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Human resource management and performance: still searching for some answers

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Over the past 20 years, there has been a considerable expansion in theory and research about human resource management and performance. This paper reviews progress by identifying a series of phases in the development of relevant theory and research. It then sets out a number of challenges for the future on issues of theory, management processes and research methodology. The main conclusion from the review is that after over two decades of extensive research, we are still unable to answer core questions about the relationship between human resource management and performance. This is largely attributed to the limited amount of research that is longitudinal and has been able to address the linkages between HRM and performance and to study the management of HR implementation.
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INTRODUCTION

When the *Human Resource Management Journal* was launched in 1990, it was able to capture a rising wave of interest in human resource management and in particular the relationship between human resource management (HRM) and performance. The following decade provided the initial sound evidence about a positive association between HRM and firm performance (see, *e.g.* Arthur, 1994; Huselid, 1995; Delery and Doty, 1996). A decade later, the number of studies had grown to such an extent that two major reviews of the research (Boselie *et al*., 2005; Combs *et al*., 2006) confirmed that the large majority of published studies demonstrated an association between HRM and performance; but both also emphasised that their analysis provided evidence of an association rather than causation. Both reviews also concluded that there was insufficient evidence to explain why there was an association. It is this lack of an explanation that provides the point of departure for this paper. Its aim is to provide an analysis of how after 20 years of extensive research we are more knowledgeable but not much wiser, in that we have not been able to explain the demonstrated association between HRM and performance with any conviction, and to outline possible lines for developing research that might provide some answers.

While it is always easy to be critical of developments in a complex area of organisational research, it is also important to acknowledge the considerable progress that has been made in research on HRM and performance. This is a rapidly evolving field that has gone through a fairly typical series of steps towards a perhaps wiser and more reflective perspective on the subject (see, *e.g.* Paauwe, 2009). I will argue that in retrospect several overlapping phases can be identified, reflecting the ebb and flow of theory, research and application and it is useful briefly to consider these, both to recognise the progress that has been made and to highlight the distinctive challenges for future research that each reflects.

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The beginnings

The first phase in the development of theory and research on the association between HRM and performance occurred in the 1980s. In this period a series of articles and books by, for example, Fombrun *et al*. (1984) and Miles and Snow (1984) began to link business strategy to human resource management. Others such as Walton (1985), writing from more of an organisational behaviour perspective had highlighted the need for a shift from control to commitment as the basis for management of people at work. In the work of the Harvard group (Beer *et al*., 1984) and Schuler and Jackson (1987) we began to see an integration between a strategic view, that highlighted the importance of 'external fit' and organizational behaviour with its focus on 'internal fit' to produce a conceptual perspective or 'map' (Noon, 1992) that forms the basis of contemporary HRM. In the UK, this was contrasted with the pluralism of an industrial relations perspective to highlight a distinctively normative perspective on HRM (see, *e.g*. Guest, 1987 and Storey, 1992). In parallel with this, Foulkes (1980) and Peters and Waterman (1982), among others, had provided glimpses of evidence about successful organisations that seemed to apply the 'high commitment' HRM principles. This first phase, therefore presented the promise of HRM in the form of semi-prescriptive analytic frameworks alongside somewhat anecdotal cases that appeared to confirm this promise of an association between HRM and performance.

Empiricism

The second distinctive phase occurred in the 1990s when the first set of survey-based, statistically analysed studies of HRM and performance began to appear. The seminal paper was by Huselid (1995) but equally useful sector specific research was reported by Arthur (1994) and by Ichniowski *et al*. (1997) in steel mills, by MacDuffie (1995) in the auto industry and by Delery and Doty (1996) in banking. All indicated that the adoption of more HR practices was associated with higher performance and in so doing began to provide an evidence-base for the claim about a link.

Backlash and reflection

A backlash swiftly followed as it was recognised that the rush to empiricism had occurred at the expense of sufficient consideration of some key conceptual issues, and a third phase emerged. Articles by Dyer and Reeves (1995) and by Becker and Gerhart (1996) demonstrated that the published studies used an array of different HR practices, measured in different ways. There were similar though less strongly voiced concerns about the measures of performance. The implication was that we needed a sounder conceptual basis for determining the appropriate HR practices and needed a serious debate about generalisability highlighted in the discussion of universalist, contingency and configurational perspectives (Becker and Gerhart, 1996; Delery and Doty, 1996).

A rather different kind of backlash had been developing among a number of critics, particularly in the UK, who viewed the emergence of HRM in general and of a normative high commitment approach in particular as a new and insidious form of exploitation of workers (see, *e.g*. Blyton and Turnbull, 1992; Legge, 1995; Keenoy, 1997), a view that still persists in certain quarters (Hesketh and Fleetwood, 2006). The response to the concerns about the need for greater conceptual clarity and worker exploitation ushered in two further streams of conceptual and empirical work that overlap chronologically but which I will describe as phases four and five.

Conceptual refinement

The fourth phase focussed on conceptual refinement. Guest (1997) had argued that we needed a better theory about HR practices, about outcomes and about the link between them. Both he and Becker *et al*. (1997) had advocated an approach that drew on expectancy theory (Vroom, 1964; Lawler, 1971) as a basis for determining core elements of HRM content and its link to performance. Variants of this approach were subsequently widely adopted in the work of Appelbaum and colleagues in the US (Appelbaum *et al*., 2000) and by Purcell and colleagues in the UK (Purcell and Hutchinson, 2007) in what has come to be known as the Ability, Motivation, Opportunity (AMO) model. This approach represents an essentially universalist perspective.

A rather different approach emerged out of the theoretical work of Barney (1991) who had presented the resource-based view of the firm. This has been developed and applied to HRM by others (see, *e.g*. Wright *et al*., 1994; Lepak and Snell, 1999; 2002) as a basis for focusing investment in human resources and predicting the types of investment most likely to result in superior performance and competitive advantage.

A third shift in emphasis was presented by Paauwe (2004) who highlighted the importance of an institutional perspective and reminded us that in Europe the legislative framework as well as the institutions relating to education and training and to employee representation ensured that a minimum set of HR practices were in place in most organisations. This meant that any competitive advantage was more likely to come either from the way in which these standard practices are implemented or from the use of additional distinctive practices. The refinements that emerged from this period arguably continue to dominate our conceptual thinking about HRM and performance today and are reflected, for example, in the burgeoning interest in human capital.

Bringing the worker centre-stage

The fifth and overlapping phase in the development of research that has emerged as more central in the past decade concerns the key role of workers and the importance of workers' perceptions and behaviour in understanding the relationship between HRM and performance. The workers' voice had been surprisingly neglected in some of the early research and its absence had been cited as a major issue in some of the more critical writing about HRM. More generally, there was a frequent call to 'open the black box' to explore the process linking HRM and performance and this invariably led to a greater focus on the way in which workers responded to HRM (see, *e.g*. Wright and Boswell, 2002). In recent years therefore this neglect has been remedied. In the UK, the inclusion of a survey of employees in the Workplace Employment Relations Surveys of 1998 and 2004 provided a large national data base with which to study workers' perceptions. Some initial interpretations of these results led to a claim that HRM might not be in workers' best interests (Ramsay *et al*., 2000). In the North America, some surveys critical of HRM on the basis of workers responses began to emerge (see, *e.g*. Cappelli and Neumark, 2001; Godard, 2004) giving greater credence to the view that any gains in performance from HRM might be at the expense of workers rather than because of their positive reaction to it. Subsequent research has offered a more nuanced view. One result has been to focus on worker outcomes as a central issue in HRM and to consideration of how far it is feasible for HRM to result in both higher performance and enhanced workers' well-being (Peccei, 2004).

Growing sophistication

The final, in the sense of the most recent phase of development of theory and research on HRM and performance can be defined in terms of growing sophistication and complexity. This includes increased sophistication in particular with respect to theory and research methods and it provides a stepping off point for considering future developments. An important starting point for consideration of both sophistication and complexity is the work of Bowen and Ostroff (Ostroff and Bowen, 2000; Bowen and Ostroff, 2004). In their first paper (Ostroff and Bowen, 2000), they presented a conceptual multi-level framework suggesting that the HRM – performance relationship could be considered at both the individual and organisational levels and through the linkages between them. The second paper (Bowen and Ostroff, 2004) offered a theory of HRM implementation. In this paper, they suggested that to understand the HRM – performance linkages, much of the emphasis needed to be placed on processes, reflecting the oft-cited view that it is not enough to have good practices if they are not properly implemented. What this does is switch the focus to line management and places greater emphasis on HRM effectiveness.

A further indication of growing sophistication is the use of models from organisational and social psychology. An interesting reflection of this can be found in the work of Nishii *et al*. (2008) who argue that it is not just the presence of practices that is important but perceptions about the intentions behind the practices. They have used attribution theory to explore the way in which workers interpret practices and to show how this interpretation can shape their response. This is further confirmation that it is not sufficient to restrict our focus to the presence of practices and it points to the need for organisations to pay much more attention to how they communicate the purpose as well as the content of HR practices.

Bowen and Ostroff set out the case for a 'strong' HRM climate and HR system and researchers are acknowledging that we gain a limited picture by collecting information on HR practices without paying full attention to the context within which they are enacted. The need to link organisation, unit, group and individual level climates, inputs and outcomes requires a complex research methodology and sophisticated statistical analysis. The use of structural equation modelling, with the discipline required to specify linkages, has become commonplace in HRM research. We are beginning to see more multi-level analysis and where this is allied to longitudinal data, we are entering a more sophisticated level of methodology. At the same time, it makes researching much more difficult, particularly if the aim is to research across rather than within organisations, since gaining a sufficient number of organisations for multi-level analysis with detailed information on internal climates, is daunting. Recognition of the requirements this presents has led to an optimistic call for 'big research' (Wall and Wood, 2005) and the associated funding.

A methodological issue that continues to be debated concerns who should provide information about HRM. There have been persistent calls for multiple informants about the presence of practices (Gerhart *et al*., 2000). It has been suggested that, particularly in the context of large organisations, senior HR managers are not very reliable informants and that it is more sensible to seek information from those experiencing the practices, namely workers. Bowen and Ostroff suggest that consensus about HR practices is one indication of a 'strong' HRM climate and Liao *et al*. (2009) provide some empirical support for this view.

If the contemporary picture of HRM is one of growing complexity and sophistication, it also implies a risk of fragmentation. By raising the bar in terms of complexity of research methods and statistical analysis, a growing number of researchers may feel excluded from the field. There is also a risk that the research ceases to be accessible to practitioners and policy makers. This highlights a need for regular integrative reviews. However, it also neglects the many areas of potential research on HRM and performance that are still amenable to a variety of quantitative and qualitative research methods.

This paper has reviewed some of the key stages in the development of theory and research from 20 years of exploration of the relationship between HRM and performance as a basis for highlighting where future priorities might lie. It is most unlikely that funding for a single big study will emerge; but the experience of 20 years has made us more aware of some of the avenues that are likely to be more or less fruitful. For example, the core research question has been somewhat refined over the two decades. It started out as 'What impact does HRM have on performance?' It has extended in two directions. The first is the contingency question: 'Under what circumstances does HRM have an impact on performance?' The second concerns process and asks 'What is the process whereby HRM can have an impact on performance?' Building on these questions the next sections outline an admittedly idiosyncratic overview of certain priorities under three broad headings covering understanding of theory, management processes and research methods. There is no attempt at a comprehensive review; that can be found elsewhere (Guest *et al*., 2011). Rather, it is an attempt to highlight some key issues and some that risk being neglected.

SOME CHALLENGES FOR THEORY OF HRM

We might have thought that the debate about a universalist versus a contingency approach would have been settled long ago in favour of contingency theory. It has a strong logic, particularly in the context of strategic fit with the external environment. However, the empirical evidence appears to favour a universalist model (Combs *et al*., 2006). An initial challenge is therefore to provide insights that will help to resolve this apparent contradiction between logic and evidence. A related challenge linked to the contingency approach, concerns the alternatives to HRM. Is there just one high commitment form of HRM that is associated with high performance? What do the alternatives look like? Is it the 'control' model set out by Walton (1985), perhaps reflected in management of some call centres, or is it something else? We need to pose the contra question; if not a form of high commitment HRM, then what other ways of managing the workforce lead to high performance and other positive outcomes?

A further challenge we may need to address is the possibility that our theory is too narrow. What if any positive impact of HRM on performance is simply picking up good management? To address this, our research models need to contain appropriate controls, perhaps going beyond those that are conventionally used, and to consider more explicitly alternative explanations rather than focussing just on HRM and outcomes. In this context, the research of Birdi *et al*. (2008) is interesting in suggesting in a longitudinal study that HR practices have more impact on productivity than operational and lean management approaches. Advocates of the influence of leadership will tell us that it is good leadership that makes a difference; and leadership will have an impact on the content and practice of HRM as well as on management activities. Taking this a step further, we need to develop theory about how much independent influence we expect 'good' HRM to have and the conditions under which its impact will be larger or smaller. Allied to this, we need to shift our analysis of impact away from concern for statistical significance to a concern for effect size. The question then becomes – how much of the variance in outcomes do we expect to be explained by HRM? The review by Combs *et al*. (2006) found that the effect size was greater in manufacturing than in services, implying that one of the benefits of good HRM is that it can leverage other resources. We could speculate that in highly complex services such as large hospitals, its impact might be further diminished.

A topic that has been under-theorised in HRM research concerns the assumptions about the motives and commitment of the workforce. There can be a tendency to assume that workers have a propensity to display high commitment in response to the kind of policies implied by Walton's model and reflected in the contemporary advocacy of employee engagement (Albrecht, 2010; Rich *et al.*, 2010). However, this assumption needs testing. There has been much written in recent years about Generation X and Millenials and about changing workforce values (Smola and Sutton, 2002; Tulgan, 2000; Hewlett *et al.*, 2009). Within an HRM framework, this could be viewed as a selection issue, leading to selection on the basis of values, motivation and commitment as much as on competencies. Leaving aside the difficulties of doing this, it assumes a degree of stability. Yet work experiences and changing personal circumstances can alter priorities. Already we are beginning to see research on HRM for older workers (Kooij *et al.*, 2010), where the argument is that older workers have different priorities and respond more positively to HR practices that reflect these priorities. Similarly, at certain stages, workers may give greater priority to achieving an acceptable work-life balance. Research on HRM and performance has largely neglected theories of workers' values and motives and individual differences, despite some recognition of the need to build in 'micro' issues (Wright and Boswell, 2002). It is time to build these in to attempts to understand the processes that might link HRM and performance.

Finally, after considering a range of challenges for theory, we need to retain a focus on the basic and as yet unresolved question of what combination of practices are likely to have the greater impact on performance and other outcomes. If we opt for a universalist perspective, we probably need to offer clear hypotheses based on the AMO model or some variant. The alternative is to offer a form of 'high performance HRM' that is distinctively different from 'high commitment HRM' although, as noted earlier, it is not at all clear what this might look like. If we adopt a contingency model, then we need to consider under what conditions certain practices are likely to be more effective. And if we prefer a configurational approach, the question is what combinations of practices can be effective in given circumstances. It may be that these questions are ultimately unanswerable; but it is probably too early to give up on them.

SOME CHALLENGES IN UNDERSTANDING MANAGEMENT PROCESSES AFFECTING HRM

Researchers face a challenge in recognising that management behaviour may not be aligned with traditional methods of collecting data about HR practices. For example, advocates of a human capital perspective have argued that organisations should invest in those human resources that will provide a good return (Wright *et al.*, 1994). Lepak and Snell (1999) have presented a classification of human capital and a justification for high investment in core workers who are valuable, scarce, inimitable and non-substitutable. Following this logic, other categories of worker will require different levels of investment and therefore different human resource policies and practices. Many large organisations are likely to have a number of quite highly differentiated internal labour markets, each of which can have a distinctive set of HR policies and practices. In short, one size does not fit all creating problems for research that assumes that an organisation has an overall set of human resource practices. At one level, it may have such practices, including, for example, careful selection based on best methods and general use of goal setting as part of performance management. But the way these are applied to specific groups will differ in important ways. This suggests that management is not about the universal application of standard HR policies and practices, so collecting data at the level of the firm will provide limited information. An alternative, that has been quite widely debated, is to focus on an identifiable key category of staff within and across firms. But again, that will provide only limited information about firm practice in general and may therefore provide a misleading picture, especially if the core group is small in relation to the overall workforce, as might be the case in some banks.

A second major challenge for much of the research on HRM is that it collects data on the presence of practices whereas Wright and Nishii (2006) and Khilji and Wang (2006) among others have highlighted the need to distinguish between intended and implemented practices. Collecting information on the presence of practices, from whatever source, may be misleading. There is the risk that line managers simply fail to implement practices or may implement them badly. As noted earlier, Nishii *et al*. have shown how workers' attributions about the rationale behind practices affect their response to them. We can assume that the same process affects how managers choose to implement them. Indeed, there is evidence from research on the implementation of change initiatives showing how middle managers interpret, and therefore adapt for implementation, new policies and practices promulgated by senior management (Balogun and Johnson, 2005). Managers' priorities between market, financial and human resource issues will also affect the priority they give to HRM implementation. The importance of some kind of strategic integration, whereby line management values and priorities are integrated with those of the human resource strategy was identified long ago. But it has been largely neglected in research. It is line managers, not HR managers who implement HRM. This is beginning to be recognised (see, *e.g*. Bos-Nehles, 2010) and it implies a shift from studying the presence of HR practices to how well they are applied, and by implication, a shift in focus from HR managers to line managers.

Bowen and Ostroff (2004) have offered a theoretical framework within which to address the kinds of management behaviour that might determine the effective implementation of HRM. One of the factors they highlight is the consistency of messages about HR practices, both in terms of what is communicated and the sources of communication. By implication, the messages need to come from the top management team not from the HR department. We are beginning to see research exploring the consequences of top management support for HR implementation (Stanton *et al*., 2010) and there is scope to expand research on this process.

One of the problems arising from the limited data from longitudinal studies is that we do not know when good HRM was introduced or by whom. This points to the need for more studies of the genesis of new HR practices. An alternative is to explore firms that have had a marked change in performance and to identify the causes of this. Previous research on this topic (see, *e.g*. Grinyer *et al*., 1988) has generally highlighted the importance of leadership. There is evidence, at least for the UK, that HR professionals play little role in HR innovation (Guest and Bryson, 2009). This raises the question of who takes decisions about HR innovations. We need two additional kinds of research. The first is research that explores the genesis of new HR practices; greenfield sites might provide one kind of fruitful environment (Guest and Hoque, 1996). The second is the study of contexts where there is significant HR change taking place with respect either to the presence and content of certain practices or to the ways in which they are implemented. Some of this is likely to be case study research with room for mixed methods approaches.

CHALLENGES FOR RESEARCH METHODS

A frequently cited problem with most of the research on HRM and performance is that it is cross-sectional. This makes it difficult to be confident about cause and effect. The problem is exacerbated when we review the small number of longitudinal studies (see, *e.g*. Guest *et al*., 2003; Wright *et al*., 2005). They tend to show that HRM leads to performance, but this link ceases to be significant when we control for past performance. Invariably, past performance is much the strongest predictor of current performance and this can knock out any impact of HRM. Research at a rather different level of analysis on job satisfaction and performance, particularly the longitudinal study by Schneider *et al*. (2003) also challenges the standard assumption that satisfaction, perhaps resulting from good HRM, leads to higher performance and concludes that the opposite direction of causality is at least as powerful an explanation. This reinforces the need for better theorising about the likely size of the HRM impact and for research that tries to take into account the timing of the introduction and updating of relevant HR practices or other key changes. Allied to this, we need to give more attention to the largely neglected issue of the anticipated lag between the introduction, implementation and impact of HR practices. This is a well-recognised concern in longitudinal studies but has not been extensively discussed in relation to research on HRM and performance.

Other problems with the dominant research methods have been widely cited (see, *e.g*. Gerhart, 2007). There is a need for multiple sources of information about both the presence and the implementation of HR practices. As already noted, it is naive to assume that a senior HR manager can provide information about local practice either in terms of whether practices are implemented or whether they are effective. We also need to explore more carefully the effectiveness of practices. Goal setting may be present but it might be poorly formulated. Everyone may get an appraisal but it may be based on a poor system. The research evidence suggests generally low levels of agreement on the presence of practices when management accounts, usually from HR managers, are compared with workers accounts (for an exception, see Liao *et al*., 2009) with managers invariably reporting more practices in place than reported by workers. Since the intended impact is on workers, and they are closer to the point of implementation, there is a strong case for assuming that their views should be given more credibility. Perhaps the key message is that research needs to move beyond simply reporting the presence of practices.

Despite the presence of quite well-established models linking HRM to performance through the impact of HRM on workers' attitudes and behaviour, their link to internal performance such as productivity and quality and through this to external measures such as sales and profit per employee, very few studies have explored this chain. We would expect a stronger association between HRM and proximal rather than distal outcomes. Yet the meta analysis by Combs *et al*. (2006) indicated a stronger link to financial outcomes than to productivity. This may reflect a problem of measurement, bearing in mind that the measurement of productivity in the service sector can be particularly problematic (Atkinson, 2005), whereas it may be easier to obtain standard, comparable financial data. Overcoming the methodological requirements for testing these linkages presents considerable challenges, perhaps requiring the big research called for by Wall and Wood. But we can get closer within specific organisations such as banks and retail businesses where there are numerous branches.

In summary, the research is riddled with error both with respect to data on HRM and on outcomes. As some have argued, this may hide the size of any true effect (Gerhart *et al*., 2000). But is also leaves room for considerable doubt about the processes at play. We therefore need to recognise the need for more careful formulation of research and perhaps less research with a wide sweep. Indeed, we probably need to move away from the 'big research' concept.

There is a risk that research sophistication, and more particularly statistical sophistication can become an end in itself, driven in part by the publishing policies of some top journals. It can also lead to a focus on the use of established measures, even if their appropriateness for the research context is questionable. It would be unfortunate if this deterred some good researchers, including qualitative researchers, from entering the field since there are still some basic methodological questions that remain unresolved. For example, we remain uncertain about how to measure HR practices and HR implementation. We have made little progress in establishing ways to measure an HR system. Indeed, it is salutary that we still do not know which practices or combinations of HR practices have most impact nor when, why or for whom they matter. There is a risk of neglecting some core questions in favour of statistical rigour and abstracted empiricism.

In conclusion, after 20 years of HRMJ, we can reflect on impressive progress and a lot of ground clearing in theory development and research on HRM and performance. But there is a danger that we will let the weeds grow under our feet as we try to raise our sights to new challenges. Many of the basic questions remain the same and after hundreds of research studies we are still in no position to assert with any confidence that good HRM has an impact on organisation performance.

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