution of HRM-practices to line management, stimulated by amongst others Bowen and Ostroff (2004) and Wright and Nishii (2008) with their distinction between intended, implemented and perceived practices. This also implies more attention for employees. In the words of **Guest**: *'A more general implication of these current developments is that there is a recognised need for multiple respondents and a central place for employee views since they are key recipients of HRM'*. Related to this is an increased focus on context and concepts like organizational climate (cf. Schneider, Ehrhart, & Macey, 2013).

This has important implications for the kind of methodologies being used. Originally (1985-1995) the focus was mainly on organizational level asking key respondents (i.e., HR managers) to report on HRM practices being present and subsequently linking these to performance outcomes (preferably based on objective indicators for organizational performance). Today researchers increasingly make more use of multilevel research designs, as they include both organizational level data as well as individual level data. Needless to say that this is paralleled by increased complexity and statistical refinement, which also has to

do, according to **Guest**: *'...with the incorporation of ideas from organizational and social psychology, reflected for example in the research on attributions.'*

According to **Wright**, in the last 10 years two developments seemed to emerge: *'First, researchers began to critique the methodological paradigms that had emerged, particularly focusing on the lack of reliability of single respondent studies (Gerhart, Wright, Mc Mahan, & Snell, 2000) and the causal direction (Wall & Wood, 2005; Patrick M. Wright, et al., 2005). Second, the research that had been largely dominated by U.S. researchers began to become more integrated with research coming from the U.K. and Europe. This non-U.S. work had existed for some time, but U.S. researchers began to get acquainted with these literatures and integrate them into their own work'*. This can be observed also in the intellectual structure described above by the presence of Guest's work in the dominant domain and by the fact that several of the knowledge domains outside of the dominant domain are regionally oriented.

However, bundling of HRM practices in so-called high performance or high involvement work systems (Boxall & Macky, 2009) remains important. **Huselid** especially emphasizes the importance of HPWS as being representative for the present state of the art: *'an increasing body of literature argues that the adoption of what have come to be known as High Performance Work Systems or HPWS (including comprehensive employee recruitment and selection procedures, incentive compensation and performance management systems, and extensive employee involvement and training) can improve the knowledge, skills, and abilities of the firm's current and potential employees, increase motivation, reduce shirking, and enhance retention of quality employees while encouraging nonperformers to leave the firm'*. Several meta-studies confirm the systems effect above the effect of individual HRM

practices (see for an overview Combs, et al., 2006; Jiang, Lepak, Hu, & Baer, 2012; Subramony, 2009).

Also typical for the present state of the art is the search for conceptual clarity - what do we mean by HRM, by HRM practices, by HR systems, and what are the kind of mechanisms, which explain the link between HRM and Performance at various levels of analysis? In the words of **Delery**: *'Recent work has attempted to better define the conceptual logic of the domain (Lepak, Liao, Chung, & Harden, 2006; Lepak & Snell, 1999) and empirically test this logic (e.g. Takeuchi, Lepak, Wang, & Takeuchi, 2007; Patrick M. Wright, et al., 2005)'*.

We also see more attention given to the important role of mediating mechanisms, such as human capital, motivation, opportunity to participate and the role of climate. **Guthrie** considers the "mediational" domain as the most important current domain. He explains: *'this stream of work (e.g., by scholars such as Collins & Smith; Sun et al., Takeuchi et al.) has focused on understanding the processes and mediators (e.g., knowledge creation, citizenship behavior) linking HR policies and practices with organizational outcomes'*.

Reflecting on the state of the art of the field **Guthrie** also draws our attention to the so-called *'critical camp, comprised of both methodological critics (e.g., Gerhart, Wright, Kaufman, etc.) and critics who propose both ideological and methodological criticisms (e.g., Wall & Wood; Legge, etc.)*. *The critical domain questions whether "high performance" HR systems are (1) really as good for firm performance as espoused and (2) whether employees working under these systems find them enjoyable as opposed to stressful and dissatisfying*. Nowadays, it has become quite common to look for both positive as well as negative effects of HPWS on well-being (see for an overview Van De Voorde, Paauwe, & Van Veldhoven, 2012).

Hesketh indicates some striking characteristics of the present state of the art. First of all, he observes *'an apparent lack of clustering around methodological reflection'*. A second point in his view revolves around how far our understanding of HR's role in unlocking financial performance can be developed by *'simply doing more empirical work with the same ontological, epistemological and meta-theoretical assumptions imported from a largely "scientistic" meta-theory'*. Third, examining the results of the co-citation analysis, which results in a number of knowledge clusters *'does not constitute their epistemological justification but reveals particular orders of discourse within the management research community'*. He claims that it is evident that researchers in our field, as in others, *'clearly hunt in packs'* and warns that *"safety does not come from numbers, both in the plural and quantitative form!"*

Finally, **Sparrow** has doubts whether when talking about HRM-performance debate we can *"really argue for a single field"* because *HRM-performance research area is so diverse and is making use of established debates elsewhere (like strategy, RBV, I/O psychology)'*. In his view, recent research *'seems to have become totally a-contextual, a-theoretical, and removed from the trajectory signaled by within-firm practices'*. This is, according to Sparrow, reflected in studies that focus on very generic bundles of HR practices and equally a-contextual generic organizational performance outcomes. He argues that in the need/search for strong theoretical foundations HRM-performance research has *"focused on the wrong end of the chain"* (i.e., starting with bundles of HR practices and then assuming generic links to generic organizational performance outcomes).

PERSPECTIVES ON THE FUTURE OF THE HRM-PERFORMANCE DEBATE

We now organize our panelists' views on future of the debate. As we wanted to extend our bibliographic analysis and experts' insights about the state of the art with a projection of the future, we followed up with panelists to gather opinions on the future development of the debate. We were interested in both their general view on future development of the debate as well as in their perspectives about specific knowledge domains that could move the debate forward. Panelists were also given the chance to discuss where they agreed and disagreed with their colleagues, which allowed us to establish major commonalities and controversies about the future of the debate. Although panelists emphasized very diverse perspectives of the HRM-performance debate, as a group they provided a very comprehensive view of the future.

In the short run they do not expect dramatic changes in the way HRM-performance research is done, nor the research questions that are being asked. There is some support among them for going along with **Huselid**, who argued that future research examining HRM-performance link *"should follow dramatic changes in the business reality, specifically towards addressing increasing complexity, while at the same time trying to provide better and more convincing answers to core questions"*. Thereby, more fundamental changes in the development of the debate seem only possible in the longer run. In the words of **Sparrow** *"slowly, and perhaps more reactively, (I think very slowly) the academic institutions, research outlets and research centres and groupings will start to echo the problem-solving configurations <a new approach of doing HRM research, where HRM contributes to solving important societal questions>"*.

Most of the main directions mentioned in the state of the art description will still be pursued in the near future. First, utilization of ideas from organizational psychology, organizational behavior and other fundamental social science issue will continue to attract a considerable

share of contributions to the debate and ensure that it retains its link to micro research.

Guest lists *“the exchange theory allied to the psychological contract and perceived organizational support and information/signaling processes”* as examples of such ideas and argues *“these will be linked to a body of research on the role of line managers (and others) in implementing HRM”*. He also anticipates *“more research on missing variables such as culture and leadership that may account for both HRM and performance”*. In addition, **Sparrow** adds psychological contract, happiness and fairness to complete the list of suggestions of variables for future research.

Second, it is also expected that the debate will continue to strive for a greater conceptual clarity (Chadwick, 2010; Gerhart, 2012). **Hesketh** observes that there are *“problems on both sides of the equation”*. That is, in *“identifying the accurate independent variables – those factors in managing people which release out performance in the workforce,”* but also in how we conceptualize and measure performance. There is a lack of consensus about what to use as a measure of performance in research. According to **Hesketh** some of the measures *“rest on conventional accounting variables... but the majority use other constructs as proxies for the quantum form. These range from the ratings on ordinal scales by supervisors of their direct reports, or by HR executives of the performance of the individual processes comprising the major activities of their function.”* So to make progress in understanding the HRM-performance link we would first need to achieve a greater consensus on how to measure both sides of the equation. **Guest** also sees *“a strong need for clarification about the nature of HRM and what to measure as the independent variable (i.e., what we mean by, and how best to measure HRM)”*. However, he is not optimistic that in the near future *“a consensus on this will emerge”* and sees this as a major bottleneck for future development of the debate.

Third, there seems to be some agreement among the panelists (i.e., comments by **Sparrow**, **Guest**, **Delery**) that the future development of models should start with the dependent variables. **Sparrow** notes that it were more appropriate if our research “would start with the intended performance outcome and then *‘reverse engineer how such performance is in any way dependent upon a collection of HR practices’*. The focus on outcome variables would help to improve the value added of our research in several ways. First, it would enhance the practical business relevance of the research on the HRM-performance link. In addition, it would demand inclusion of other relevant factors (which are currently not part of our models) and thus increase the need for reaching across boundaries of the debate. And finally, it would certainly broaden the range of outcomes we explore. For example, **Guest** suggests that this will be *“partly stimulated by a broadening of the research contexts away from settings where public financial returns are the key outcome.”* He also sees a second cluster of research, which *“is likely to seek the conditions under which HRM promotes the happy productive worker.”* **Sparrow** nicely summarizes this issue: by encouraging the field to move towards *“understanding how HRM contributes to constellations of performance <clusters of relevant and interdependent performance indicators>.”*

The change of emphasis towards outcomes also implies greater sophistication, most probably in the form of better multi-level models. Several members of the panel expressed support for more multi-level modeling, but at the same time felt that much needs to be done in the area of theorizing at multiple levels, which remains challenging. **Delery** nicely summarizes this thinking: *“The biggest challenges will be to develop better truly multi-level theoretical models. Right now, most of this work may test something at one level and something else at another level, or simply use logic of one level to test something at a different level. What will lead to more important advances is when we start truly thinking*

*about how something at one level creates a different construct at a different level. This will involve more complex thinking and methodology. Simply averaging individual responses and calling it a group response will not suffice.” This development will require “cross-discipline knowledge and citation (**Sparrow**)” as well as “a better understanding of the context (**Guest**)” and may accordingly result in much more complex models.*

Two trends emphasized by our panelists depart from the current state of the art. First, although strongly aware of the current importance of the High Performance Work Systems and their appeal to practice, **Huselid** suggests an upgrade of the best practice model, which might generate many new interesting research questions. For the field to maintain both its theoretical and practical relevance, *“it must move beyond a “best practice” model to a “differentiated workforce investment” model. This means we must develop a better understanding of strategic work and strategic jobs. A differentiated workforce strategy – focused on placing greater investments in those jobs which create the most wealth – represents a significant conceptual shift for both academics and practitioners.... The key takeaways are that firms need to invest in talent based on 1) performance variability, and 2) strategic impact – not based on hierarchy or external labor market scarcity. “*

Second, **Wright** also suggests a departure from the current mainstream focus on HR practices. However, he argues for a renewed focus on human capital as a resource and simultaneous reduction of interest in studying HR practices *per se* (cf. Wright, Coff, & Moliterno, 2014). He argues *“The future trends in SHRM will be characterized by 'Back to the Future'. By this I mean that I see two trends that will simply be rediscovering some of the early areas of emphasis. First, researchers have seemingly rediscovered the human resources in S”HR”M. While HR practices and performance research will march onward, more emphasis*

will focus on human capital. Second, this emphasis will lead to a remarriage of strategy and HR researchers (as evidenced in part by the Strategic Management Society's new "Strategic Human Capital Interest Group")".

Remaining controversies

What will be the remaining controversies? The role of human capital and RBV seems to be the focal point of a major disagreement among our panelists. **Guest** opposes **Wright's** call for a renewed focus on human capital and hence also predicts a decline in the future importance of RBV *"in the face of criticism about its limitations"*. His rationale for avoiding the emphasis on human capital is the following *"The simple reason is that if the term (i.e., human capital) is properly defined, it emphasizes the A in the AMO model and this is too narrow. The temptation may be to broaden it to include the other elements, but then it loses any distinctive meaning as human capital, so any such colonization is inappropriate. It also risks distracting attention away from the M and O."*

Delery joins **Wright** in seeing continuation of the RBV as a dominant perspective informing the debate. He understands the position of RBV's opponents and acknowledges that *"Other meta-theories relating HRM practices, human capital, and outcomes may be useful when examining effects on alternative outcomes such as satisfaction, commitment, and retention/turnover. However, when the relationship between human capital, the management of human capital, and firm financial performance is in question, "RBV still provides a better overall framework."*

For **Sparrow** the issue of the role and importance of human capital and RBV raised by his colleagues is important, because he expects that *“many more forms of capital will be given more consideration – social capital (as it relates to being connected to appropriate and unbiased sources of information and knowledge), intellectual capital (as it relates to an understanding of business models and their organizational consequences), and reputational/political (as it relates to impact and levels of followership, be that in formal or even social media).”* **Delery** makes a related remark by saying that *“if we continue to use firm performance as the ultimate dependent variable, we will have to acknowledge the larger work in strategy, where human capital is just one of a number of causes of firm performance.”*

The main cause of disagreement lies in the level of abstraction and breadth at which human capital is conceptualized. **Guest** rejects the appropriateness of use of the human capital concept from the micro view, whereas **Wright** and **Delery** conceptualize it at a higher level of abstraction. Thus, more work on how human capital can be conceptualized and viewed at multiple levels could help to bring these positions closer together (cf. Ployhart & Moliterno, 2011). In the words of **Delery**, who sees a promise in the literature attempting to define, measure, and study human capital at all levels in the firm: *“Understanding how individual skill, motivation, and performance combines to influence group, establishment, and higher level performance will be important for the field.”*

Although recently examining the black box or the mediational domain as labeled by **Guthrie** has been very popular there is doubt about its ultimate viability. While agreeing that mediators will play an important role in future research **Sparrow** wonders if *“we shall ever be able to have certainty over the role and strength of a wide set of mediators, and their*

causal sequence, as it were.” He argues that “to understand mediation we need to have a view of the causal sequence of antecedent and consequent variables.” And goes on explaining that “human psychology, especially in rapidly changing contexts, seems to be too complex for a single sequential model to apply...For some employee segments one set of mediators might apply, yet for another segment the mediator has no agency, or there is a system shock (historical event) and everyone suddenly gives attention to a previously switched-off mediator”.

This discussion illustrates the role of and need for meta-analyses and aggregate empirical studies in the HRM-performance debate. Looking back we can see that such contributions have played a very important role (e.g., Combs et al., 2006; Datta et al., 2005; Jiang et al., 2012) in the development of the debate. However, it also raises the question if we will be able to continue aggregating more complex studies, which are expected in the future. If the ability for aggregation of individual studies and consequently interpretation of aggregate findings is lost, we will not be able to see the big picture anymore. It is reasonable to expect that no individual study is expected to be able to do this in the future.

Although the vast majority of the panel agrees that the HRM-performance debate has ultimate practical business relevance, some also expressed their doubts. **Hesketh** disagrees with other panelists in “that all is well within the field and tweaking the deductive and largely positivistic framework from within is where its future lies.” He believes that the state of the debate could be described by “We think we now know what we thought we already knew back in 1985” and thus questions its ultimate practical relevance. To illustrate his critique he uses the example of a frequently used meta-analysis study by Combs and colleagues (Combs et al., 2006), which revealed that the performance of the human

resources function could account for 4.6% of the variation in the return on assets between companies. In his view the “4.6% percent return is more indicative of our not having yet fully quantified the way in which HR unlocks performance, or how we even begin to understand it.” He goes on to argue that “in practical business terms, such a small return has rather led to executives rejecting further investment in HR” than being convinced that HRM matters.

Finally, as there were several suggestions that if the debate moves towards more complexity and statistical sophistication the question arises if this is sustainable and where it will bring us. **Guest** illustrates this issue: *“I suspect there will be a growing concern that statistical sophistication rules at the expense of meaning. This, combined with the difficulty of getting good research access, may shut out many potential researchers. When the choice of control variable can make a lot of difference to the outcome, it can be difficult to have a lot of confidence in the results of complex studies. This means we will need to be more open to alternative methodologies (<ontologies>).”* **Guest** and **Sparrow** suggest that carefully designed case studies could help in complementing increasingly complex research designs by providing more holistic and in-depth understanding of the relationship between HRM and organizational effectiveness. **Sparrow** adds that *“how we police the relative contribution of the two (i.e., increasingly complex quantitative and qualitative research) will be a challenge for the field. That said, access to data may still represent a bottleneck for further development and more intense cooperation with practitioners might be needed to eliminate it”*.

Vision of a future model of the HRM-performance debate

We now turn to perspectives on how more profound changes could be made in the development of the HRM-performance debate. One way is the emergence or integration of

new unexpected knowledge domains. **Guthrie** keeps an open mind to unexpected, unforeseen developments and events. He explains that *“although careful, planned study plays a critical role, as with evolutionary development, randomness or happenstance also affects the creation of knowledge and knowledge domains. Conversations between diverse camps and the “collision” of ideas may take the debate and literature in unforeseen, unpredictable directions.”* As we have learned from the past in the HRM-performance and other scholarly debates, these unexpected developments or events could trigger the HRM-performance debate into new directions.

The critical element here is collision of ideas, because the HRM-performance debate was built on compromise and inclusiveness with a relative lack of serious collisions of ideas in the past. **Hesketh** makes an interesting observation about the agreement among authors contributing to the debate: *“Different <academic> packs – be they ‘strategic HRM’, ‘contingency theory’, ‘institutional theory’, ‘high commitment systems’, the ‘resource-based view... have come and gone. But the overarching paradigm in which they all operate remains the same: if the explanandum is to be measured in the calculable quantity of finance, then it seems to follow that the only effective way in which to capture the explanans is also via the mechanics of calculation.”* Is it reasonable then to expect a serious enough collision of ideas in the future? Or is it rather that the magnitude of numerous small collisions of newly emerging ideas will suffice to bring the debate to a new level?

Another line of thought argues that we do not need to do very much in the area of content (i.e., the composition of the intellectual structure) but rather in the overall approach to research on the role of HRM in organizational effectiveness. If the move towards greater emphasis on outcomes is the first step in changing the way our field operates, moving

towards a problem-based approach is a much more profound one. As **Sparrow** explains, it might hold for a more distal future that *“HRM-type thinking gets subsumed into other disciplines...rather than HRM academics try and subsume the other disciplines within their own field. We shall (all) be working on complex organizational and societal issues and problems – or at least the financial resources and access opportunities will start to gravitate towards these “questions of the day”. As a consequence, we in HR shall compete for whose insights should be part of the academic study process.”* Following this logic HRM might lose its appeal as an independent variable and become a more prominent contextual variable for explaining how core business processes result in selected outcomes. This research approach could also be one way to address and cope with the issue of methodological homogeneity. **Hesketh** reminds us that we should keep trying to *“breakout of our current methodological approaches to engage with alternative ontologies in order to create new perspectives on performance in general and “the people contribution” in particular.”*

The decision for a particular research initiative, which at the end of the day shapes the intellectual landscape of the debate, is still in the hands of individual researchers, who are more or less embedded in the community of the HRM-performance researchers. Our panelists believe that we will continue to see specialization and diversity of various methods that researchers use. **Delery** commented on the discussion of the panel in the following way: *“All of these <future research> directions are likely worthwhile research endeavors, though I do not see any one researcher going in all of the directions. As researchers, we focus on our particular interests and work often in our “comfort zone.” This will lead some of us to continue large scale empirical studies, while others continue exploring less positivistic research methodologies. There will be no one accepted framework or methodology for studying the relationship between HRM (including practices, policies, strategies, and human*

capital itself) and firm performance. All of this is good for the field". Wright identifies another trend, which was not characteristic for earlier development of the debate and could have an important impact on further development and that is "an increased integration of the global research through increased dialogues and partnerships among researchers across the globe." If this integration will build on diversity of ideas and pluralism, then we can expect stronger development of the debate.

IMPLICATIONS FOR THE RESEARCH-PRACTICE LINK

Finally, we explore views on the link between academic research and the relevance for HR practitioners and the wider business community—the research-practice link or gap, which have been extensively discussed in HRM research in general (Rynes, 2012). This is understandable as the HRM-performance debate has had *"broad appeal and has become widely cited because it addresses issues of importance to both academics and practitioners – that is, there has been a focus not only on theoretical and statistical significance but also on managerial relevance (Huselid)"*.

Hesketh agrees that the debate has had a broad appeal among practitioners, but questions its actionability. He wonders *"why the majority of executives (still) do not turn to either their own HR function, or less still, the Academy, for answers to what are the big issues facing post-Lehman capitalism. For example, how people simultaneously drive and constitute risk, in all its various forms."* He also questions whether the new trend in human capital analytics is the right direction to strengthen the research-practice link. *"What I can predict, is that predictive analytics and the "decision sciences" of people will in the future merge in to what can only be best described as the enthusiastic, but ultimately flawed cul-de-sac of "big data".*

The problem with predictions is that they cannot be wrong. But, retrospectively, they are rarely right."

Guest also has doubts about the HRM research-practice link. He fears that the *"emerging trend in the debate is towards complexity and contingency and that there is an absence of »warm« marketable ideas or clear links between policy, practice and attractive outcomes, which will all make knowledge produced less accessible to practitioners and be perceived as less relevant by them"*.

In sum, the panelists agree that it is essential for the research-practice link and relevance of the debate to have translators of essential findings in the debate, who are skilled at communicating with practitioners. **Delery**, who also believes that research on HRM-performance will continue to have implications for businesses, illustrates the dissemination of knowledge by for example Lepak and Snell's (1999) work on HR architecture, which has been successfully translated into practitioner audience targeted book "The Differentiated Workforce" by Becker, Huselid, and Beatty (2009). From his response we could also sense that the dissemination and production of more actionable knowledge for practice could be stronger if incentive systems in academia changed in the direction of more credits for translating/presenting research outcomes in understandable terms to society at large.

Delery also opens up another important issue, which could contribute to more relevance of the knowledge produced – the collaboration between academics and practitioners. He argues that *"for empirical work to continue in this field, it will be essential to develop strong relationships with HR professionals, especially due to the need for more complex and fine-grained data. Thus, "large scale <complex> empirical work, <which will be needed for next*

steps> will necessitate cooperation of practitioners and will need to be seen by firm leaders as potentially helpful to the firm.”

CONCLUSION

Our co-citation analysis offered an overview of the intellectual structure of the HRM-performance debate. It drew our attention to the dominant central pillar in the knowledge landscape built upon or preferably said *stimulated* by the 1) Resource based view, 2) Social exchange theory, 3) Human capital theory, 4) Institutional theory, and 5) the Critical perspective. The dominant knowledge domain became especially well established in the mid to late 1990s, when Strategic HRM merged with the High Performance Work Systems domain. In this way, it became a solid foundation -the conceptual backbone- of our field.

Numerous new domains (e.g., e-HRM, Talent Management, M&A, positive psychology) are entering the debate at the moment without substantially changing the core domain of strategic HRM, but without doubt in due course some will make an impact. The most salient recent knowledge domain informing the debate consists predominately of review studies, meta-analyses and critical reflections on the current methodological paradigms, which was also an important theme among our panelists. This Reflective domain, as we have called it, is probably typical for the stage in the life cycle our debate is currently at. It's like taking a break, reflecting on what has been achieved and then moving forward. However, moving forward in which direction?

As we have described and discussed unanimity is not yet on the horizon. Some of our panelists suggest that going “back to future” and rediscovering the importance of human capital instead of focusing exclusively on HR practices would be the best direction to move the debate further, others hint at the importance of focusing more on the kind of outcomes we want to achieve (so a shift from the emphasis on the independent variable-being HR practices or HPWS bundles- to the dependent variables with a variety of possible outcomes), still others refer to the importance of HRM research contributing to the solving of real live business and societal issues. A hindrance for most of these directions might be the increased need for sophistication of HRM research, which increasingly demands multi-level modeling. These statistical analytical techniques have the tendency to overrule the need for multi-level theorizing, which of course should precede the analysis of data. Agreement across the board is that we need greater conceptual clarity, more qualitative work and alternative ontologies in order to create new avenues for the firmly established dominant knowledge domain of (strategic) HRM and performance.

Finally, a sincere word of thanks to our panelists, who cooperated with us in this endeavor to take the field forward by reflecting on the past and sharing glimpses of the nearby and more distant future of the debate.

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Figure 1: The HRM-performance knowledge (domain) landscape

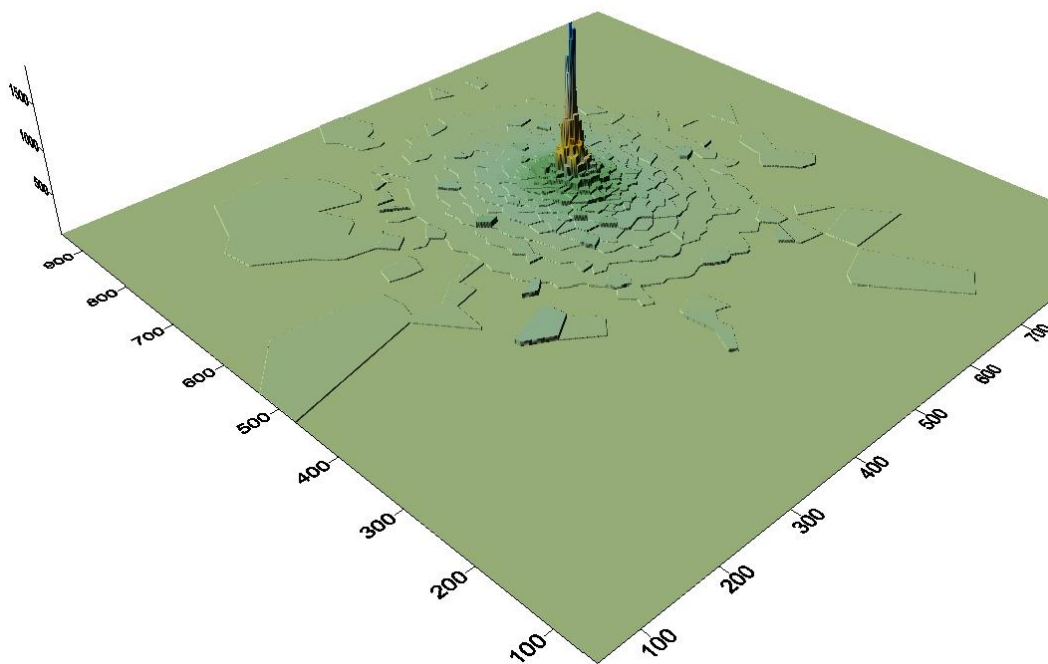
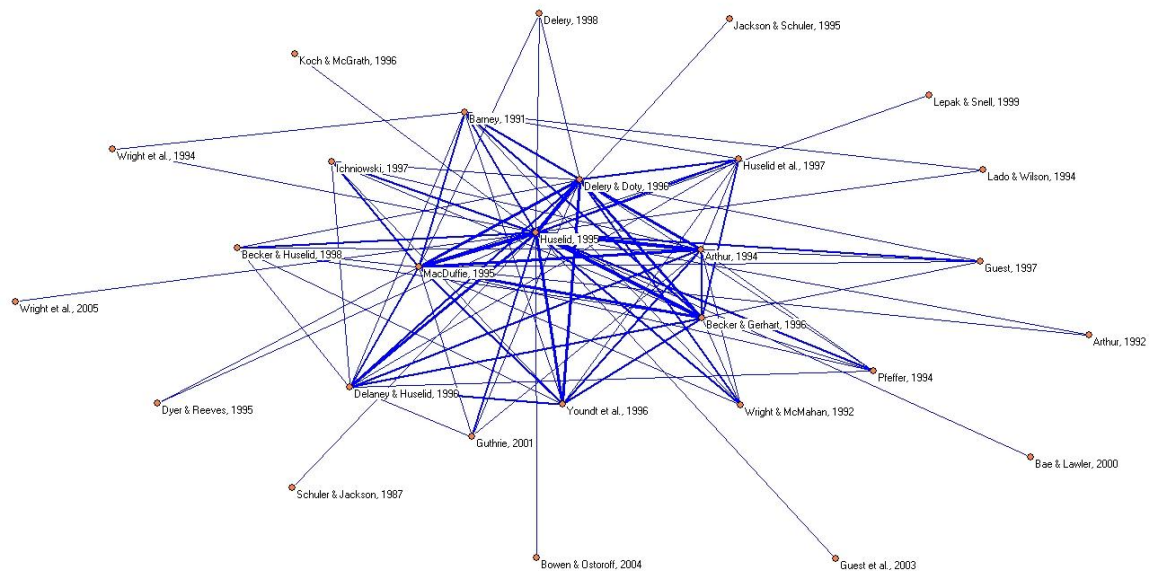
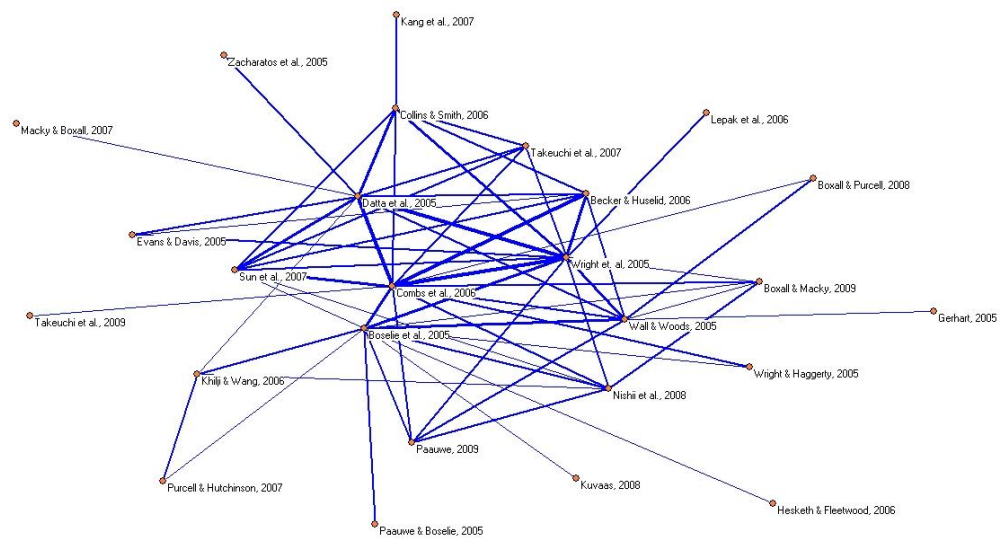


Figure 2: A zoom-in into the dominant domain of the HRM-performance debate²



² References for labels used in Figure 2 are available as supplementary material.

Figure 3: Recently emerging knowledge clusters that are part of the dominant domain³



³ References for labels used in Figure 3 are available as supplementary material.